LEONHARDI EULERI OPERA OMNIA
SUB AUSPICIIS SOCIETATIS SCIENTIARUM NATURALIUM HELVETICAE
EDENDA CURAVERUNT
ANDREAS SPEISER · ERNST TROST · CHARLES BLANC

SERIES SECUNDA · OPERA MECHANICA ET ASTRONOMICA · VOLUMEN UNDECIMUM
SECTIO SECUNDA

C. TRUESDELL

THE RATIONAL MECHANICS

OF FLEXIBLE OR ELASTIC BODIES

1638—1788

INTRODUCTION TO

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TURICI MCMLX

VENDITIONI EXPONUNT
ORELL FÜSSLI TURICI

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HELVETICAE

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CONTENTS

PART II. THE BEGINNINGS OF THE GENERAL THEORIES, 1727—1748

PART III. THE CONTROVERSY OVER SMALL PLANE VIBRATIONS OF A STRING OF UNIFORM THICKNESS, 1746—1788

PART IV. RESEARCHES SUBSEQUENT TO EULER'S "FIRST PRINCIPLES OF MECHANICS", 1752—1788

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I am happy to record that the writing was begun in the Condé-Zimmer in the Engelhof on the Nadelberg, which belonged to the BERNOULLI family during the latter part of the period described.

FOREWORD

We search the *concepts* and *methods*[1]) of the theory of deformable solids from GALILEO to LAGRANGE. Neither of them achieved much in our subject, but their works serve as termini: With GALILEO's *Discorsi* in 1638 our matter begins[2]) (for this is the history of *mathematical theory*), while LAGRANGE's *Méchanique Analitique* closed the mechanics of

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1) There are three major historical works that bear on our subject. The first is *A history of the theory of elasticity and of the strength of materials* by I. TODHUNTER, "edited and completed" by K. PEARSON, Vol. I, Cambridge, 1886. Unfortunately it is necessary to give warning that this book fails to meet the standard set by the histories TODHUNTER lived to finish. Much of what TODHUNTER left seems to be rather the rough notes for a book than the book itself; the parts due to PEARSON are fortunately distinguished by square brackets. Researches prior to 1800 are disposed of in the first chapter, 79 pages long and almost entirely the work of PEARSON; as frontispiece to a work whose title restricts it to *theory* he saw fit to supply a possibly original pen drawing entitled "Rupture-Surfaces of Cast-Iron". While PEARSON took pains to describe a long list of worthless papers, many of them devoted to mere speculation or to experiment yielding no definite results, he omitted mentioning a number of major works by the BERNOULLIS and EULER, and in general he seems to have been unwilling to take the pains necessary to follow the more solid researches of the eighteenth century on rational mechanics. While I have studied PEARSON's chapter with care, in the end I have been able to make no use of it.

The second is the magnificent report of H. BURKHARDT, "*Entwicklungen nach oscillirenden Functionen und Integration der Differentialgleichungen der mathematischen Physik,*" Jahresber. deutsch. Math.-Ver. 10_2, 1800 pp. (1901—1908). Parts I, II, and IV concern vibrating bodies. It is difficult to express sufficient admiration for this work, which I have used again and again. To justify my including here a new history of the theory of vibrating bodies, presenting in some rare cases an interpretation differing from BURKHARDT's, I must explain that his emphasis lies on analysis; mine, on mechanics.

The third is TIMOSHENKO's *History of strength of materials with a brief account of the history of theory of elasticity and theory of structures*, New York, Toronto, London, McGraw-Hill, 1953. Although this work is drawn from a rather capricious selection of sources, it is drawn from them directly and with understanding. In the few cases where TIMOSHENKO's subject crosses mine, I acknowledge with gratitude the assistance his book has provided. Additional material is given by С. А. БЕРНШТЕЙН *Очерки по Истории Строительной Механики*. Moscow, 1957. These two books sketch also the history of statical theories of arches and frameworks, which are mentioned in the present essay only in cases where they influence or are influenced by theories of deformable media.

While E. HOPPE's *Geschichte der Physik*, Braunschweig, Vieweg, 1926, is an unusual historical work in that it concerns positive and specific achievements, evaluated by its author's own examination of the sources, unfortunately as far as concerns our subject HOPPE mentions but a small fraction of the relevant material and often draws unwarranted or even false conclusions from it.

2) The only earlier mathematical theory is BEECKMAN's, described in § 3; this brilliant work, while not without influence, remained unpublished for two centuries.

the Age of Reason with a formal treatise, since regarded universally, though most wrongly, as the definitive repository of the best from all that went before. As will appear, the giants of our subject are JAMES BERNOULLI and EULER. Here, for the first time, may be read the story of what these men really did for the theories of flexible or elastic bodies. Modern theories of materials are set chiefly upon the foundation laid down by CAUCHY from 1822 to 1845. Thus our account serves as preface to his researches.

The prolix speculations on the *causes* of elasticity, deriving from classical antiquity and developed in mechanistic terms by GALILEO, DESCARTES, HOOKE, NEWTON, and many other great philosophers and scientists before and after, often in accompaniment to the mathematical or experimental researches described here, are excluded from this history as being physical or philosophical rather than rational.

In an essay of this kind it is futile to attempt completeness, and hence I have not given the elaborate citations found in modern historical monographs. The footnotes serve rather to fill out the details and to illumine the strong personalities which must be recognized if not understood in any full view of the growth of mathematics. A connected account of the essentials may be gotten from the text alone.

To discuss the works in the order they appeared in print, when they were printed at all, would lead to perplexities which disappear of themselves when we follow the order of discovery, as here we do. But we must not forget that in many cases the results were known to succeeding investigators only after delay or not at all.

For the most part, the researches are reported in quotations or paraphrases from the originals. My own comments and interpretations I have tried to distinguish by square brackets[1]). With regret, I have realized that to reproduce the original notations would require an effort unlikely to be granted by the reader of a work of this kind. I am aware that in reducing all formulae to a uniform modern notation I am in a measure misquoting the sources and making everything seem too easy; now, once and for all, let the reader be reminded that as a result it is far easier for him today to see to the heart of one of these old researches than it was for those who first grappled with it and sought to do better.

1) *E. g.*, in a passage paraphrasing an original, from the words "by [HOOKE's] law" the reader is to infer that the author, without citing anyone, used the law now associated with HOOKE's name; from the words "by the [HOOKE-] LEIBNIZ law," that the author in using that law attributed it to LEIBNIZ.

PREFATORY NOTE

concerning the presumed technological origin
of the science of elasticity

In support of the currently received preconception that science arose from the needs of technology, or upon the basis of experience gained from practical solution of technological problems, I have found nothing as regards elasticity. Here, however, not being able to search for sources, perforce I have rested content with secondary material. Even works on the history of engineering present accounts suggesting more often the enthusiastic project of an early thinker than a contrivance actually built and used. In the earlier volumes of a recent encyclopaedia[1]) most references to elasticity and flexibility occur in peripheral remarks[2]) on the scientific theories and planned experiments we shall closely analyze in the following pages; far from answering to a call from technology, these researches had to wait decades or even centuries until engineers saw their relevance.

Of course, some elastic phenomena have long been known and utilized in daily life and technology, although in earliest times, as even today, the rigid body and the fluid are the primary elements of most mechanisms. As remarked by D. FORDE[3]), the wooden bow, "specially interesting as the first method of concentrating energy," is late among primitive weapons, not being demonstrably in use before 30,000 A. C. The age of the compound bow, arising in "response to the shortness of pieces of elastic material," is not known; it is represented on the column of Trajan (c. 110 A. D.)[4]).

Wooden springs were used in other machines in the middle ages. *E.g.*, VILLARD DE HONNECOURT (c. 1250) illustrates a water-powered saw so arranged that a limb bent downward in the driven stroke springs back to effect the return motion[5]).

Wood is a particularly unfortunate material on which to try to gain experience of elasticity. Use of horn and sinew for bows and catapults indicates familiarity with some more typical elastic materials in antiquity. While bronze fibulae are of great age, other employment of the elasticity of metals is late. According to A. P. USHER[6]), "there is no evidence that springs of either bronze or steel came into general use" in classical antiquity. He refers to the passage from PHILO of Byzantium that we shall quote below, p. 17, as being "the first clear indication of the possible significance of the elasticity of metals . . . Until this there is no record of the use of any form of metal spring except in [fibulae]. FELDHAUS gives no record of the use of leaf springs before the later sixteenth century, nor any record of spiral springs in locks or other devices before the fifteenth century." However, another authority[7]) states that metal crossbows are mentioned about A. D. 1370. Development of the spring as a driving mechanism for clocks, and solution of the practical problem of equalizing the force, took place in 1500–

1) *A history of technology*, ed. SINGER, HOLMYARD, HALL, and WILLIAMS, Oxford, 5 Vols., 1954–1958.

2) *E.g.*, A. P. USHER, "*Machines and mechanism* [1500–1750]," *op. cit. ante* **3**, 324–346 (1957).

3) Pp. 161–163 of "*Foraging, hunting, and fishing,*" *Op. cit. ante* **1**, 154–186 (1954).

4) In ch. 3 of *op. cit. infra*, p. 16, HERON of Alexandria refers to the *παλίντονα*, a catapult having a doubly curved bow, as to a thoroughly familiar object.

5) Pp. 643–644 of B. GILLE, "*Machines* [*to A.D. 1500*]," *op. cit. ante* **2**, 628–662 (1956).

6) P. 133 of *A history of mechanical inventions*, Revised ed., Harvard, 1954.

7) A. R. HALL, "*Military technology,*" *op. cit. ante* **2**, 695–730 (1956); see p. 723.

1550[1]). The invention of the balance spring, claimed by HUYGENS, HAUTEFEUILLE, and HOOKE, came long after scientific studies of elasticity had begun.

Among the various artillery pieces of the later Greeks which utilize the elasticity of some member, at least two employ the effective torsional elasticity obtained by turning a rod fixed perpendicularly within a tight bundle of cords or sinews. The idea which this device suggests, namely, that torsional elasticity may be explained by the extensional elasticity of the longitudinal fibres, seems not to have been taken up prior to EULER's day (see below, p. 341).

That structural members break, and sometimes deform markedly before breaking, must be an observation as old as the building of structures, but there is no evidence that builders' rules of thumb influenced the development of theories of materials, while application of even the crudest principles of statics to the practice of construction had to wait until long after mathematical statics had become an element of any solid scientific training.

While it would be unsafe to generalize, such information as I can find shows no ground for inferring any direct influence of technology upon the early theories of elastic and flexible bodies. Rather, it seems that the early theorists pondered over the phenomena of experience, usually simple daily experience apparent to anyone; thereafter came scientific experiment; and only much later, after the end of the period studied in this essay, was there interplay between science and technology. Thus the present history will not attempt to trace the technological side of the subject.

1) USHER, *op. cit.*, pp. 305–307.

PROLOGUE

1. Remarks of the ancients on vibration and elasticity. From before 1600 there is little—at least, little available to the working scientist—that survives of a concrete nature in our subject. Nearly everything specific concerning elasticity and vibration arose in the context of music. An account of early acoustics is given by F. V. HUNT[1]).

Traditionally associated with the school of PYTHAGORAS is the first law of the vibrating string:

$$\text{(1)} \qquad \textit{Numerical ratio of pitches} = \frac{1}{\textit{Ratio of lengths}}$$

for a given string at constant tension. "Numerical ratio of pitches" refers to the Pythagorean association of numbers to intervals, recognized by hearing: for the "octave," 2/1, for the "fifth", 3/2, *etc.*

That *sound is a vibratory motion of bodies* is an idea of early origin; gradually, from Greek times onward, it gained wider support, until by 1600 it was commonly accepted. The very idea of vibration would seem to carry with it the *isochronism* of the motion of a sounding body, but I have found no early explicit statement, although a connection between *musical pitch* and *frequency of vibration* was suggested by ARCHYTAS (c. 400 A. C.)[2]): "Clearly swift motion produces a high-pitched sound, slow motion a low-pitched sound," but the rest of the fragment indicates confusion of the acoustical effects of frequency and amplitude. Perhaps EUCLID (c. 350 A. C.)[3]) is only repeating the views of the school of ARCHYTAS and EUDOXUS when he writes, "Some sounds are higher pitched, being composed of more frequent and more numerous motions," but his explanation of why numerical ratios are attached to sounds is far from clear. It is stated emphatically, repeatedly, and very clearly by BOETHIUS[4]) (c. 480—524 A. D.), whose writing is considered to reflect much older views, that sound is a vibratory motion and that pitch increases with frequency, but he gives no definite relation. This idea was well known, though not generally accepted, in classical antiquity and subsequently. There was a gradual tendency to regard the loudness

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1) *Origins in acoustics*, forthcoming. I am indebted to Professor HUNT for some of the material in this section.

2) Fragm. 1, ed. DIELS, 8th ed. (1956), **1**, 435, ll. 1–2. Quoted by M. R. COHEN & I. E. DRABKIN, *A source book in Greek science*, New York, Toronto, and London, McGraw-Hill, 1948.

3) Introd. to *Sectio Canonis*, quoted by COHEN & DRABKIN, *op. cit.*

4) *De institutione musica* I. 3, quoted by COHEN & DRABKIN, *op. cit.*

of a sound as connected with the magnitude or violence of the displacement of the sounding parts and thus to separate the effect of amplitude from that of frequency.

That the pitch of a string increases with its tension is immediate from experience and could not fail to have been known to everyone[1]); likewise, that the thicker string has the lower tone, other things being equal, must have been known to every lyre player; but these simple remarks are not to be found in the early literature. Indeed, the reader of the fragmentary and inaccurate secondary accounts of Greek science surviving is led to conjecture that the pre-Socratic philosophers inferred some definite results which subsequent philosophic schools failed to understand or at least to appreciate, as when the muddy THEON OF SMYRNA (c. 125 A. D.)[2]) attributes to PYTHAGORAS an investigation of the ratios of pitches as dependent upon the *length*, *thickness*, and *tension* of the sounding strings, as well as a study of the sounds of disks and bowls. THEON refers several times to determining consonances by weights, magnitudes, and motions, but unfortunately all that he reports definitely is the [supposed] result that the pitches of two identical vessels partly full of water are proportional to the empty spaces.

Sympathetic vibration, in which a body is set a-trembling when an appropriate tone is sounded nearby, seems to have been well known to the ancients[3]).

[That a machine uniformly scaled from a small model does not generally perform in the same proportion must have been learned from many a sad experience of the builder.] The earliest *scaling laws* I have found are in the works of the Greek mechanicians, PHILON of Byzantium and HERON of Alexandria.[4]) The *Artillery*[5]) of PHILON gives many rules, clearly of empirical origin, for constructing catapults of the same type but sufficient to cast missiles of various weights. HERON'S *Artillery*[6]) states, "It is necessary to know that the determination of the measurements has been gotten from experience itself. For the an-

1) *Cf.* VITRUVIUS, *De architectura* 10. 12. 2. English transl. by M. H. MORGAN, Cambridge, Harvard, 1926. Quoted also by COHEN & DRABKIN, *op. cit. Cf.* also BOETHIUS, *loc. cit.*

2) 2. 12—13. Quoted by COHEN & DRABKIN, *op. cit.*

3) VITRUVIUS, *De architectura* 5. 5, reports the practice of the Greek builders to set about their theatres, so as to magnify the sound of the actors' voices, large vessels tuned to appropriate pitches.

4) The dates of these authors remain uncertain: PHILON, A. C. 180 to A. D. 1; HERON, A. C. 250 to A. D. 75. Modern scholars incline toward the later dates. The matter is further complicated by uncertainty that the same HERON is the author of both the *Artillery* and the *Mechanics*.

5) "PHILONS *Belopoiika* (*Viertes Buch der Mechanik*)," Greek text and German translation ed. H. DIELS & E. SCHRAMM, *Abh. k. Preuss. Akad. Wiss. 1918*, No. 16, 68 pp. (1919). Chs. 3 and 16 seem to imply knowledge that uniform proportion does not suffice. While Ch. 13 describes a method of effecting uniform scaling, we need not infer any mechanical rule; PHILON may intend this passage only as an aid to construction after the proportions have been determined.

6) "HERONS *Belopoiika* (*Schrift vom Geschützbau*)," Greek text and German translation ed. H. DIELS & E. SCHRAMM, *Abh. k. Preuss. Akad. Wiss. 1918*, No. 2, 56 pp. See Chs. 31–33. There is a strong likeness between HERON'S Ch. 31 and PHILON'S Ch. 3.

cients, paying attention only to the scheme and the construction, reached no great range with their artillery, since they did not select harmonic proportions. But the moderns, reducing some parts and enlarging others, made the above described machines consonant and efficient." While HERON states that "the rule and principle is the bowstring," apparently he refers only to its size, for he writes also, "Let the diameter of the machine [*i.e.* the calibre of the piece] be AB, and let it be required to build a like machine which will cast a shot, *e.g.*, triple that of the above-stated. Since the bowstring gives rise to the cast of the stone, the machine to be built must have a bowstring three times as great . . ." However, HERON warns that not any diameter will do, and he gives and illustrates an explicit rule for determining the sizes of the remaining members of the machine. [I am not fully able to understand the rule; moreover, since it involves "harmonic proportion", it is scarcely likely to be "gotten from experience" as HERON claims. But what is most important is that a *definite scaling law* for like performance is given.]

The earliest known descriptions of *elasticity*, and in particular of the elasticity of
metals, are found in PHILON'S work. He advises that the bowcord be stretched so tight Ch. 27
"that when the machine is drawn, the diameter is lessened by a third part." He mentions 18
the fatigue of the cord as a result of use and advises against the common practice of trying
to regain the tension by twisting the cord until it is tight again. He recommends "tight- 27
ening all the strings of the bowcord at once, in their natural straight position," so as to
avoid weakening them by twisting. He claims the invention of bronze leaf springs and 43—44
describes their fabrication. His innovation appears to have aroused some doubts: ". . . many 46
persons . . . say that it is impossible that curved bands [*i.e.* springs] when straightened out by the force of the bow will not remain straight thereafter but will instead regain their original curvature. While indeed by its nature horn has this property, and some kinds of woods (and bows are made of such), bronze on the contrary is hard and stiff in its nature, as is iron, so that when bent . . . it cannot straighten itself out. Let these persons be forgiven for holding such an opinion without trying the details. For the production of the aforementioned bands is seen by the agency of the swords called Spanish or Celtic." After severe bending, they spring back straight, "having no thought of curvature. Also when
[the test] is done many times, they remain straight." [That elasticity was unfamiliar, at 47
least as a subject of science,] is shown by the immediately following inquiry into its cause; while it is attributed to a choice of especially pure metal, neither too hard nor too soft, followed by gentle cold working, no special name is given to it. [1])

1) The word κατευτονεῖν, "to be extraordinarily well behaved," is translated by DIELS & SCHRAMM as "elastisch sind"; τὸ τὴν εὐτονίαν ποιοῦν as "was ihnen Spannkraft gäbe." In Ch. 44 they translate the old word νευρώδης, "sinewy", as "elastisch."

In the course of a long, dull work on statics[1]), HERON interposes a list of physical questions and answers, three of which concern elasticity and rupture. "When [a bow] is bent strongly, the bowstring with the arrow is more taut . . .," but HERON does not give an elastic law. In explaining why a stick is more easily broken against the knee, he suggests that each portion acts as a lever, but he seems to believe the effect arises only because the knee is not quite in the middle, so that one hand "outweighs" the other.[2]) "Why is a piece of wood as much weaker as it is longer, and why does its bending increase when it is set upright upon one of its ends?" HERON explains, "the whole overbalances the fastened part . . . Hence the same effect takes place as in a short stock when something hung upon its ends bends it down. The increase of length of the stock corresponds to the weight that draws the short stock down." [This is the first reference to the *buckling* of a heavy vertical column, and HERON gives part of a correct explanation.

2. Western researches before 1600. DUHEM's great historical studies showed that the apparent darkness of mediaeval physics is but darkness of our knowledge of it. How great a proportion of mediaeval work survives, and how much of that is now available, I do not know. The only writing of value on deformable bodies that I have been able to see] is the fourth book of JORDAN DE NEMORE's *Theory of Weight*[3]) (13th Century), and remarkable it is, Western in spirit, ambitious beyond anything in the Greek or Arab tradition[4]). The seventeen propositions on fluid flow, resistance, fracture, and elasticity are all original. While the style is mathematical, it would be unfair to expect what JORDAN brings forward as "proofs" to be more than plausible reasoning alleged in favor of assertions drawn from experience and conjecture by a scientist well trained in the ancient mathematical statics. Only two of the propositions concern our present subject.

In Prop. 12 we are told that the coherence of a beam hung up by its two ends may or may not suffice to keep it from breaking in the middle. The beam, whether supported in this way or at one end only, is to be regarded as a lever. Greater bending is produced by a body striking the beam than by the same body resting upon it. [This is the earliest distinction between *static and dynamic loading* in respect to deformation.]

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1) *Mechanics* II 34f—h, in Arabic, ed. with German translation by L. NIX & W. SCHMIDT, Leipzig, 1900.

2) Chs. 21 and 41 of PHILON, *op. cit. ante*, p. 16, likewise attempt to apply the law of the lever to the action of the bow, but I cannot understand what is meant.

3) *De ratione ponderis*, first printed at Venice in 1565 from a manuscript belonging to TARTAGLIA. Ed. and transl. into English by E. A. MOODY, pp. 167—227 of *The medieval science of weights*, Madison, 1952.

4) An account of Arab views on acoustics is given by HUNT, *op. cit.*, but the only thing concrete I have found there beyond what is known from Greek sources is that SAFI AL-DIN (d. 1294) wrote, at last, that the thinner string has a higher pitch.

Prop. 13 reads, "When the middle is held back, the ends are more easily curved." The "body" is taken as a line fixed at its midpoint; the ends are supposed to receive an impulse. "... since the ends yield more easily, while the other parts follow more easily insofar as they lie closer to them, it turns out that the whole body is curved into a circle." [This is the earliest statement of the problem of the *elastic curve* or *elastica*. JORDAN asserts, in modern terms, that a band clamped at one end and struck by a weight falling upon the other assumes the form of a *circular arc*.

The reasoning is vague, qualitative, and insufficient if not erroneous, but the attempt at a precise argument to prove a concrete result in a domain never previously entered is of splendid daring. This work of the thirteenth century is better than many to be published by learned academies in the seventeenth and even the early eighteenth.]

LEONARDO DA VINCI (1452—1519) seems to have been the first to use a light rider to make visible a very faint tremor, and specifically in the case of sympathetic vibration[1]): "The blow given in the bell makes another, like bell answer and move a little, and the sounded string of a lute makes another, like string of like voice [*i. e.* pitch] in another lute answer and move a little, and this you will perceive by placing a straw upon the string like to the one sounded."

Moreover, from LEONARDO we have the earliest known project of tests of wires and beams for their breaking strength[2]). In Figure 1, sand is poured from the hopper into the basket until the wire breaks; thereafter, the sand is to be weighed[3]). "Note how much weight broke the wire, and note in what part of itself the wire breaks, and do this trial several times so as to see if it always breaks in the same place." LEONARDO does not state that he has ever performed this test, and he expects that

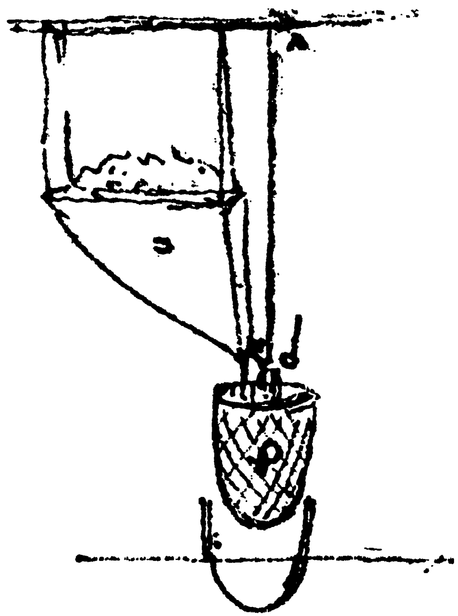

Figure 1. LEONARDO DA VINCI's projected tester for the breaking of wires

1) MS Inst. France A, f 22v. *Cf.* also Codice Atlantico, f 242 v. a): "... the campanile shakes at the sound of its bells."

The reader must be warned that the various translations from LEONARDO's works are so inaccurate as to be of scarcely any use in connection with science or engineering.

2) I have found helpful the account of W. B. PARSONS in Ch. VI of his *Engineers and Engineering in the Renaissance*, Baltimore, Williams & Wilkins, 1939, but I cannot participate in PARSONS' enthusiastic extrapolations beyond what LEONARDO wrote, nor do I consider his translations always just.

3) Codice Atlantico, f 82rb). This is a very clear page.

the breaking strength will vary appreciably with the length of the wire[1]), [a common error, which MERSENNE and GALILEO are later to refute (below, pp. 31, 37)].

LEONARDO wrote what is almost a small treatise on the strength of pillars, beams, cords, and arches[2]), remarkable in that it gives definite rules (right or wrong) rather than mere qualities or tendencies. This treatise is perplexing, for while LEONARDO often speaks of experiments, it is always in the future tense, and he gives no indication that he has ever carried out any measurement. His rules, while showing that he was an acute observer of experience, seem to arise from a kind of plausible rhetoric in a background of deep attachment to simple proportion[3]).

LEONARDO begins[4]) with drawings of vertical pillars supporting a load. "If you load a pillar erected vertically in such a way that the center of the pillar is beneath the center of the weight, it will compress rather than bend . . ." [While the reason given is merely one of symmetry, we find here the first allusion since HERON's day to a problem whose solution is to be one of EULER's most brilliant successes.] LEONARDO gives two rules[5]) for the strength of pillars bearing a load P:

$$P \propto \frac{\sqrt{A}}{l} \quad \text{and} \quad P \propto d^3\,, \qquad \begin{aligned} A &= \text{cross-sectional area,} \\ d &= \text{diameter,} \\ l &= \text{length.} \end{aligned} \tag{2}$$

[These are not consistent with one another; in LEONARDO's crabbed writing there are few definitions, and it is often not clear what is held constant. If we regard the second rule as a correction for the first when $l = \text{const.}$, then it *may* follow that LEONARDO's final rule is

$$P \propto \frac{d^3}{l}\,, \tag{3}$$

but this is far from certain.]

LEONARDO considers other kinds of support and load (*cf.*, *e. g.*, Figure 2). For a horizontal beam clamped into a wall at one end and loaded at the other, he seems to claim the same law of strength[6]). He proposes the problem of determining the *deflection*

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1) *Cf.* Inst. France MS A, f 49r, where LEONARDO states that the strength of a cord is proportional to its length.

2) Inst. France MS A, ff 45—55.

3) On f 45v he writes, "This is proved by reason and confirmed by experiment," but the further text supports only the former assertion, not the latter.

4) f 45v.

5) Inst. France MS A, ff 46r, 47r. PARSONS, who misquotes the second rule, states that the first is $P \propto B/l$, where $B =$ breadth; this is a correct rule, but it is not borne out by LEONARDO's arguments or numerical specimens. While LEONARDO elsewhere shows his familiarity with the concept of static moment, I fail to verify PARSONS' claim that it is applied here.

6) Inst. France MS A, f 49r.

of a beam supported at both ends and loaded by a weight at its middle[1]). He discusses also the forces exerted by a heavy beam on two supports placed variously along its length[2]).

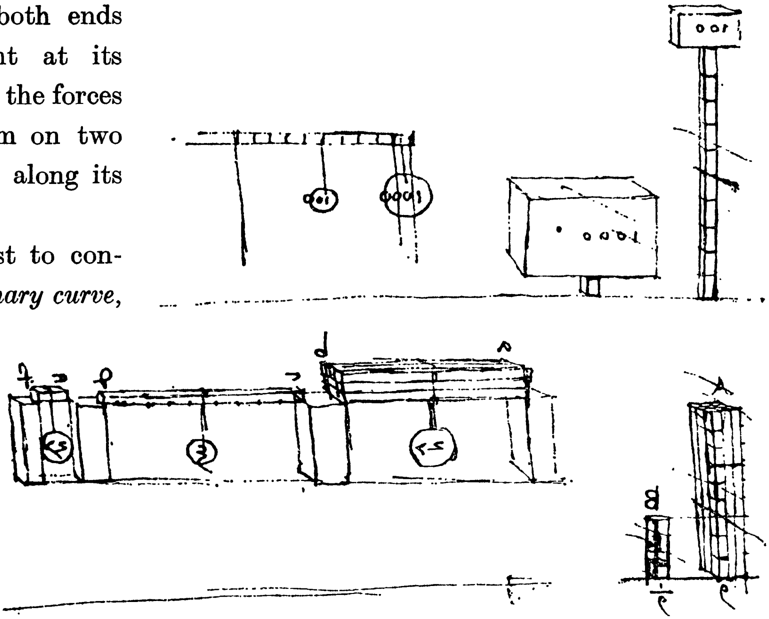

Figure 2. A project of LEONARDO DA VINCI for measuring the strength of beams

LEONARDO is the first to consider the form of the *catenary curve*, the figure assumed by a cord or fine chain hung between two points[3]) (Figure 3). "The lowest point of the arch made by a string which is longer than the space between the supports holding up the ends at two different heights, will touch the earth nearer to the lower support than to the higher, and in the proportion that the height of the lesser goes into the greater." [That is, the lowest point of the catenary is the point of intersection of straight lines dropped from the supports so as to make equal angles with the vertical. Except in the trivial case when the supports are at equal heights, LEONARDO's assertion is false, but there is a germ of truth in it.] In the second drawing in Figure 3, LEONARDO concentrates all the weight of the string in the middle [and thus introduces the first *discrete model for a continuous system*]. For this case, his assertion is true and determines the figure of equilibrium completely. In another attempt[4]), he seems to regard the weight of the string as equilibrated by weights

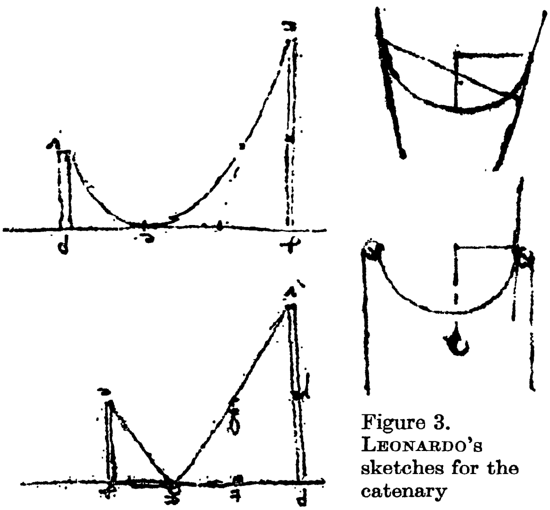

Figure 3. LEONARDO's sketches for the catenary

1) *Ibid.* f 48r.
2) Codice Atlantico f 185 r a. This passage is fragmentary and vague.
3) MS Inst. France A, f 48r. *Cf.* also f 51v.
4) MS. Inst. France E f 60v. The text makes no reference to the weights, and the drawing is not

hung over pulleys at the ends and infers that "A cord of whatever size or strength . . . can never become straight if it has any weight in the middle of its length," [anticipating a famous proposition of GALILEO (below, p. 44)]. Moreover, LEONARDO's *drawing* of the catenary appears to be copied from fact.

The nature of resonance was first correctly explained by JEROME FRACASTORO[1]) in 1546. "One unison promotes another, since when two strings are equally taut, they are fitted to make and receive like undulations of the air. Those that are diversely taut are not in case to be moved by the same circulations, but one circulation hinders another. The beat of the string, the motion, is composed of two motions, by one of which the string is driven forward, that is, toward the circulations of the air; by the other, backward, the string thus restoring itself to its proper location. Therefore, if one moved string is to be moved by another, in the second there must be such a proportion that the undulations and circulations of the air which impel and make the forward motion do not hinder the backward motion of the string. Such a proportion is had only by those strings that have a like tension. On the contrary, strings of random tension do not set each other in motion, because when the second motion happens, that is, the return of the string backward, the second string hinders it, and they get in each other's way. Whence there occurs no motion except the first impulsion, which is insensible. I myself have seen in a certain church where many wax statues stood high up around a chapel, at a certain tinkling only one of the statues moved... The cause was nothing else than the fact that only one was in unison." FRACASTORO then draws an analogy to lifting a weight by rhythmic action. "The same thing happens also to those who beat bread, when two or three men alternately lift up and press down a long heavy beam, for if indeed they do not act together, all lifting and then all pressing down, but when one lifts another begins to press, the motion is hindered . . . In strings, however, it is not perceived because of the speed of the circulations."

[Thus FRACASTORO discerns the reciprocal or vibrating motion of musical strings and of sound in air, observes that not only strings but also other bodies are "fitted" to take on motion at a definite natural frequency, and asserts that sympathetic vibration occurs when the source communicates a motion that reinforces the natural motion of the receiver.

The passage just quoted implies a knowledge of sound more precise than anything preserved from classical antiquity. In particular, FRACASTORO clearly takes it for granted that *sound is a vibratory motion of a definite frequency*. His book, however, does not read

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clear. LEONARDO's mastery of statics is exaggerated by his enthusiasts. *E.g.*, the rule stated in Codex Forster II f 67v for finding the tensions in the two cords of the discrete model is false if taken quantitatively, as seems to be LEONARDO's meaning, and equivocal if taken only qualitatively.

1) Ch. 11 of *De sympathia et antipathia rerum liber unus . . .*, Venice, 1546, [viii pp.] + 76 leaves + [vi pp.].

like a work of an originator but seems rather to be a miscellaneous collection, though thoughtfully presented. I am led to conjecture that future studies of mediaeval sources will reveal a considerable knowledge of acoustics that had become common domain by the sixteenth century.

This is borne out to some extent] by the work of JOHN BAPTIST BENEDETTI, *On musical intervals*, published in 1585[1]). At the very end he writes, "Let a monochord be imagined . . .; when it is divided into two equal parts by the bridge, each part will make the same sound . . ., because the one makes as many strikings in the air as does the other, so that the waves of air go out in the same way and agree equally, without any intersection or breaking of each other.

"If the bridge divides the string in thirds, so that one part is twice as long as the other . . ., then the greater part . . . will sound an octave below, for the strikings of its ends will bear such a proportion to each other that in every second striking of the lesser string, the greater will strike and agree with the lesser at the same instant, since there is no one ignorant that by so much the longer is a string, by so much the slower it moves. Wherefore, since the longer is twice the shorter, and both are equally taut, in the same time that that longer completes one interval of trembling, the shorter will complete two intervals." After illustrating the idea by a fifth and by other musical intervals, BENEDETTI concludes that "the number of intervals [of trembling] of the lesser portion will stand in the same ratio to the number of intervals of the greater as does the length of the greater to the length of the lesser . . ."

[Thus BENEDETTI regards the number of "intervals of trembling", or, as we say now, the *frequency* of the vibration, as a measure of pitch. To speak of such "intervals" as associated with a sound presumes that

(4) *Sonorous vibrations are isochrone.*

BENEDETTI goes further; since "no one is ignorant" that the speed of a string is inversely proportional to its length, other things being equal, it follows that

(5) *Pythagorean ratio of pitches = ratio of frequencies.*

These fundamental tenets of the theory of vibration are soon to be rediscovered by BEECKMAN (1614—1615), MERSENNE (1623), and GALILEO (by 1636).]

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1) "*De intervallis musicis,*" pp. 277—283 of *Diversarum speculationum mathematicarum et physicarum liber*, Taurini, Haered. Nic. Bevilaquae, 1585; 2nd ed., date unknown; 3rd. ed., Venetiis, Baretium, 1599. Reprint of "*De intervallis musicis,*" ed. J. REISS, Z. Musikwissenschaft 7 (1924/5), 13—20.

3. Beeckman on the suspension bridge (1614—1615), on vibration (1614—1618), and on elasticity (1620—1630). Stevin[1]), in a work published in 1608, considered a weightless string loaded at various points by an arbitrary number of different weights, but he contented himself with finding the tensions when the figure is given, and with testing the result experimentally. In annotating this work of Stevin in 1634, Albert Girard[2]) claimed that he had proved in 1617 that the continuous string hangs in a parabola. Meanwhile, however, the problem had been taken up by the gifted but overly modest Isaac Beeckman (1588—1637)[3]), who considered it in notes dating from 1614—1615[4]). In 1618 Descartes[5]) writes that Beeckman "asked me if the rope *acb* hung up on pins *a*, *b* would describe a part of a conic section. I have no time to look into this now." Beeckman, however, in a note[6]) from this period or earlier, had set up the problem of the weightless string loaded by equal weights which seem to be equally distant along the horizontal and had given part of a geometrical proof that *the points where the weights are attached lie on a parabola.* If this interpretation of his note is correct, Beeckman was considering the problem of the *suspension bridge* and had conjectured, if not proved, its correct solution.

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1) Coroll. 6, Part I ("Spartostatics"), "*Byvough der Weeghconst,*" part iv, 7 of *Wisconstighe Ghedachtenissen* . . ., Leyden, 1605—1608. Latin transl., *Hypomnemata mathematica* . . ., Lugduni Batavorum, 1608. Dutch text and English translation of part iv, 7 = *Princ. Works* **1**, 523—607.

2) "But one must know that Stevin . . . has seen that . . . loose or very extended strings are parabolic lines (as I proved in about the year 1617), and this I will prove below, after the next corollary . . ." There is no published writing of Stevin that substantiates this statement, and when Girard later on the same page finishes with "the next corollary," he adds only, "to discharge my promise, since I do not have the time to copy out my whole proof, I will give it to the public on some other occasion, by the help of God, when scientific research is more profitable than at present." See p. 508 of *Les Œuvres Mathématiques de Simon Stevin, . . . le tout reveu corrigé, et augmenté par Albert Girard,* Leyden, Bonaventure & Elsevier, 1634.

3) *Journal tenu par Isaac Beeckman de 1604 à 1634,* ed. C. de Waard, La Haye, Nijhoff, 4 vols., 1939—1953.

The posthumous publication of a small part of this diary in 1644 does not indicate the extent of Beeckman's influence. It was Beeckman who in 1618 initiated the young Descartes into physics and encouraged him to apply his talents to the sciences. Each told the other in 1618 that he had never theretofore met anyone who "joins physics precisely with mathematics" (*Journal,* f 100v.). (This ambition notwithstanding, most of the contents of Beeckman's *Journal,* including all the numerous passages concerning elasticity and resistance, are philosophico-physical and devoid of mathematical reasoning.) Descartes surely saw Beeckman's journal in 1618 and probably also in 1628. Beeckman met Mersenne and Gassend in 1629; in 1630 Mersenne spent whole days studying Beeckman's notes, the contents of some of which he published. Beeckman corresponded both with Descartes and with Mersenne by letter.

4) *Journal,* f 20v.

5) Oeuvres **10**, 219—228. This note was first published in 1859.

6) *Journal* **1**, Appendix 1. The drawings, unfortunately ill copied, suggest the influence of the published work of Stevin. In 1613 Beeckman had had access to unpublished papers left by Stevin.

The fundamental acoustic principles (4) and (5), while implied by a passage published three times in the sixteenth century, apparently were rediscovered independently[1]) by several savants of the next. [They must have seemed natural ideas to any inquiring mind prepared to view the doctrines of the ancients in the light of the rising mechanism of the baroque, and we should not be surprized if they were discovered or shared by others besides those we name.] In 1618 DESCARTES[2]) writes, "BEECKMAN thinks that the strings of a lute move faster in proportion to their pitch, so that the one higher by an octave gives out two motions while the lower gives one; likewise, one higher by a fifth gives $1\frac{1}{2}$, *etc.*" Every one of the many passages in BEECKMAN's journal concerning vibration reflects the basic principles (4) and (5), though he nowhere expresses himself so clearly as does DESCARTES. In 1614—1615 he writes[3]) that "a sound . . . is composed of as many sounds as there are returns of strings to their place . . . I suppose the nature of the human voice, of whistles, of the lute, and of any musical instrument to be the same as the nature of a string, since experience confirms that all voices can be consonant with strings. Therefore whatever we shall prove in this matter concerning strings, we postulate could be proved also for the remaining kinds of voices." He attempts to prove that the frequency of half a string subject to equal tension is twice as great. More generally,

(6) $$\nu \propto \frac{1}{l}, \qquad \nu = \text{frequency}, \quad l = \text{length},$$

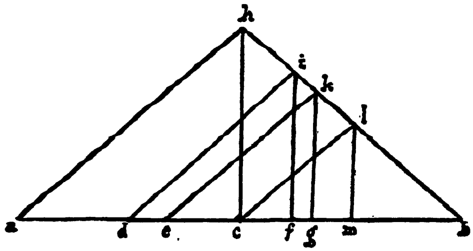

Figure 4.
BEECKMAN's drawing to prove (6) (1614–1615)

[for this is an immediate corollary of (1) and (5), or, conversely, if (1) is taken as a fact of experience and if (6) may be proved from mechanical laws, then (5) follows.] Consider two strings ahb and clb plucked into similar triangular forms as in Figure 4. Since the strings are of the same material and subject to the same tension, the restoring force at h on ahb is the same as that at l on clb and thus will induce the same velocity in each string. When the strings are released, the point h must travel twice as far as the point l, but at the same velocity, and hence it will require

1) As is shown below, BEECKMAN explained his ideas freely to DESCARTES, who apparently adopted (4) and (5) at once but certainly deserves no credit for them. While it is thus possible that DESCARTES imparted (4) and (5) to MERSENNE, there is no positive evidence that DESCARTES and MERSENNE were even acquainted before MERSENNE published these principles in 1623. The personal correspondence of MERSENNE and BEECKMAN began in 1629. There seems to be no reason for doubting the independence of GALILEO, who had cause to delay publication; his work is described in § 5 below.

2) *Op. cit. ante*, p. 24. This passage shows that (5) was not common knowledge in 1618.

3) *Journal* ff 23v—24r.

twice as much time to reach the straight form. A parallel argument applies to two strings whose lengths are in any given ratio. [Upon reflection, we perceive that this reasoning is sound in principle! It applies, strictly, only to the first instant, when a finite velocity or impulse is produced at the corner; to apply it at later instants one must know something about the motion. BEECKMAN does not say anything about this, but from other passages one suspects that he considers the form to remain triangular, which is false. To determine the motion of a string plucked initially into triangular form requires dynamical principles not known in BEECKMAN's day; it soon became and remained a major problem, until it was solved finally by EULER 150 years later. BEECKMAN's achievement is great: By furnishing the *first mathematical proof* of any acoustical proposition, he stands father to the theory of vibration.]

In 1618[1]) BEECKMAN gives a convincing physical argument in support of (4). "Since the string comes to rest at last, we must believe that the space through which it moves at the second stroke is shorter than that at the first stroke; and thus the spaces of the strokes diminish. But, since to the ears all sounds seem the same up to the end, it is necessary that all the strokes are always distant from one another by an equal interval of time, and therefore the following motions move more slowly . . ., since the string crosses a little space in the same time it formerly used to cross a greater one." Then[2]) he compares the vibration of a string to the motion of "chandeliers hung from a rope," which he says is isochrone in a vacuum. He seems to have done experiments on this, and he gives a sort of theory.

After remarking that only properly tuned strings are resonant, and that a string may set into resonance another tuned an octave higher[3]), BEECKMAN gives a correct physical

1) *Journal* f 102r. *Cf.* also ff 105r (1618), 367r (1630—1631), and the repetition of this argument by MERSENNE, quoted below, p. 31.

2) *Journal* f 105v.

3) *Journal* f 54v (1616—1618).

Here we take note of some passages in FRANCIS BACON's *Sylva Sylvarum, or a Naturall Historie*, London, 1627, republished in the various collected editions of his works. § 279 describes as "a common observation" resonance of a string tuned to like pitch or an octave higher, made visible by a superincumbent straw; BACON uses words almost the same as LEONARDO's (above, p. 19). He discusses the tones of strings as follows: "So we see in strings: the more they are wound up and strained, (and thereby give a more quick start-back) the more treble is the sound; and the slacker they are, or less wound up, the baser is the sound. And therefore a bigger string more strained, and a lesser string less strained, may fall into the same tone" (§ 179). BACON says that shortening a string raises its pitch, since it causes the string "to give a quicker start" (§ 181). He proposes an experiment on the effect of tautness by recording the pitches corresponding to 1, 2, 3, . . . turns of the peg, so as to discover "both the proportion of the sound towards the dimension of the winding; and the proportion likewise of the sound towards the string, as it is more or less strained." Far from anticipating the work of MERSENNE, BACON seems to know less than the ancients regarding the tones of a monochord and a pipe. He explanation[1]): "If . . . the other string, however it is struck, always moves equally to the first, and both end their motions at the same time (which is the nature of unison), this happens if the air impinging upon the quiet string moves it, even invisibly. But when the air strikes this string a second time . . ., something is added to the [same] motion. Thus again for the third and fourth time, and thus finally the motion becomes visible."

Also[2]), " . . . when a bell is sounding, its . . . parts tremble so that the parts in the midst of it push quickly inward and outward again and again . . . Today I saw an experiment of this. There was a glass half full of water or wine and a wet finger pressing the edge was drawn around it. While this happened, a sound was heard coming out of the glass, and at the same time the water near the edge jumped and cast up little drops . . . The water seemed to boil around the sides but to lie quiet in the middle, and the boiling was drawn around, following the motion of the finger." In 1618 BEECKMAN writes[3]) that DESCARTES showed him that the low strings of a lute can excite the higher ones, but not *vice versa;* also, that a sounding string will excite another tuned up a fifth, but not one tuned a fourth higher. BEECKMAN then gives his former explanation more clearly: While the second string tuned an octave and a fifth higher makes three vibrations, the first string makes one, so that the vibrations "agree alternately". In 1635 MERSENNE[4]) published this passage almost word for word, attributing its content to BEECKMAN.

In considering the bending of a beam, BEECKMAN in 1620 recognizes that the parts on the convex side are extended, while those on the concave side are contracted, but he does not attempt to formulate a theory[5]).

In 1630 BEECKMAN[6]) informs MERSENNE that when a weight is attached to a string, "the longer is the string, the more the weight descends . . ." That is,

$$\varepsilon \equiv \frac{\Delta l}{l} = \text{const.} \quad \text{when} \quad F = \text{const.} \tag{7}$$

presents 189 "experiments" or pronouncements on sound and music; while not the only early writer who prefers projecting experiments to performing them, he shows talent for missing essentials while reporting trivia, and his book exemplifies the vacancy of experiment and speculation undisciplined by mathematics.

1) *Journal* f 67r (1616—1618).

2) *Journal* ff 86v—87r (1618).

3) *Journal* ff 100r—101v. *Cf.* also f 105r. On f 128r (1619) is an unsatisfactory discussion which seems to indicate that BEECKMAN may have the idea that the same body may resonate at different frequencies.

4) *Harmon. Libri 12* (cited below, p. 29), Lib. IV, Prop. 29. *Cf.* also Prop. 29.

5) *Journal* ff 137 bis v, 139 bis v.

6) *Journal* f 362r. DESCARTES writes to MERSENNE in January 1630 that a string stretched slowly will break in the middle; quickly, at the ends. This seems to be first recorded observation since HERON's day that the static and dynamic strengths of a body may differ.

[Thus BEECKMAN perceives that it is *strain* ε, rather than merely elongation Δl, that measures the effect of a force in stretching a string of given material and cross-section.]

4. MERSENNE on vibration and rupture (1623-1636). In 1623 MERSENNE, before he met BEECKMAN or saw his work, published[1]) (4), (5), and (6). Moreover, MERSENNE writes that a bell can give out three tones at once: its proper sound, the octave, and the twelfth, and possibly also two more. He thinks he distinguishes the same phenomenon in organ pipes and other instruments. [That is, *a vibrating body may emit several definite tones simultaneously*[2]).] That different methods of blowing cause a pipe to emit a sequence of different tones had long been known from musical experience, and it seems that MERSENNE connects these phenomena and proposes the problem of determining *the sequence of overtones of a vibrating body*[3]), *e. g.*, a string.

In 1625 MERSENNE[4]) published rules of proportion equivalent to the law

(8) $$\nu \propto \frac{1}{l}\sqrt{\frac{T}{A}}\,, \qquad \begin{array}{l} T = \text{tension or stretching weight,} \\ A = \text{cross-sectional area or ``thickness''}, \end{array}$$

which he had inferred from experiment. [This beautiful discovery of MERSENNE, generalizing (6), may fairly be recognized as the first concrete result in the science of vibratory motion.] The circumstances of finding it are not known[5]).

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1) Cols. 1559—1561 of *Quaestiones celeberrimae in Genesim*, Paris, 1623. I have never been able to see this work; for the specific attribution, I am content to cite DE WAARD, Note 1 on p. 161 of BEECKMAN's *Journal* **3**.

In an entry dated 12 August 1630 (*Journal* f 362r) BEECKMAN writes that MERSENNE asked him the reason why (6) holds, and BEECKMAN replied along the lines he had written in 1614—1615 (above, pp. 25—26).

2) According to MATTHEW YOUNG, *op. cit. infra.* p. 294, there is a letter of 1618 from DESCARTES to MERSENNE (cited by YOUNG as "Ep. P. 2 Ep. 106") referring to "the different tones which are produced at the same instant by the same string," but no such letter is printed in *Correspondence du P. Marin Mersenne*, ed. DE WAARD, **1** (1617—1627), 1932; **2** (1628—1630), 1936; **3** (1631—1633), 1946; **4** (1634), 1955.

3) MERSENNE is a rather vague writer, and besides this it is necessary to infer the question from the replies sent him by various correspondents from 1625 onward, since MERSENNE's relevant letters are lost. A feeble explanation is given by DESCARTES about 1626 (*Corresp. de Mersenne*, No. 51): ". . . all the higher sounds are present in the lower ones, just as the shortest strings are in the longest," *etc.*, and "sound is easier to divide in two parts," *etc.*

4) P. 616 of *Vérité des Sciences*, Paris, 1625. I have never been able to see this work; for the specific attribution, I am content to cite DE WAARD, Note 2 on p. 98 of BEECKMAN's *Journal* **3**.

DESCARTES communicated (8) to BEECKMAN in 1628—1629 (*Journal* f 334r), at the same time characteristically disposing of it as "no wonder . . ., since a string twice as thick behaves in the same way as two simple strings separately."

5) In the twenty-five published letters to and from MERSENNE prior to 1625, (8) is not mentioned. Evidently in answer to questions from MERSENNE, there are discussions of sympathetic vibration

On 28 February 1629 MERSENNE proposes[1]) to BEECKMAN the problem of determining the motion of a vibrating string; in particular, of calculating the ratio of successive amplitudes. On 13 November and 18 December 1629 DESCARTES writes to MERSENNE that the amplitudes diminish in geometric progression. DESCARTES[2]) considers the restoring force to be proportional to the deflection; hence "... the force which makes the string return is greater in proportion as the string is pulled away from its straight line, and, being unequal, it makes the diminution of the returns likewise unequal, and that is the geometric progression." [If the remarks of DESCARTES are unsatisfactory, the reader should recall that an adequate theory of the viscous and frictional damping of a vibrating string remains to this day unknown[3]).]

In 1635 MERSENNE published a great treatise on acoustics and music, his *Books on harmonic matters*[4]). Book II gives a disordered list of propositions on vibrating bodies; [these show that MERSENNE is now somewhat beyond his depth in attempting to generalize from the definite results he had inferred from experiments on strings.] Prop. 1: The difference of sizes and shapes of bodies makes the difference of their sounds. Prop. 2: By so much the moister is a body, by so much lower is its sound. Prop. 3: By so much the harder is a body, by so much the higher is its sound. Prop. 4: The loudness and pitch of sounds are not always as the weight of the sounding body. Prop. 5: The denseness and rareness of bodies make different sounds, but not proportionally. Prop. 6: As the length of one body is to that of another body of like material, or as the volume to the volume, so is sound to

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in the letters from CLAUDE BREDEAU of 30 January 1625 and from JEAN CHATELIER of 12 April 1625; the latter shows that sympathetic resonance of a string tuned an octave, a twelfth, *etc.*, above the sounding string was more or less well known.

Later letters of MERSENNE contain hundreds of references to problems of vibration.

The book of R. LENOBLE, *Mersenne ou la naissance du mécanisme*, Paris, 1943, furnishes little or no information regarding MERSENNE's work on acoustics and strength of materials.

1) Letter of MERSENNE to ANDRÉ RIVET. BEECKMAN's replies of March, June, and 1 October 1629 do not go beyond his old work on this problem (above, pp. 25—26).

2) DESCARTES also tells MERSENNE sarcastically that he had explained sympathetic vibrations in a treatise he had left with BEECKMAN for eleven years (*i. e.*, since 1618), "and if that time suffices for copying it, he has the right to attribute it to himself." DESCARTES had indeed written such a treatise and left it with BEECKMAN, but BEECKMAN had written his explanation (above, pp. 25—26) in his Journal long before the entry stating that DESCARTES was in the course of writing the treatise (*Journal* f 104v); DESCARTES' explanation to MERSENNE is precisely the same as BEECKMAN's.

3) Even for the motion of a pendulum in air the question of frictional damping is one of celebrated difficulty. The first quantitative treatment is to be given by EULER, E569, "*De motu penduli circa axem cylindricum, fulcro datae figurae incumbentem, mobilis, habita frictionis ratione. Dissertatio altera,*" Acta acad. sci. Petrop. 1780: II, 164—174 (1784); presentation date: 19 August 1776. In this work EULER finds that the amplitudes decrease in geometric progression.

4) *Harmonicorum libri* ..., Paris, Baudry, 1635, [xii] + 184 pp.

sound. [As we shall see below, the former statement of this "broadest . . . of all propositions in music" is true, but the latter statement seems obviously to contradict it.]

Props. 7—13 and 18 state (8) and its various consequences at length. In commenting on Props. 8 and 9, MERSENNE writes that in order to increase by an octave the pitch of a string stretched by a 1 lb. weight, we have to stretch it not by 4 lbs. but by 4¼ lbs. [This may represent the correction arising from the slight stiffness of real strings.]

Props. 14—17 and 19 give evidence for (4) and are the source whence this basic acoustical law was immediately diffused. " . . . experiment always confirms that if two strings of brass, hemp, or gut are stretched until they are in unison, they make their returns in the same time, however their lengths and thicknesses may differ; whence it follows that the ratio of the sounds is the same as the ratio of the number of returns." For the "number of returns" MERSENNE introduces the term *frequency*. The pitch of an organ pipe may be defined as the frequency of a consonant string.

Prop. 21 seeks to establish "an exemplary and stable sound by which we may delimit the other sounds" [*i. e.* a standard of frequency]. The figure MERSENNE gives here and at several later points in the book suggests he thinks the shape of an initially triangular string remains triangular during the motion[1]). Coroll. 1 to Prop. 26, which asserts (4), states in effect that the frequency of large oscillations is about 3% less than that of small oscillations, *caeteris paribus*. Prop. 29: "All the returns of a string are approximately isochronous; that is, they occur in the same amount of time." The explanation shows that MERSENNE is thinking not so much of two different motions started with different amplitudes but of the successive vibrations in the same motion as it is damped. Thus Prop. 30 demands the time taken by the "whole motion". According to Prop. 32, repeated experiments show that the ratio of successive amplitudes decreases, but MERSENNE [following DESCARTES, *cf.* above, p. 29] considers that in a vacuum this ratio would be a constant, which his experiments suggest should be 20/19. [All of MERSENNE's statements about strings are interwoven with remarks on the motion of a pendulum; like BEECKMAN and GALILEO, he senses but cannot prove a connection.]

Warming to the subject of frequency, in Prop. 33 he writes, "Since this [concept of] frequency is applicable not only to strings but also to other bodies giving out a sound, as bells, organs, flutes, bands, *etc.*, let us now discuss only sinews or strings, from which the judgment of the rest may be gathered." Prop. 37: "To determine whether a sinew gives out a lower tone at the end than at the beginning of its motion . . ." Experiment shows that the amplitude decreases but the frequency remains the same; therefore the speed decreases.

1) DESCARTES objected not only to this but also to considering the motion as plane rather than whirling. *Cf.* his letter to MERSENNE of 15 May 1634.

If the pitch depended on the speed of the motion, it would thus decrease, but we hear no such effect. MERSENNE regards this argument [due to BEECKMAN, above, pp. 25—26] as crucial in favor of (5). A still better one follows: The points nearer the fixed ends of the string move at far lesser speed than those in the middle, yet the string gives out but one note. Finally, Coroll. 7 to Prop. 36 asserts that frequency is as "the Lydian stone" for everything concerning sound.

In Book III, Prop. 2 discusses the proportions to be assigned to the strings of an instrument so that it will give out an equable tone. According to Prop. 3, a musical string should be stretched to half the tension under which it breaks. Prop. 7 lists the results of experiments on the breaking strength of strings but reaches no definite conclusion, while Prop. 16 [contrary to the expectation of LEONARDO DA VINCI, above, pp. 19—20] asserts that experiments show the breaking strength of a long string to be the same as that of a short one, with some reservations.

A final attempt to determine the motion of a vibrating string, in Prop. 21, leads to nothing.

In the next year appeared MERSENNE's *Universal harmony*[1]), written in his own idiom and for the most part a still more diffuse account of what was in his Latin treatise. Prop. 8 of Book III asserts that "... strings and all other kinds of bodies make three or four different sounds at the same time, and these are harmonious." [To explain the former statement from mechanical principles while disproving the latter is to be DANIEL BERNOULLI's great achievement a century later.]

At the end of Prop. 8 MERSENNE writes, "... it does not follow that other bodies of cylindrical or other form obey the same law with respect to sounds as do strings, though many have believed so hitherto ..." Prop. 9, after remarking upon the difficulty of experiments on cylinders and repeating that their various tones are harmonious, gives experimental results which seem to imply that for similar prismatic bars having cross-sections that are circles, squares, triangles, *etc.*, we have

$$\nu \propto \frac{1}{a}, \qquad a = \text{typical linear dimension}. \tag{9}$$

E. g., to get a bar that sounds an octave higher than a given one, we are to cut down both the length and the diameter by $\frac{1}{2}$. [This law is correct[2]), though by the restriction to simul-

1 *Harmonie universelle* ..., Paris, Cramoisy, 1636. The date of the Privilège is 13 October 1629. From MERSENNE's letter of 20 March 1634 to PEIRESC we learn that the book was complete then and had cost ten years of work. I have never been able to consult the French and Latin treatises simultaneously; thus my citations do not imply that any particular statement in the one is not also in the other.

2) By dimensional analysis, for a material having elastic modulus E, density ϱ, and characteristic linear dimension a, we have

$$\nu \propto \frac{1}{a}\sqrt{\frac{E}{\varrho}}.$$

taneous proportional change of all linear dimensions it falls short of later results. In Prop. 10 MERSENNE notes that a cylinder is not held tense by "a weight or any other foreign force, but only by its own consistency." Coroll. 1 discusses inconclusively the effects of length, breadth, and depth on the vibrations of bars of tin or iron. Coroll. 2 proposes what seems to be the general law $\nu \propto 1/a^3$, [but this[1]) contradicts both (9) and the law that follows from (8), *viz*, $\nu \propto 1/a^2$].

Prop. 11 gives the results of experiments on bars of many materials. Since all woods give out nearly the same tone, as do both hard steel and soft iron, MERSENNE decides that little can be determined about a material by the sounds it emits. Prop. 16 attempts to correlate the pitch with the material of the sounding body, but offers only vague speculation.

Prop. 15 discusses the breaking strengths of beams in four tests: extension, transverse load in the middle, longitudinal thrust, and impact in the middle. We gather that MERSENNE does not consider his experiments complete, for he is hesitant to draw any conclusion. He thinks that for horizontal beams supported at both ends, the breaking force is inversely as the length.

A sequel to the *Universal Harmony*, published along with it, is the *Treatise on Instruments*. In Book IV, Prop. 11 asserts that "the string struck and sounded freely makes at least five sounds at the same time, the first of which is the natural sound of the string and serves as the foundation for the rest . . ." All these sounds "follow the ratio of the numbers 1, 2, 3, 4, 5, for one hears four sounds other than the natural one, the first of which is the octave above, the second is the twelfth, the third is the fifteenth, and the fourth is the major seventeenth." Then there is "a fifth one higher yet, that I hear particularly toward the end of the natural sound, and at other times a little after the beginning; it makes the major twentieth with the natural sound." Of all these, "none is ever heard that is lower than the natural sound of the string, for all are higher . . . They follow the same progression as the jumps of the trumpet." [Thus MERSENNE is the first to determine the *sequence of overtones* of the vibrating string[2]).]

In Book VII, Props. 7 and 10 claim to correct the bad practice of the bell makers by a

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The rule (8) is not included because, as MERSENNE in effect remarks, the transverse vibrations of a string are not elastic vibrations. From (8), or rather its generalization (10) below, follows $\nu \propto \frac{1}{a^2}\sqrt{\frac{T}{\varrho}}$ when all linear dimensions of the string are scaled proportionally, and this gives $\nu \propto 1/a^2$ in place of (9).

1) *Cf.* the second, false alternative in Prop. 6 of Book II of the Latin treatise, above, pp. 29—30.

2) MERSENNE's recognition of the pitches of these tones seems to date only from 1633, since in that year he proposed to several correspondents the problem of explaining them. On 30 May 1633 BEECKMAN replied that the "globules of air" may be broken into 1, 2, 3, . . . parts, *etc.* On 21 June 1633 BOULLIAUD transmitted MERSENNE's observation to GASSEND in a letter full of the new enthusiasm for science: "I hope to be able to prove something physically and geometrically by a cylinder and a cone inscribed in it . . ." *Cf.* also the replies of DESCARTES, 22 July 1633, and DE VILLIERS, September 1633.

better rule relating the tone of a bell with its dimensions [but what the rule is, I cannot determine.] In a later work[1]) MERSENNE asserts that for "bells, cylinders, and other bodies of the sort used in harmony" we have $\nu \propto 1/\sqrt[3]{W}$, where W is the weight. [This is but another expression for (9).

There are few figures in the history of science so appealing as MERSENNE. His work is often belittled for its errors, its contradictions, and its disorder. However, his positive achievements[2]), obtained not only before there was any theory but also long before any reasonable standards had been set for experiments, are the greatest ever gotten from purely experimental study of vibrations.]

1) Prop. III of "*Harmoniae liber primus,*" Art. II, in *Cogitata physico-mathematica in quibus tam naturae quam artis effectus admirandi certissimis demonstrationibus explicantur*, Paris, Antonius Bertier, 1644.

2) A discussion of MERSENNE's work on acoustics, including some of the topics we have presented and also his discovery, description, and explanation of *beats*, is given on pp. 35—58 of H. LUDWIG's *MARIN MERSENNE und seine Musiklehre,* Halle-Saale and Berlin, Buchhandlung des Waisenhauses, 1935.

Part I. The earliest special problems, 1638—1730

5. The vibrating string, the breaking of a beam, and the catenary in Galileo's Discorsi (1638). Since they were read by everyone, Galileo's *Discourses and mathematical demonstrations regarding two new sciences concerning mechanics and local motions*[1]) must be given greater notice here than their content or novelty would otherwise deserve[2]).

141 *a. The vibrating string.* At the end of the First Day, Salviati emphasizes that a pendulum can oscillate only at one determined frequency and describes what would now be called the phenomenon of resonance. [The example given is much the same as that published by Fracastoro almost a century earlier, but Galileo's writing is brilliant:] A single man by pulling the rope successive times at proper intervals can sound a great bell whose
142 motion suffices to lift four or six men off the floor. This allows us to explain "the wonderful problem of the string of a guitar or harpsichord which causes to move and resound another, not only one in unison with it but also one at the octave or the fifth" [*i. e.* twelfth. Here, too, Galileo's explanation is much like that of Fracastoro;] he mentions explicitly

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1) *Discorsi e dimostrazioni matematiche intorno a due nuove scienze attenenti alla mecanica ed i movimenti locali,* Leiden, Elsevier, 1638 = Opere (Ed. Nazionale) 8, 39—318 (page references are to this edition). In English, *Dialogues concerning two new sciences,* transl. H. Crew & A. de Salvio (with use of technical terms sometimes suggesting Galileo had had the benefit of a freshman course in physics), New York, MacMillan, 1914, and later reprints, besides two earlier translations by others. German transl., *Unterredungen und mathematische Demonstrationen . . .,* Ostwalds Klassiker Nos. 11, 24, 25, Leipzig, 1890—1891.

2) There is much evidence that some of the contents of the *Discorsi* dates from 1602 or earlier, but in Galileo's correspondence I have been able to find nothing whatever concerning the vibrating string or the catenary prior to the book itself, which was written, apparently, in 1630—1635.

Not so with the material on beams, for on 11 February 1609 Galileo writes to Antonio de' Medici as follows: "I have recently finished finding all the conclusions, with proofs, concerning the strengths and resistance of beams of various lengths, sizes, and shapes, and by how much they are weaker in the middle than at the ends, and how much more weight they will sustain if it is distributed along the beam rather than in one place only, and what shape they should have so as to be equally sturdy all along; which science is very necessary in making machines and all kinds of buildings, but there is no one who has treated it."

On 17 September 1633 Niccolò Arrighetti communicates to Galileo his views on the breaking of a heavy horizontal bar supported at its middle. His words are interpretable in two ways, one of which is consistent with the theory of heavy beams Galileo published later in the *Discorsi, i. e.,* with (14), which Galileo states, more or less, in his answer of 27 September 1633.

In March 1635 Galileo writes to Antonio de Ville an emphatic refutation of the prejudice in favor of scaling by simple proportion. Suppose a bridge can bear 1000 lbs. "It is desired to know . . . if another bridge, made of the same wood but with all its members increased fourfold . . . will be strong enough to bear 4000 lbs. There I say no; and I say no even thus far, that it could happen that such a bridge would not even be able to support itself, but would collapse from its own weight," *etc.* Galileo writes of the Second Day as if it were then complete.

(The three letters just described were first published in 1718.)

that "the string tuned to unison with the one touched is disposed to make its vibrations in the same time," *etc.*, [but he says nothing to explain the sympathetic vibration of a string tuned to the octave or the twelfth[1]).] "The undulation, spreading out through the air, moves and causes to vibrate not only strings but also any other body disposed to tremble and vibrate with the same [periodic] time as that of the trembling string, so that if one fixes upon the case of the instrument little pieces of bristle or other flexible material, when the harpsichord sounds it will be seen that now this, now that little body trembles too, according as is touched that string of the harpsichord whose vibrations occur in the same time: The others will not move at the sound of this string, nor will that one tremble at the sound of another." [Thus GALILEO perceives that a bristle has a
natural period, but he gives no attention to determining it.] The sounding of an appro- 143
priate tone on a musical string causes a glass nearby to emit the same tone, and if the glass is partly full of water, this same act induces standing waves on the surface. "... and sometimes it happens that the tone of the glass jumps up by an octave, and at the same moment I have seen each of those waves split in two, which effect most clearly shows the form of the octave to be the double[2])."

Into the mouth of Sagredo [and hence perhaps to be regarded as accepted science of 143
the day] GALILEO puts the statement that in order to make a string emit a tone higher by an octave it is sufficient (1) to shorten it by one half, (2) to quadruple the stretching weight, (3) to diminish its greatness[3]) fourfold, other things being equal. [We are tempted to conclude that MERSENNE's formula (8) was common knowledge. This is not so.] Sagredo is not convinced when the authors "who have written learnedly on music ... say that
the octave is contained in the double, ... the fifth in the three halves' part." From the 143—144
facts (1), (2), (3) one could just as well consider the octave as the quadruple [or as the inverse quadruple]. But since to number the vibrations of an audible sound is "entirely impossible," we could never know if "the string an octave higher really makes twice as many vibrations in the same time," were it not shown by the standing waves on the water glass. [Thus GALILEO regards (8), or at least the satisfactory establishment of it, as his own.]
Indeed, after recounting the celebrated observation that an iron file which emits a tone 144—146
when scraping brass leaves parallel and equidistant scratches, the closer together the higher the sound, Salviati goes on to correct (8). The effect Sagredo refers to greatness "ought more properly to be attributed to weight"; Salviati then states clearly that

1) Thus it is unlikely that GALILEO was influenced by the more complete idea of resonance which BEECKMAN had developed in 1618 and which MERSENNE had published in 1635.

2) This remark was to be appropriated by BLONDEL in 1681; see Hist. acad. sci. Paris 1666—1699, 1, 4to ed., Paris, 322 (1733).

3) I translate "grossezza" by "greatness"; from the context it is plain that GALILEO here means "cross-sectional area", while in the Second Day he means "depth".

$$(10) \qquad \nu \propto \frac{1}{l}\sqrt{\frac{T}{\sigma g}}\,, \qquad \sigma g = \varrho A g = \text{weight per unit length,}$$

independently of the material. [This capital refinement of (8) GALILEO may have inferred from experiment[1]).]

146—147 Salviati goes on to say that "the nearest and immediate reason [or rule?] for the forms of musical intervals is neither the lengths of the strings nor the tension nor the bulk, but rather the proportion of the numbers of the vibrations . . . Consonant and pleasantly received will be those pairs of sounds that strike upon the tympanum of the ear with some order[2]), which order requires first that the blows made within the same time be commensurable in number, so that the cartilege of the tympanum shall not have to be in a perpetual torment, bending itself in two different ways so as to agree and obey the ever discordant beating." To this Simplicio, who has long been silent, says "I should like this
147—149 matter explained with greater clearness." [The following explanation is most confusing]; the amplitude is at first taken proportional to the period, but it seems this is only a device for visualizing the period as a line. Without actually stating an analogy between the vibrating string and a pendulum, GALILEO plays upon the effect of resonance noted above; the "order" of the commensurable vibrations seems to consist in the fact that the two oscillating points if started at the same time will reach their maximum displacements simultaneously after a stated number of periods.

[GALILEO's contribution to the science of vibration has been exaggerated. His

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1) This is not proved by his explanation, which in addition to asserting a comparison between the tones of harpsichords fitted with brass and gold strings, respectively, draws an analogy to the different resistances attributable to the weight and the size of a body moving in a medium.

MERSENNE did not know the correct dependence of ν on σ at this time, as is shown by his references to "thickness and material" in Prop. 18 and the discussion of the effect of qualities such as hardness in Props. 41 and 42 of Book II of *Harmonicorum libri* (cited above, p. 29). In Prop. 4 of Book III MERSENNE gives a table of measured frequencies of strings as a function of their weights when T, A, and l are the same; while these measurements may be seen to verify (10), MERSENNE does not perceive this proportion.

In Props. 17—18 of Book III of his French treatise (cited above, p. 31), MERSENNE in reporting experiments on various kinds of vibrating bodies writes that it is very difficult to determine the effect of the density, and his results seem to contradict any simple dependence upon it.

In his *Cogitata* (cited above, p. 33), published after the appearance of GALILEO's work, MERSENNE states (10) in Prop. II of Art. II of *Harmoniae liber primus*.

MERSENNE expressed great admiration for GALILEO, who did not reciprocate. MERSENNE attempted to correspond with GALILEO from about 1625 onward, but with little success. MERSENNE took careful account of everything GALILEO published and had knowledge of some of GALILEO's unpublished work. There is no indication that GALILEO took any notice of the work of MERSENNE.

2) This is not a new idea, being merely a mechanical paraphrase of the PYTHAGOREAN views, which were held, in one form or another, also by many other scientists, *e. g.*, by BEECKMAN.

adopting (5), which was not new, doubtless hastened its widespread acceptance. His formula (10) is an important refinement of MERSENNE's formula (8), but he gives no evidence of knowing many of the experimental facts observed and published by MERSENNE. For example, while he barely mentions harmonic resonance, he states nothing regarding the overtones of a string. On the other hand, there is no hint of mathematical proof or even theory. Like BEECKMAN, GALILEO sets the sonic motions side by side, as it were, with the swinging of a pendulum, but he does not apply mechanical principles at all and does not even state (4) explicitly, although it is presumed by (5). In regard to the vibrating string, GALILEO is inferior to MERSENNE as an experimenter, inferior to BEECKMAN as a theorist, but superior to both in imagination and in persuasive writing.]

b. The breaking of beams. The *Discorsi* open with Salviati's statement that "the com- 50
mon opinion" that a machine proportionately larger is also proportionately stronger is
"absolutely wrong". [In a word, GALILEO will initiate us into the mysteries of *scaling*
laws.] He begins by considering the breaking of a column by pulling it, but he is diverted to 55
other subjects; when he returns, we find that he considers the breaking strength of a bar in 156—157
tension to be *independent of the length.* He has told us that the coherence of some solids, 54—55
at least, is like that of a rope, in reference to which he gives the following argument.
Salviati says, "I fear, Simplicio . . ., that . . . you are making the same mistake as many 161—162
others; that is, if you mean to say that a long rope . . . cannot hold up so great a weight as a shorter length . . . of the same rope." He attaches a weight C (Figure 5) just sufficient to break the rope and asks Simplicio where the break will occur, and Simplicio replies, "Let us say at D . . ., because at this point the rope is not strong enough to support, say, 100 lbs." Salviati then, fixing the rope at F, just above D, and attaching the weight at E, points out that at D the rope is still subject to the same pull, and thus the short segment FE will break again at D, by Simplicio's admission. [While this *reductio ad absurdum* is in itself unsound, it convinced many readers and has been repeated by many later authors. To complete the argument one has to assume that the section of rope DB has no function but to transmit the force of the weight, and this is tantamount to assuming the conclusion. The value of this passage lies in its considering the whole effect at D of the rope and weight DC to be a force in the direction DEB. In replacing the action of the system below D on that above D by one force, it furnishes the first primitive example of the *stress principle* of continuum mechanics[1]).]

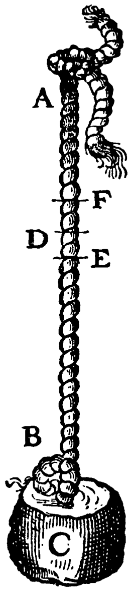

Figure 5. Sketch for GALILEO's argument to show that a long rope is as strong as a short one (1638)

1) By later authors and historical writers GALILEO's arguments on beams are sometimes pre-

156—157 Shortly after the beginning of the Second Day, GALILEO takes up the problem of a prismatic beam built in at one end and loaded by a weight at the other (Figure 6). He regards the beam as a compound lever with fulcrum at the under side *B*; the length *BC*

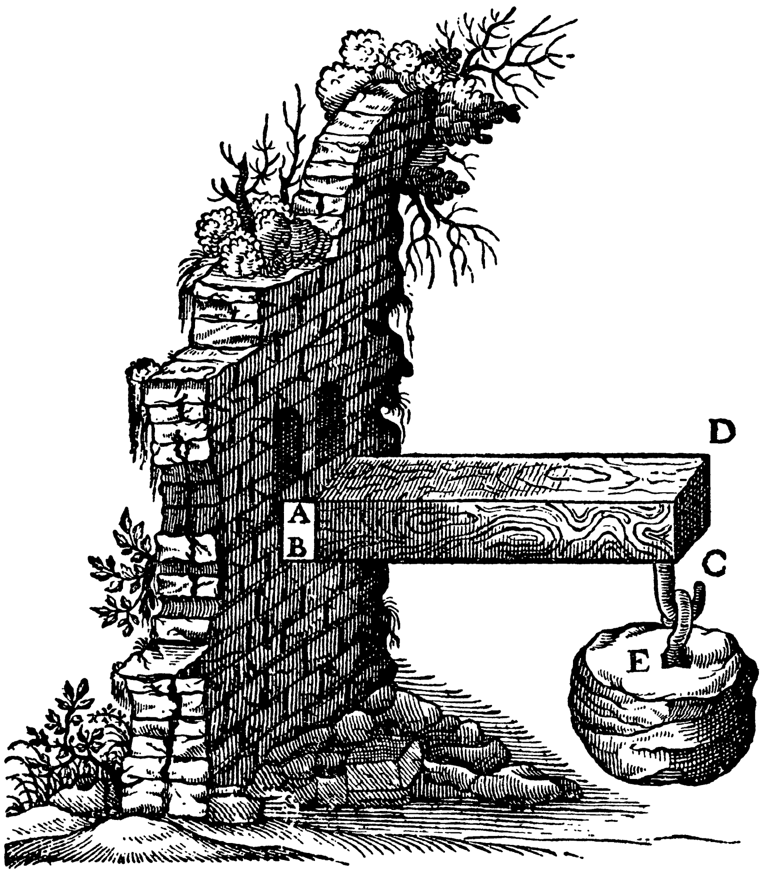

Figure 6. GALILEO's figure for the breaking of a beam by terminal load (1638)

is one arm, on which acts the weight *E*, and [half of] the greatness [*i. e.* depth] *AB* is the other arm, "in which resides the resistance." The first proposition is, "The moment of the force at *C* to the moment of the resistance . . . has the same proportion as the length *CB* to the half of *BA*, and therefore the absolute resistance to breaking . . . is to the resistance [in the present case] in the same proportion as the length *BC* to the half of *AB* . . ." The "absolute resistance" is "that which occurs when the beam is pulled straight on, since then there is as much motion in the mover as that of the moved." [This last

sented in terms of the concept of stress, but it is not to be found in GALILEO's own words. See esp. p. 159, where the temptation is great.

is difficult to understand; we infer that] GALILEO's "absolute resistance" is the weight P_t required to break the beam by direct pulling. Thus the proposition reads[1])

(11) $$\frac{P_t}{P_b} = \frac{l}{\frac{1}{2}D}\,, \text{ where}$$

P_t = "absolute resistance" or breaking force in tension,
P_b = breaking force in bending by terminal load,
l = length,
D = depth or thickness.

While GALILEO says this follows "from the things asserted", the preceding passage merely describes the actions of levers and mentions the common experience that a long beam is broken by a lesser weight transversely than directly. In order to take the weight of the 157—158
beam into account, add half of it to P_b.

When a beam is loaded first in the direction of its thickness D and then in the direction 158
of its breadth B, by (11) we see that the breaking strengths P_b in the two cases stand in the ratio D/B, explaining why a rule supports a much greater weight when stood on edge than when laid flat.

"There is no doubt" that the [absolute] resistances P_t of two cylinders are to each 160
other as the base areas, "since by so much greater are the fibres, the filaments, or the tenacious parts that hold together the parts of the solid." [That is,

(12) $$P_t = KA\,,$$

where A is the area of the cross-section and where K is a constant depending only on the material, not on the shape.] From (11) follows

(13) $$P_b \propto \frac{AD}{l}\,, \quad \text{or} \quad P_b \propto \frac{D^2B}{l}\,,$$

where the latter form is asserted for rectangular beams. An argument supporting the so far unproved basic formula (11) is now supplied. The filaments are "scattered over the whole surfaces" of the cross-sections, so they may be regarded "as if all were reduced to the centers." [Thus we see that (11) results from the *balance of moments* about the lower edge of the beam. The moment of the load E is $P_b l$; this equals the moment of the absolute resistance P_t, thought of as concentrated at the *mid-point* of the base; therefore

$$P_b l = P_t \cdot \tfrac{1}{2}D\,.$$

Later writers will replace this crude approximation by an integral over the base (see below,

1) The lengths B and D are defined here for consistent later use; they are not to be confused with the points labelled B and D in Figure 6.

162—163 pp. 61—62, 102—104).] Taking $A \propto d^2$, where d is now the diameter or typical linear dimension of the cross-section, since $D \propto d$ Galileo obtains from (13) the rule (3), [known at least in part to Leonardo da Vinci].

163—164 To analyse the bending of a beam due to its own weight, Galileo considers the weight W concentrated at some unspecified point, so that (3) applies. Since $P_{\mathrm{t}} \propto d^2$ by (12), it follows from (13) with $W \propto P_{\mathrm{b}}$ that

$$Wl \propto d^3 \propto (P_{\mathrm{t}})^{\frac{3}{2}}, \tag{14}$$

a result which Galileo interprets as asserting that the ratio of the bending moments exerted by similar heavy beams is as the $\frac{3}{2}$ power of their breaking strength in tension.
165 "Among heavy prisms and cylinders of similar figure, there is one and only one which under the stress of its own weight lies just on the limit between breaking and not break-
166—169 ing . . ." There follows a [mysterious] passage in which Galileo tries to apply (14) so as to determine the scaling rule for a beam to break under its own weight, or, more generally, to determine the laws under which an arbitrary relation between bending moment and resistance is preserved. [Much of his reasoning is correct, but his summary of it is not[1]). Writing M for the bending moment, replace (11) by $M = \alpha D P_{\mathrm{t}}$, and for bending of a heavy beam take $M = \beta W l$, where α and β are constants[2]); since $W = \varrho g A l$, by (12) follows $\beta \varrho g l^2 = \alpha K D$, or

$$D \propto l^2 \, .] \tag{15}$$

169 Thus "not only art, but also nature cannot make its machines grow to a vast immensity" unless harder and harder materials are found, for to make a beam of greater length have a proportionately greater strength requires a disproportionate thickening, as Galileo illustrates by a figure of a little bone and one three times as long and sufficiently strong as to "perform the same function". [Galileo does not disclose what the function is, and he carefully avoids saying what scaling law he uses. Measurement of his figure indicates that he takes $D \propto l^3$. Be this as it may,] Galileo concludes that "if the size of a body is diminished, the strength of that body is not diminished in the same proportion; indeed, the smaller the body the greater its relative strength." [This may be true, but it is a flowing generalization of the very special results he has obtained.]

173 By an appeal to symmetry, Galileo infers that if a beam is just long enough to break

1) The error is not noted in any edition or translation I have seen. Both toward the end of p. 167 and at the beginning of p. 169 Galileo states that $d^3 \propto P_{\mathrm{t}}$, contradicting his own result $(14)_2$, which is stated in his Prop. VI. The passage is hard to understand because of shifty wording and may be corrupt.

2) On pp. 157—158 Galileo has said that $\beta = \frac{1}{2}$. The formula $M = \frac{1}{2} \varrho g A l^2$ is the essential content of his Prop. 3.

when built in at one end, a similar beam twice as long is just long enough to break when simply supported at its middle or at its two ends. [This is the first occurrence of an argument later to be used frequently in connection with elastic curves.]

Toward the end of the day GALILEO proposes the problem of the solid of equal resis- 178—181
tance. Such a solid is so shaped that its absolute resistance at each cross-section is just sufficient to balance a fixed load of a given type. From (13) we see that for a weightless beam loaded by a weight at one end, the general equation of such a solid is

$$AD/l = \text{const.} \tag{16}$$

GALILEO assumes the solid to be a cylinder with horizontal generators normal to the plane of bending; then $A \propto D$, and from (16) the generating curve is $D^2/l = \text{const.}$, a parabola. [To the problem of calculating solids of equal resistance subject to various loads and geometrical conditions a large subsequent literature was devoted[1]).]

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1) GALILEO's theory is applied to different shapes and different loads by V. VIVIANI, "*Trattato delle resistenze,*" completed by G. GRANDI, Opere di GALILEO **3**, 193—305, Firenze, 1718 = Opere di GALILEO **3**, 213—307, Padova, 1744. A diffuse account and elaboration of GALILEO's theory is given by FABRI, Lib. V of Tract. II of *Physica, id est, scientia rerum corporearum* . . . [1], Anisson, Lugduni, 1669. According to MUSSCHENBROEK, FABRI is often in error.

Cf. MERSENNE, Props. 18—19 of "*Tractatus mechanicus theoricus et practicus,*" included in his *Cogitata,* cited above, p. 33.

Cf. also RICCI's letter to TORRICELLI of 18 July 1643.

GALILEO's results are attacked by BLONDEL in two discourses dated 1657 and 1661, being the fourth part of "*Resolution des quatre principaux problèmes d'architecture,*" Paris, 1676 or 1677 = Mém. acad. sci. depuis 1666 jusqu'à 1699, **5**, 355—530 (1729). HUYGENS saw this work, and in his letter to LODEWIJK HUYGENS of 10 August 1662 he expressed a low opinion of it: " . . . at least for me, these are very easy things." HUYGENS himself, in notes dating from 1671, Oeuvres complètes **19**, 70—72, considered a rectangular beam fixed obliquely into a wall, as had FABRI. A. MARCHETTI, *De resistentia solidorum,* Vangelisti & Martini, Florence, 1669, [xii] + 127 pp., claims in his preface to disprove GALILEO's proposition that the prismatic solid of equal resistance is parabolic. According to MUSSCHENBROEK, there are errors in MARCHETTI's work.

Examination reveals that MARCHETTI adopts (13), spins out endless corollaries and generalizes it to beams of various simple shapes, including non-prismatic ones, but I do not find in his text either errors or the source of his criticism of GALILEO. His Props. LXXXII sqq. on parabolic beams seem to agree with GALILEO's theory.

G. GRANDI's *Risposta apologetica* . . ., Lucca, Pellegrino Frediani, 1712, [xvi] + 288 pp., is a most wordy answer to MARCHETTI. Pp. 45—47 give a chronology of the work of BLONDEL and MARCHETTI from 1649 to 1673. Lib. II, Cap. VII, gives seven propositions which are claimed to correct those of MARCHETTI on solids of equal resistance.

It is difficult to find sense or interest in this diffuse literature. It exemplifies the common historical experience that once mechanical principles, right or wrong, sufficient to set definite and not too difficult *mathematical* problems are proposed by a recognized authority, an abundant harvest of *taediosa* follows.

Further bibliography is given by PEARSON, § 5 of *op. cit. ante.* p. 11.

186—187 "But, in order to bring our daily conference to an end, I wish to discuss the strength of hollow solids, which are employed in art, and still oftener in nature, . . . so as greatly to increase strength without adding to weight. Examples are seen in the bones of birds and in many kinds of reeds . . . For if a stem of straw which carries a head of wheat heavier than the entire stalk were made up of the same amount of material in solid form it would offer less resistance to bending and breaking." Comparing a hollow cylindrical tube with a solid
188—189 one of equal area and length, since by (12) P_t is the same for each, we see by (11) that their breaking strengths P_b for bending are in the ratio of their diameters. "Thus the strength of a hollow tube exceeds that of a solid cylinder in the ratio of their diameters . . .[1])," and the more general proportion $(13)_1$ applies for all cylinders of the same material.

[In summary, GALILEO takes account of the effect of a load on a beam only through its moment. He recognizes that the resistance of the beam is due to the mutual action of its fibres but is unable to formulate a mathematical theory in which these fibres occur. He tacitly regards a solid body as *rigid and undeformable prior to rupture*. In accord with this, he takes it as self-evident that the criterion for failure[2]) is the *magnitude of the load*.

While GALILEO proves the various corollaries following from (11) with elaborate rigor, for the basic law (11) itself he gives only some mysterious juggling[3]). It is sometimes said that GALILEO regarded the stress in the beam as uniformly distributed over the cross-section; while this false assumption suffices to derive (11), GALILEO himself uses no concept of interior stress, and his regarding P_t as acting at the midpoint of the base is no more than a guess or a postulate. Since all his subsequent results are proportions such as (13), the

1) GALILEO does not notice the paradoxical corollary that the strongest tube of given area is of infinite radius and zero thickness.

2) *Cf.* also the discussion at the beginning of the First Day, esp. p. 55. The modern literature often attributes to GALILEO the idea that a solid fails when a certain maximum stress is attained; indeed, this is a natural modern inference from his expressed viewpoint, but of course nothing of a local character occurs in his work.

3) The two weak points in GALILEO's theory of strength, namely, (12) and the factor $\frac{1}{2}$ in (11), were pointed out by BALIANI, who in his letter of 1 July 1639 to GALILEO writes, "I wish you had explained ever so little more," *etc.* GALILEO's answer of 1 August 1639 gives a vague allusion to the symmetry of the cross-section and the law of the lever but does not face the issue.

In his celebrated critique of the *Discorsi*, sent to MERSENNE on 11 October 1638, DESCARTES pounces upon (11): that ". . . the force . . . is like a lever with fulcrum at the middle of its thickness . . . is not at all true, and he gives no proof of it."

As regards the catenary, "His two means of describing the parabola are merely mechanical, and in good geometry they are false." (Doubtless DESCARTES knew of BEECKMAN's partial proof that the parabola corresponds to uniform load per unit horizontal length (above, § 3), whence it is clear that the catenary is not a parabola.)

Most of DESCARTES' criticisms are ill taken, however, as when he denies the dependence on σ as given by (10), asserting instead that strings of different materials vibrate at different frequencies in consequence of the differences of hardness.

error introduced by the factor $\frac{1}{2}$ is cancelled out, but the error resulting from neglect of the bending of the beam is not. According to GALILEO's theory, in effect, the dimensionless scaling parameter is KAD/lP_b, where K is the mean stress for rupture in tension; according to the BERNOULLI-EULER theory (below, § 60), the parameter is KAD^2/l^2P_b, where K is the stress required to produce a specified elastic strain of the fibres. By dimensional analysis alone, the general parameter is $\frac{KA}{P_b} f\left(\frac{A}{D^2}, \frac{D}{l}\right)$, where f is a dimensionless function to be determined by some hypothesis of elasticity or rupture. In engineering practice it is customary to take $f(\zeta, \eta) = \eta^a$, where $1 < a < 2$; in a sense, that is, to interpolate between GALILEO's theory and the BERNOULLI-EULER theory.

The central concept of modern theories of materials is the *stress vector*, introduced in its final generality by CAUCHY in 1822. In this history we shall follow with especial care and interest the preliminary concepts from which it grew. To this end, the properties defining it must be distinguished:

i. Its *dimensions* are [force]/[area].
ii. In elasticity theory, there is a *material constant* of the same dimensions.
iii. The constant mentioned in (ii) represents a specified stress required to produce a specified *elastic strain*.
iv. The stress vector represents the action of interior parts of the body upon one another.
v. The stress vector may subtend an arbitrary angle with the (imagined) boundary across which it acts.

All these properties are independent of each other and belong to varying levels of sophistication in mechanics.

The equation (12), described in words by GALILEO, introduces properties (i) and (ii); in this sense, we may say that GALILEO *initiated the theory of stress*. But in his work there is no trace of any of the further properties, except for the hint toward (iv) mentioned on p. 37. In particular, while K in (12) is a material constant having the dimensions of stress, it is *not* an elastic modulus, being rather the stress such that, if uniformly applied over a cross-section, it will *rupture* a body heretofore rigid.]

c. The hanging cord. Among other means of describing a parabola, GALILEO mentions 186
the following. "Fix high up on a wall two nails equally distant from the horizontal . . . and from them hang a little thin chain . . .; this little chain will bend itself into a parabolic figure[1])." [Thus GALILEO's ideas are inferior to the unpublished work of BEECKMAN on the

1) On pp. 369—370 of vol. 8 of the Ediz. Naz. is a fragment indicating that GALILEO's motive for this supposition is an analogy with the motion of a projectile, which he knew to be parabolic: Just as the parabola of a projected body is described by two motions, horizontal and perpendicular, so the form of the little chain results from two forces: horizontal, from what pulls it at the end, and per-

catenary[1]).] The *Discorsi* close with GALILEO's proof that any string, no matter how tightly stretched, sags somewhat in the middle. To this end GALILEO considers the weight of the string as concentrated at its center, [as had LEONARDO[2]).

d. GALILEO's method. To the reader without preconceptions, GALILEO's writings on our subject bring a strange experience. A complete absence of mathematical proof at essential points[3]) is set against a background of an almost Platonic love of regular geometrical figures and strict demonstration of trivial details, accompanied by a complete absence of reference to specific experiment. Experiments, indeed, can scarcely have entered the process, since most of the physical assertions GALILEO makes are not consonant with later experiments. Rather, it is difficult to regard his work as more than a sequence of ingenious conjectures, brilliantly described and eloquently pled.

In contrast to earlier writers, GALILEO here avoids seeking causes and never attributes anything to "tendencies". Not only are his words usually clear and concise, but also he is the first to put forward any considerable body of definite, quantitative statements, capable of subsequent proof or disproof by reason or experiment.

For his application of statical principles to the problem of rupture of a beam he deserves to be regarded as the founder of the theory of strength of materials. His great achievement here is refutation of the common idea (indeed, common even today) that all effects are proportional to the sizes of the members, and his construction of a *theory of scaling.* That his proportions are correct only subject to a hypothesis not generally verified in practice is less important than that he did obtain *definite scaling laws*, right or wrong. Herein lie his enormous insight and originality.]

6. The unpublished work of HUYGENS on the suspension bridge (1646), the breaking of a beam (1662), the vibrating string (1673), and the vibrating rod (1688).

a. The suspension bridge. On 28 October 1646 HUYGENS[4]), seventeen years old, writes to MERSENNE, "In another letter I will send you the demonstration that a suspended chain

pendicularly downward, by its own weight. The same reason is advanced somewhat less clearly at the end of the Fourth Day, pp. 309—310.

1) According to LEIBNIZ, JOACHIM JUNG "excluded the parabola by calculations begun and experiments finished, but could not find the true line." I have never been able to see the book of JUNG, *Geometria empirica*, Rostock, 1627; later eds., Hamburg, 1630, 1642, 1649.

On 18 June 1645 RICCI writes to TORRICELLI that a friend wished to measure depths by the fall of a line hung from the two sides. RICCI suggests letting a weight run freely over the line; he can prove that the two sides of the string will then be inclined equally to the horizontal. This is a rediscovery of the result of LEONARDO DA VINCI (above, p. 21).

2) The proofs and drawings of GALILEO and LEONARDO here are similar.

3) *Cf.* footnote 3, p. 42.

4) All works of HUYGENS are cited from his Œuvres complètes, where the letters and pre-

or string is not at all parabolic, and what must be the pressure on a mathematical or weightless string in order to hang so, of which I have found the demonstration not long since." MERSENNE replies on 16 November 1646, "... if you can adjoin also the way in which to press it so as to make it hyperbolic or elliptic, you will surpass yourself." [The importance placed on familiar curves seems frivolous today, but was scarcely avoidable prior to the "calculation of curves", as the infinitesimal calculus was often called in its early days[1]).]

In his analysis, not published during his lifetime, HUYGENS considers the weightless string loaded by discrete weights, [as had BEECKMAN]. He sketches treatments starting

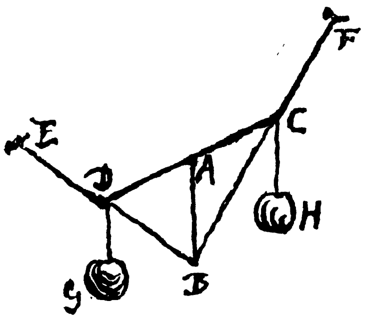

Figure 7. HUYGENS' drawing for STEVIN's theorem (1646)

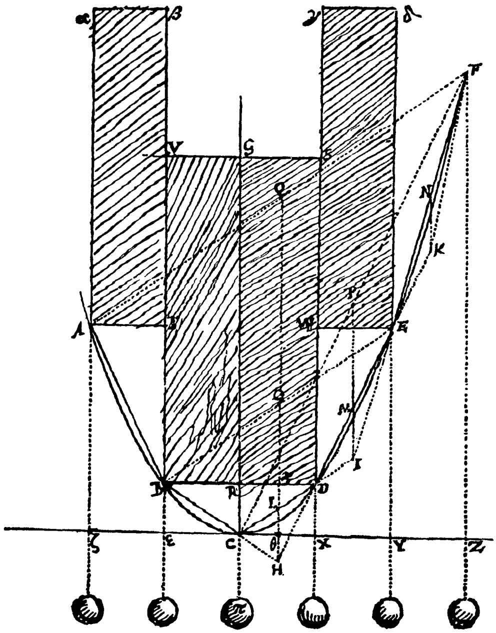

Figure 8. HUYGENS' drawing for the problem of the suspension bridge (1646)

from two different statical principles. The first method[2]) is based on a theorem[3]) he attributes to STEVIN: When the weights *G* and *H* in Figure 7 are equal, the vertical through the midpoint of a segment meets the two adjacent segments produced. The second treatment[4]) rests on an *extremal principle:* "The center of gravity descends as far as possible." To disprove GALILEO's claim, HUYGENS passes a parabola through three points and then shows it

viously unpublished fragments are printed in chronological order; thus detailed citation is usually superfluous. Most of the correspondence between HUYGENS and LEIBNIZ was published also in LEIBNIZens math. Schriften 2.

1) *Cf.* the comments HUYGENS was to apply many years later to JAMES BERNOULLI's solution of the problem of the elastica, below p. 97, and also footnote 2, p. 68.

2) Pieces No. 20 and 21, which, despite being written in different languages, form a single work. They date from November, 1646, as does No. 22; according to a note on p. 811 of Œuvres **10**, by 15 June 1646 DESCARTES had seen and approved some form of HUYGENS' work.

3) Proof of a generalization is given below, p. 67.

4) Piece No. 22. Throughout his life HUYGENS made much use of this principle.

cannot pass through the rest. Proposition 10 of the first treatment asserts that the figure of a continuous chain does not differ appreciably from that of one composed of infinitely many links. [No real limit process is involved.] Propositions 11 and 12 assert that *if from a weightless string equal weights are suspended at equal horizontal intervals, the points where they are suspended will lie on a parabola*[1]) (Figure 8). Hence the limit form for the continuous cord subject to uniform weight per unit horizontal length is also a parabola. HUYGENS asserts also that if equal parallelograms are placed upon the string as shown in the figure, the points of application again lie upon a parabola; [this, as he himself was to note in 1668, is false[2])]. For a more condensed presentation in final form, HUYGENS selected the approach based on STEVIN's theorem, but his little treatise was not published during his lifetime[3]). HUYGENS' arguments, resting heavily on special properties of conic sections, are hard to follow. He gives no hint of how he was led to suspect the particular kind of loading that would yield a parabolic figure[4]).

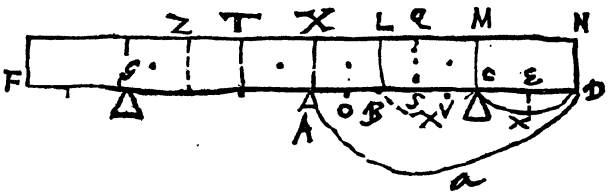

Figure 9.
HUYGENS' drawing for the breaking of a supported beam (1662)

b. The breaking of a beam. The problem of fracture of a heavy rectangular beam supported at two points (Figure 9) is considered by HUYGENS

1) A proof is given below, p. 67.

2) Since the pressure of the parallelogram on a frictionless string is normal, the tension T is constant; thus, in the notation to be used below in connection with the catenary, we are to integrate $T\sin\theta = T\frac{dy}{ds} = kx$, where T is constant; the result is a circle.

3) "*De catena pendente,*" Œuvres complètes **11**, 37—44. Our figures are reproduced from this version.

4) Between 8 December 1646 and 3 January 1647 MERSENNE received some version of HUYGENS' solution. On 24 January 1647 MERSENNE writes that he accepts the results but not all the proofs. In particular, HUYGENS had established equilibrium by asserting that "there is no cause for them to change their position;" MERSENNE objects that "Just because you see no cause, it does not follow that none exists, we do not see all at the first glance, and what does not appear to us at one time often does appear at another, it is enough that we can doubt whether there be any cause." Another fragment by HUYGENS, from 1647, treats the subject along the same lines. On 15 May 1648 MERSENNE writes, "will you permit the printing of the little treatise . . . on the string or chain hung equally? But it would be necessary to add the demonstration of what I wrote you about it." On 12 July 1648 HUYGENS replies that he will finish the treatise within another week; he regards STEVIN's proof of the statical principle as insufficient, and he will include a new proof of it. This is the end of the correspondence; MERSENNE died on 1st September 1648.

Presumably the version cited in the preceding footnote was that intended for publication. It is on its margin that HUYGENS noted in 1668 that the solution is incorrect for the loading by parallelograms; see the note on pp. 43—44 of Œuvres **11**. It seems that aside from this one remark in 1668, HUYGENS gave no attention to the problem of the hanging cord in the years 1647—1689.

in a note[1]) from the year 1662. He regards the beam as bent only at the point of fracture A. His hypotheses seem to be: (a) Wherever the fracture occurs, the angle γ between the two parts of the beam is the same, and (b) rupture occurs at the point such as to render the "descent of gravity" a maximum. [The "descent of gravity" is the loss of potential energy due to the descent of the centers of gravity of the two segments. We see here a first glimmering of an energy criterion for failure, with elastic energy of course neglected. It can be shown[2]) that this potential energy $= \mathcal{M}\gamma$, where $\mathcal{M} =$ the moment exerted by the support and the weight of either segment, taken about the point where rupture occurs. Thus HUYGENS' proposal is equivalent to the more plausible idea that the beam breaks at the point where *the moment of the applied load is greatest.* In all this, it seems most artificial that the angle γ should be assumed constant, but this angle disappears in the calculation, yielding a unique point of rupture,] which HUYGENS obtains in a special case.

c. The vibrating string. Since his earliest youth, HUYGENS had been incited by MERSENNE to provide a theory for the vibrating string[3]). In a work published in 1673, HUYGENS[4])

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1) Œuvres complètes **16**, 381—383. The same problem is treated in a fragment from 1688—1689, Œuvres complètes **19**, 74—75.

2) See the editors' explanation, Œuvres complètes **16**, 333—336, which determines the point of fracture in general according to HUYGENS' proposal. It results that the point of fracture is such as to render the weight borne by each support equal to the weight of the portion of the beam resting upon it after the break.

3) On 16 November 1646 MERSENNE proposes to HUYGENS the problem of explaining the law $\nu \propto \sqrt{T}/l$. "I foresee that your foundations of mechanics show that to make a motion twice as fast, perhaps four times as much force is required . . ." HUYGENS replies that he has thought about the matter often, but the solution would be very difficult. On 8 January 1647 MERSENNE proposes the problem anew, recalling that the successive amplitudes decrease in geometric progression (*cf.* above, p. 30).

(In February of 1645 MERSENNE had proposed to TORRICELLI the proof that $\nu \propto \sqrt{T}$ is a consequence of mechanical laws. TORRICELLI's reply, written in the same month, suggests that there may be some analogy to his hydrodynamical theorem.)

A letter of 12 January 1647 from MERSENNE to CONSTANTIN HUYGENS, the father of CHRISTIAAN, says that the explanation of the simultaneous harmonic sounds is "the greatest difficulty I have encountered in music."

A letter from MARIOTTE to HUYGENS on 1st February 1668 shows that no advance on the problem of the vibrating string beyond GALILEO's work was known to MARIOTTE at that time.

In a fragment written in 1675 (Œuvres **19**, 366—367), HUYGENS, after describing the sequence of overtones of the string, writes "And it is probable that these [harmonic] tremblings still occur, though feebly, when the whole string is sounded freely, and since there are so many ways of making this 12th [*i. e.* the second harmonic], that is the reason why one hears it always along with the sound of the string sounded freely."

4) *Horologium oscillatorium sive de motu pendulorum ad horologia aptato demonstrationes geometricae,* Paris, 1673 = (with accompanying French translation) Œuvres complètes **18**, 69—368. See Pars Secunda, Prop. XXV.

had shown that motion of a body sliding down a cycloid is isochronous. The proof does not involve any calculation of forces; rather, HUYGENS approximates the cycloid by tangents, to which he applies GALILEO's laws for motion on an inclined plane. In a fragment written in the same year or the next[1]), he states and proves as a corollary of the above that the "gravity" [accelerating force] of a body resting on a cycloid is as the length of arc from the bottom. [This apparently puts him in mind of proving the isochrony of other types of motion by showing that the accelerating force is proportional to the displacement, but this he left to his editors to say for him.] With this much in hand he strove to render definite [BEECKMAN, MERSENNE, and GALILEO's] analogy between the vibrations of a
2 string and the oscillations of a pendulum. As a model for a vibrating string he considers *a weightless cord loaded by a single central weight*, intended to represent the mass of the string

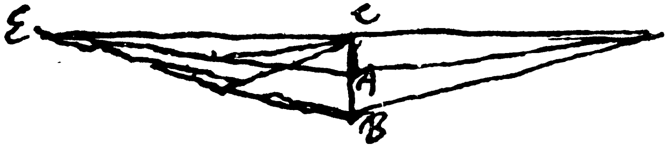

Figure 10.
HUYGENS' first model for the vibrating string (1675–1676)

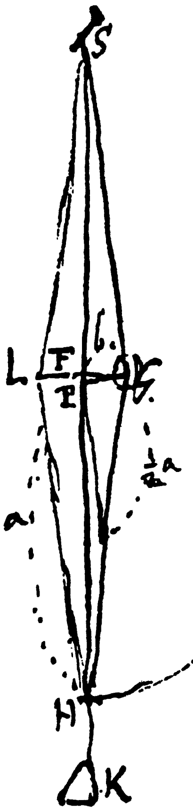

Figure 11.
HUYGENS' second model for the vibrating string (1675–1676)

(Figure 10). First he considers a horizontal string in circular vibration,
3, 5 which he finds to be isochronous if the radius is small enough. Then he considers a vertical cord stretched by a weight (Figure 11); he neglects the difference of tensions in the two parts of the string caused by the weight in the middle. In effect, HUYGENS constructs a cycloidal pendulum such that the restoring force equals the resultant force of the tension on G. Knowing the period of a cycloidal pendulum, HUYGENS is then able to write down the period of the system shown in Figure 11. His result, [here expressed in modern notation, is the correct one,] *viz*

$$(17)\qquad \nu = \frac{1}{\pi}\sqrt{\frac{T}{lM}} = \frac{1}{\pi l}\sqrt{\frac{T}{\sigma}}\,, \qquad \begin{aligned} M &= \text{mass of } G \equiv \sigma l\,, \\ l &= \text{length of string,} \\ T &= \text{tension exerted by } K. \end{aligned}$$

4 In the special case when $Mg = T$, HUYGENS finds the frequencies of circular and lateral oscillation to be the same[2]). [This is in fact true
6 in the greatest generality[3]).] Returning to (17), HUYGENS says that ex-

1) Œuvres complètes 18, 489—495.

2) HUYGENS says that the time of one complete vibration is twice as great in the circular case, but this is only because as in all early work the "time" of a lateral oscillation is the half-period.

3) For let the resultant outward force from all statical causes (other weights, tensions, *etc.*) be F; the equation of transverse motion for the mass M is then $F + F_{\mathrm{i}} = 0$, where F_{i} is the inertial force,

periments should be tried rather with a horizontal string and gives directions on how the experiment should be done; while he does not report any measured values, he says that (17) "agrees very well with experiments."

Finally HUYGENS considers the weightless string loaded by many weights (Figure 12). 7
He sketches the first steps of such a treatment, in which not only does he assume that "the curve *SAQC* is a parabola, from which it differs insensibly," but also he assumes a distribution of velocities not possible unless all masses are in simple harmonic oscillation at the same period and phase. The mechanical principle he applies is the conservation of energy[1]).

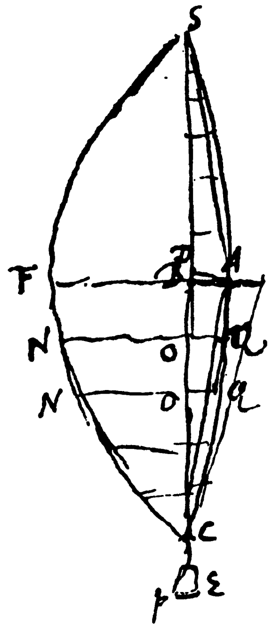

Figure 12.
HUYGENS' third model for the vibrating string (1675—1676)

d. The vibrating rod. In 1688—1689 HUYGENS[2]) considers vibrations of a bar resting upon two supports so placed as to minimize the "danger of rupture" according to his theory of breaking (above, pp. 46—47). He writes that a bar so supported gives the clearest sound when struck and that, in effect, these points of support remain at rest [*i. e.*, they are *nodes*]. His theoretical value for the fractional distance from the end to a support is $\frac{1}{2}(\sqrt{2}-1) \approx \frac{2}{10}+$; the chime makers, he says, use the value $\frac{2}{9}$, "which agrees well enough." [This is an example of experiment confirming a false theory. While the difference between $\frac{2}{9}$ and $\frac{2}{10}+$ might seem experimentally negligible, in fact for free vibrations of a rod the theoretical value (from the theory of DANIEL BERNOULLI and EULER, see below, pp. 198, 328) for the fractional distance to the node is $0{,}224 \approx \frac{2}{9}$. HUYGENS' theory, since it employs no dynamical principle and is merely a conjecture based upon a statical result itself precarious, is unsound, but it deserves notice for its *recognition of the nodes*[3]) of a vibrating body and the *first attempt to calculate anything concerning the vibrations of a rod.*

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and where F is the same in both problems considered. Let y be the transverse displacement. For transverse harmonic oscillations of circular frequency ω_h we have $F_1 = -M\ddot{y} = M\omega_h^2 y$. For circular oscillations at angular velocity ω_c, the centrifugal force is $F_1 = M\omega_c^2 y$. Hence $\omega_c = \omega_h$ [*i. e.*, each transverse frequency is also the frequency of a possible circular motion].

1) On pp. 494—495 of his Œuvres **18**, the editors carry through what they conjecture HUYGENS' ideas to have been. Their result is $\nu = \frac{\sqrt{10}}{\pi}\cdot\frac{1}{2l}\sqrt{\frac{T}{\sigma}}$, in the notation used in (17); this is close to the correct value (75) for the continuous string.

2) Œuvres complètes **19**, 74—75.

3) It is safe to presume that HUYGENS had read WALLIS' paper on the nodes of strings, published in 1677 (see § 16 below).

None of these brilliant studies of HUYGENS was published during his lifetime. Despite some measure of communication through letters and conversation, they remained unknown and do not seem to have influenced later work.]

7. PARDIES' essays on the catenary and on elastic beams (1673). In 1673 appeared the first general treatise on theoretical mechanics, an incomplete posthumous work by a man now forgotten even to historians of science, the Jesuit IGNACE-GASTON PARDIES[1]). In the preface PARDIES says he wishes to make "one body" of mechanics, and his description organizes well all aspects of the subject then investigated, but unfortunately he did not live to carry out all his promises[2]). While he appears to have performed many experiments, he always attempts mathematical proof; [here he fails almost invariably, for he seems insensible to the difference between proof and persuasion. The scorn bestowed upon his work by his great contemporaries is easy to understand, since this is the sort of book that, in a sense, ought never to have been written. With a show of the right facts and often even the right principles, little is done cleanly, yet the virginity of the subject has been defiled. As we shall see, while LEIBNIZ and the BERNOULLIS scarcely take note of PARDIES they had read his work and profited from it[3]).]

LXXIII At the beginning of his treatment of flexible bodies, PARDIES introduces the *continuous string* and applies all arguments to it without the intermediary of a discrete model. Like nearly all writers of the day, he uses infinitesimal constructions, [but he is a poor mathematician, unable to do better then guess at the results of what we now call differentiation

1) "*La statique ou les forces mouvantes,*" Paris, 1673, being the sequel to an earlier treatise on "local motion", mainly impact. I have seen this work only in the second edition, Paris, Mabre-Cramoisy, 1674, [xxiv] + 240 pp., in the third edition, *ibid.* 1688, [xxii] + 240 pp., and in PARDIES' Œuvres, Lyon, Bachelu, 1696, and second edition, 1709, where "La statique" occupies pp. 199–298, while its preface occurs among the unnumbered pages at the beginning of the volume. Also in Latin, Opera, Jena, 1693–1694, where this treatise occurs on pp. 87–211. There is also a third edition of PARDIES' Œuvres, La Haye, 1710.

2) In particular, the fifth discourse, which was to concern vibration, is lacking. PARDIES said he could prove from properties of the pendulum that the vibrations of a string are isochronous and that the frequency obeys the law $\nu \propto \sqrt{TA/l}$; this last is surely a misprint for (8), since PARDIES seemed to be generally well informed. However, the erroneous statement is repeated on p. 6045 of the English review quoted in the next footnote.

3) It was favorably reviewed in Phil. trans. London 8, No. 94, 6042—6046 (1673). After remarking that PARDIES was "cut off by an intimely Death; being regretted by those that knew his frankness and strong inclinations to promote philosophic knowledge," the anonymous reviewer continues, "Besides, the Author treats of Bodies suspended, fastned at one or both Ends; of the manner how they are broken; of the figure they take in becoming curve; and particularly of the Cases, where Cords extended will be Parabolical, Hyperbolical, Elliptical, or Circular. More-over, he examins the force of Towers and Pyramids, and shews in what part they are weakest; he determins the figure they ought to have to render them perfect and able every where to resist equally to the violence of Winds..."

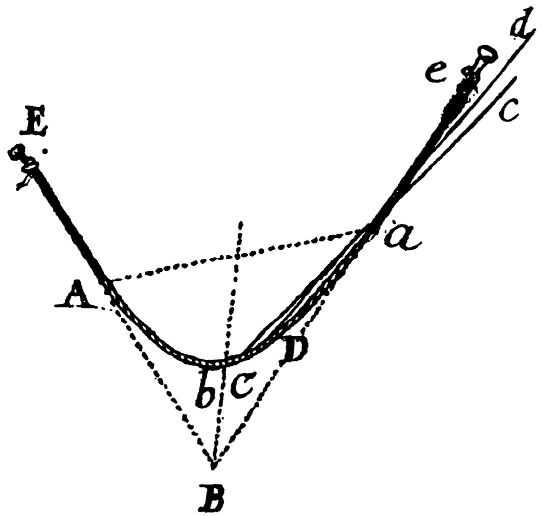

Figure 13. PARDIES' theorem (1673)

and integration.] PARDIES observes that the form of the string remains unchanged if we solidify any part, or, further, if we replace the parts above two points A and a, on each side, by *suitable forces acting along the tangents* at A and a. [This we recognize as the first occurrence of the *tension* in a curved flexible line; PARDIES does not calculate these forces, but in the concept we see the first of the two devices whereby JOHN BERNOULLI was to achieve his solution of the catenary problem (below, p. 74, especially Figure 25).] LXXIV
PARDIES' statical principle is [the continuous analogue of a generalization of the theorem of STEVIN mentioned above,] *viz, the point of intersection of any two tangents lies on the vertical through the center of gravity of the included portion of the cord* (Figure 13), *no matter what the line weight may be.* [Since some shadow of a correct proof is given[1]),] we may justly call the result *the theorem of* PARDIES. As we shall see, it forms the basis of LEIBNIZ's solution for the catenary[2]) (below, p. 71). This principle is particularly suited to solving all problems concerned with flexible lines subject to vertical load only, since, as was assumed tacitly by PARDIES and later writers, the fact that the supports can exert any desired tension makes it sufficient as well as necessary for equilibrium.]

PARDIES then asserts that the figure of the uniformly heavy cord is *not* a parabola. LXXV
"For one can imagine that the chain is now fixed at a and b (Figure 14); then this part aCb would remain in the same location as it was when attached freely at the ends a and A." [This is the second of the two devices to be used by JOHN BERNOULLI (below, Figure 26).] "Thus the center of gravity of the chain ab would be at C" [careless wording for "on the line DCE"]. "But if the figure aCb were parabolic, the line DCE would divide aF just in half, but the part aC of the parabola would be greater than Cb, and it is very easy to prove that the center of gravity of the parabola cannot be at C." [To replace "it is

1) Granted PARDIES' preceding statement, the result is obvious, since the weight of the segment is equipollent to a concentrated force acting at the center of gravity, and the lines of action of three equilibrated forces must intersect. We must not lose sight of the times we are describing: In the discrete case for two equal weights, HUYGENS had had trouble finding an adequate proof, and only years later did he obtain the generalization to unequal weights.

2) Also of the first correct published proof of that solution, *viz*, Prop. XVIII, Prob. XIII in the book of TAYLOR, *op. cit. infra* p. 86.

very easy to prove," we note that the required property of the center of gravity is $\frac{1}{2}x = \frac{1}{s}\int x\,ds$; equivalently, $s \propto x$, and this characterizes the straight line.]

LXXVI "But if we conceive a thread without weight, on which rest an infinity of equally heavy lines EC, ec, parallel and equally distant from each other, then the thread $aCbA$ will be perfectly parabolic." For then the center of gravity of the load acting on aCb lies on the line DCE bisecting aF, and "the geometers know" that the parabola is the only curve such that the tangents from A and from b intersect at a point upon this bisector.

[It is possible that PARDIES had heard[1]) of HUYGENS' results on these problems, but the line of thought is distinct from HUYGENS' and yields the simplest correct proofs ever obtained from that day to this.]

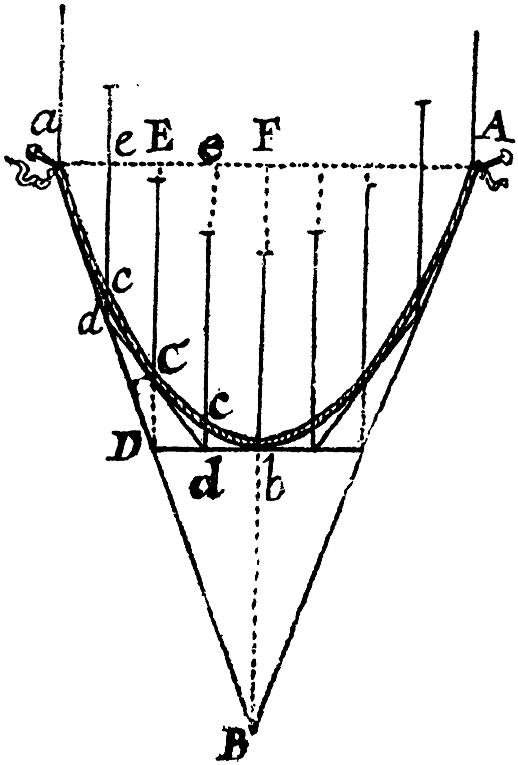

Figure 14. Drawing for PARDIES' arguments regarding the catenary and the suspension bridge (1673)

LXXVII If the string is *elastic*, says PARDIES, in order to assume a parabolic form it must be loaded by uniform forces directed toward a
LXXVIII fixed center; also, a *taut* elastic string always assumes an approximately parabolic form
LXXIX—LXXX in the small sagging due to its own weight. For such a string to be hyperbolic, it must be drawn by uniform forces directed toward a center below it; elliptic, toward a center above it. [For these results only the vaguest of reasons are given[2]).]

LXXXIV—CXIV PARDIES then considers the problems of breaking strength proposed by GALILEO, [but from a basically different standpoint. While GALILEO had considered the beam as rigid prior to rupture,] PARDIES attributes everything to *elasticity*. Indeed, he goes so far as
LXXXVII to try to reduce all phenomena of bending and even of compression to *extension*. For

1) Either through MERSENNE or from HUYGENS himself while he was in Paris. At the end of the treatise on statics, PARDIES gives a proof of the isochrony of motion on a cycloid, "so that after Mr. HUYGENS has published his proof, I can see if I have been fortunate enough to compete with so great a man." PARDIES' ingenious proof is valid and is distinct from that published by HUYGENS in the same year (above, pp. 47—48).

2) It is strange that the editors of HUYGENS' Œuvres 18, p. 487, cite this dubious material but give no hint of the solid ideas of PARDIES on the immediately preceding pages.

example, he claims that in compression of a beam the longitudinal fibres bulge outward and try to extend the annular fibres; from the resistance of these to extension arises the great compressive strength of beams, which can be further increased very notably by iron rings[1]).
As for the form of a beam built at one end and loaded by a weight at the other, "it is easy CIII
to prove" that it is a parabola. There follows a long list of specific rules regarding "the CIV—CVI
effort a body makes to break itself by its own weight". Then inclined beams are considered. CVII
Finally there is a long study of solids of equal resistance. CVIII—CXIV

[Thus to Pardies, and to him alone, belongs the credit of first attempting to introduce the elasticity of a beam into calculation of its resistance. His mathematical tools were far from sufficient to carry out his ambitious program of deriving results on the basis of his hypotheses. This is all the more evident in that he claims to calculate definite numerical proportions, yet he proposes no specific law connecting the extensions with the forces which produce them.]

8. Hooke's law of spring (1675, 1678) and researches on the arch (1675), on ropes (1669), and on sound (1675–1681). At the end of a work published in 1675 on another subject[2]), after a "Postscript" claiming priority for the "Spring to the Ballance of a Watch, for the regulating the motion thereof," against "some unhandsome proceedings" on the continent, Hooke wrote:

"To fill the vacancy of the ensuing page, I have here added a *decimate* of the *centesme* of the Inventions I intend to publish . . .

"3. *The true Theory of* Elasticity or Springiness, *and a particular Explication thereof in several Subjects in which it is to be found: And the way of computing the velocity of Bodies moved by them. c e i i i n o s s s t t u u . . .*

"9. *A new sort of* Philosophical-Scales, *of great use in Experimental Philosophy. c d e i i n n o o p s s s t t u u.*"

1) The ingenious qualitative arguments I have not tried to follow. The problem had been mentioned by Torricelli in his letter of 2 January 1643 to Ricci. Torricelli asserts that a ring sufficiently strong to prevent bulging at the center of a column in compression may be determined by the following rule, apparently empirical:

$$\frac{\text{Tension in ring}}{\text{Load on column}} = \frac{d}{l}\,.$$

Torricelli's letter to Ricci of 20 January 1643 suggests some analogy to the spreading of a crack in a wall.

2) R. Hooke, *A description of helioscopes, and some other instruments*, London, T. R. for John Martyn, 1676; reprinted, pp. 119—152 of R. T. Gunther, *Early science in Oxford* 8 (1931). The date 1676 is an error; on 15 October 1675 Oldenburg sent the printed work, including the "postscript", to Huygens; Oldenburg's review is printed in the Phil. trans. No. 118, 25 October 1675 = Œuvres complètes de Huygens 7, No. 2075.

[1] 331 Three years later he published a treatise on elasticity[1]), beginning : "The Theory of Springs, though attempted by divers eminent Mathematicians of this Age has hitherto not been Published by any. It is now about eighteen years since I first found it out, but designing to apply it to some particular use, I omitted the publishing thereof." The anagram in No. 3 deciphered reads : "*ut tensio sic vis;* That is, The Power of any Spring is in the same proportion with the Tension thereof . . . Now as the Theory is very short, so the way of trying it is very easie." With admirable clarity and directness, HOOKE describes his experiments, whose nature is made clear by Figure 15. Necessary experimental precautions and procedures are included.

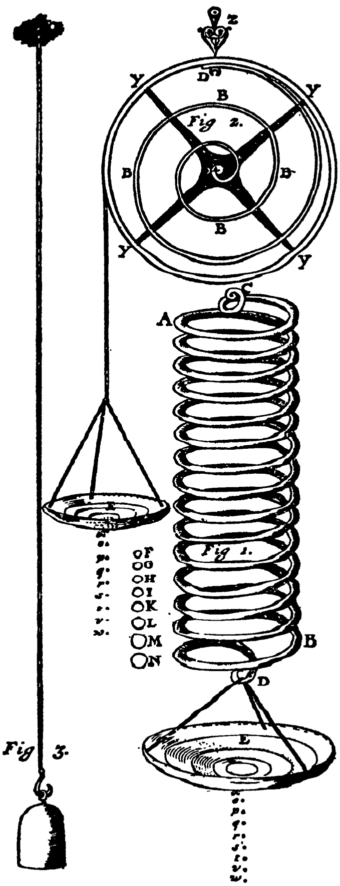

Figure 15.
HOOKE's experiments on extension (1678)

[3] 335 "The same will be found, if trial be made, with a piece of dry wood that will bend and return, if one end thereof be fixt in a horizontal posture, and to the other end be hanged weights to make it bend downwards." [*I. e.*, the elasticity of *bending* is also linear.] Corresponding experiments for the compression and rarefaction of air he published fourteen years ago. [Thus HOOKE's statement is

$$F \propto \Delta l\,, \qquad \begin{aligned} F &= \text{applied force,}\\ \Delta l &= \text{elongation or change in length.]}\end{aligned} \tag{18}$$

[4] 336 "From all which it is very evident that the Rule or Law of Nature in every springing body is, that the force or power

1) R. HOOKE, *Lectures de potentia restitutiva, or of spring explaining the power of springing bodies*, London, John Martyn, 1678; reprinted, pp. 331—388 of R. T. GUNTHER, *Early science in Oxford* 8 (1931). Page references are to the reprint.

According to records of the Royal Society published by GUNTHER, *Early science in Oxford* 6–7, Oxford, 1930, on January 27, 1663/4 HOOKE was ordered to perform experiments on springs in rarefied or condensed air. On February 3 he reported that no alteration in the elasticity was discernible in springs left in the open for some time. On December 17, 1668, HOOKE was "desired to bring in what he had considered of the cause of springiness."

The following entries in *The diary of* ROBERT HOOKE *M. A., M. D., F. R. S.* 1672–1680, ed. H. W. ROBINSON & W. ADAMS, London, Taylor & Francis, 1935, refer to elasticity:

September 2, 1675. "All springs at liberty bending equall spaces by equall increases of weight."

September 3, 1675. "Perfected Philosophicall Scales to show to the King."

September 21, 1675. "Dind with Sir Chr. WREN . . . Discoursd about Springs."

October 3, 1675. " . . . adjusted Demonstration of the equality of the motion of Springs."

October 6, 1675. "Walkd into the Park with Sir Chr. WREN. The King calld me to him, bid me thereof to restore itself to its natural position is always proportionate to the Distance or space it is removed therefrom, whether it be by rarefaction, or separation of its parts the one from the other, or by a Condensation, or crowding of those parts nearer together. Nor is it observable in these bodys only, but in all other springy bodies whatsoever, whether Metal, Wood, Stones, baked Earths, Hair, Silk, Bones, Sinews, Glass, and the like. Respect being had to the particular figures of the bodies bended, and the advantagious or disadvantagious ways of bending them." [While Hooke does not say explicitly that *the moduli of extension and contraction are the same,* this seems to be his opinion; in the case of air, the only material for which he says he has measured condensation, this is true.]

Conversely, the anagram in No. 9 is the law of the spring scale: "Ut pondus sic 337—338 [5—6]
tensio," affording an absolute rather than merely relative measure of the weights of bodies. With its aid, Hooke has sought to measure the variation of the earth's gravity with altitude, but on church towers and in deep mines no effect was discerned.

In terms of his views on the causes of elasticity, Hooke writes that "... it will be 347 [15]
very easie to explain the compound way of springing, that is, by flexure, supposing only two [elastic] lines joyned together as at $GHIK$ (Figure 16), which being ... bended into the form $LMNO$, LM will be extended, and NO will be diminished in proportion to the flexure, and consequently the same proportions and Rules for its endeavor or restoring it self will hold." [Thus Hooke remarks, as had Beeckman before him, that the outer fibres of a bent beam are stretched and the inner ones compressed. This "compound way of springing" is the main problem of elasticity for the century following, but Hooke gives no idea how to relate the curvature of one fibre to the bending moment, not to mention the reaction of the two fibres on one another.]

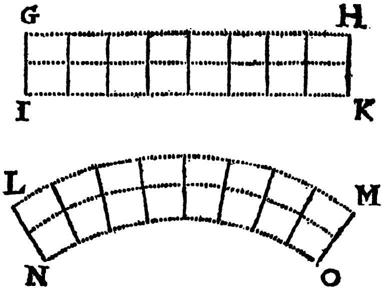

Figure 16. Hooke's drawing to show the extension and contraction of the fibres of a bent beam (1678)

"It now remains, that I shew ... the Vibrations of a Spring, or a Body moved by a 348 [16]

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shew him experiment. Followd him through tennis court garden &c. into closet. Shewd him the Experiment of Springs. He was very well pleasd. Desired a chair to weigh in."

According to records of the Royal Society published by Gunther, *loc. cit. ante,* at the meeting on August 1, 1678, Hooke showed his experiments on "a tubical spring of brass wire, and ... a spiral spring of steel ...," and on August 22 he demonstrated the law (18) with "a spring of brass wire, about thirty-six or thirty-seven feet long, extended by weights hung at the lower end thereof ..." Also, "about three years since his Majesty was pleased to see the experiment ..." The diary entries for these dates confirm these facts. Also, on August 20, 1678, "Met Sir Chr. Wren ..., discoursd about equation of Springs, etc.," and August 21, "To Sir Chr. Wren ... Discoursd much about Demonstration of spring motion ... I told him my philosophicall spring scales ..."

Spring, equally and uniformly shall be of equal duration whether
[17] 349 they be greater or less." To this end, HOOKE introduces "the aggregate of the powers of the spring" [*i. e.* the work done by it].
350—353 [18—21] To prove the isochrony, HOOKE gives two distinct arguments, [both fallacious 1)]. The error is now difficult to understand, since GALILEO had given the correct solution for the mathematically analogous problem of small oscillation of a pendulum. It must be remembered that problems of this kind were still extremely difficult; such analogies were not obvious, because it was not yet customary to think of motions as determined directly by assigned forces. We may conjecture that HOOKE observed the isochrony in his experiments and devised some sort of reasoning to conform to it.

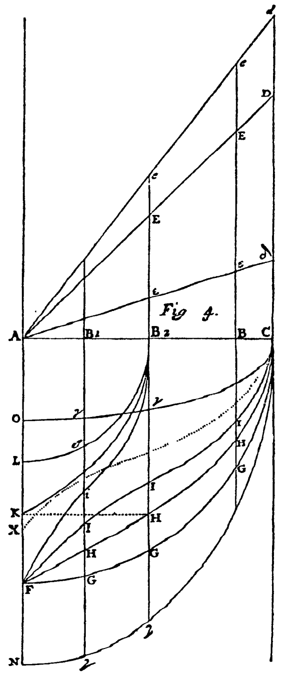

Figure 17.
HOOKE's incorrect results on the motion of a body attached to a spring (1678)

So far as I know, there is no other early treatment of simple harmonic motion in an elastic context. We have mentioned (above, pp. 47—48) the roundabout argument of HUYGENS to conclude the isochrony and calculate the period. To the modern reader of NEWTON's *Principia* (1687)[2]) it is abundantly clear that for NEWTON simple harmonic motion was a familiar and completely mastered concept. To the original readers[3]) of his book, however, it must have appeared rather different.] The results are stated as follows[4]) : "Supposing

1) The dynamical principle HOOKE uses to find the speed v of a mass M starting from rest is $v \propto \sqrt{W}$, which is correct since in fact

$$v = \sqrt{\frac{2}{M}\int_0^s F ds} = \sqrt{\frac{2W}{M}},$$

where W is the work done. However, HOOKE's first argument is based on the formula $s \propto \sqrt{W}$, which is correct only for motion starting at the equilibrium position of the spring, not from a point where $v = 0$. HOOKE's second argument, based on the correct formula for the work done by spring when released from rest at amplitude $\mathfrak{A}$, *viz.* $W = \frac{1}{2}K(2\mathfrak{A}s - s^2)$, obtains the correct formula $v^2 \propto 2\mathfrak{A}s - s^2$ for the speed, shown by the circle and the ellipses in Figure 17. Both arguments assume $t = s/v$ rather than the correct kinematical formula $t = \int ds/v$. It would seem that the resulting "S-like Line of times" CIIIF in HOOKE's figure would have aroused his physical intuition, since it has a point of inflection, implying that the velocity first increases and then decreases in each quarter period.

2) *Philosophiae naturalis principia mathematica*, London, 1687. There are many reprints and translations. Our references are to the first edition, with variants in later editions noted in parentheses.

3) As is shown below, p. 61, LEIBNIZ failed to see in NEWTON's book anything concerning the vibrations of springs. The very brief mention of sonorous vibrations of solid bodies in the Scholium after Prop. L, Probl. XIII (in later eds., Probl. XII) of Lib. II adds nothing.

4) Lib. I. Prop. XXXVIII, Theor. XII, p. 121.

that the centripetal force be proportional to the altitude or distance of the places from the center, I say that the times of falling, the speeds, and the spaces traversed are as the arcs, the versed sines, and the sines respectively." For proof we are told only to use Proposition X in the same way that Proposition XXXII was proved from Proposition XI. This means that we are to pass to the limit in results already derived for motion on an ellipse. [This oblique and scarcely illuminating approach to a problem which now seems fundamental reflects NEWTON's concentration on celestial mechanics.] In his proof of the isochrony of a cycloidal pendulum and his discussion of a simple pendulum[1]), NEWTON is content to show that the restoring force is proportional to the arc; everything then follows from the above. [What a modern reader would consider a straightforward treatment of simple harmonic motion, based on the differential equation $M\ddot{x} = -Kx$, seems first to have been given many years later by JOHN BERNOULLI (see p. 134, below).]

Returning to the "decimate of the centesme" published in 1675, we read as No. 2, "*The true Mathematical and Mechanichal form of all manner of* Arches *for Building, with the true butment necessary to each of them.* A problem which no *Architectonick* Writer hath ever yet attempted, much less performed." The anagram, when deciphered[2]), reads "Ut pendet continuum flexile, sic stabit contiguum rigidum inversum," *i. e., as hangs the flexible line, so but inverted will stand the rigid arch.* [While none of the available papers of HOOKE reveals how he reached this conclusion, there is no reason to doubt that he had sufficient mastery of statics to show that an arch of infinitely small stones in order to exert purely tangential thrust should be formed like an inverted catenary subject to inverted loads. Thus the problems of the catenary and the arch are reduced to one, but neither is solved.]

According to records of the Royal Society[3]), on July 8, 1669, "Mr. HOOKE proposed an experiment about the strength of twisted cords, compared with untwisted ones, to be tried at the next meeting . . ." On July 15 "Mr. HOOKE made an experiment of comparing together the strength of twisted and untwisted silk, and it appeared by the several trials

1) Lib. I, Prop. LI, Theor. XVIII and Prop. LII, Probl. XXXIV, pp. 151–153 (note the important corollary added to Prop. LI in the 2nd. ed.).

2) The solution seems first to have been published by RICHARD WALLER in his introduction to the *Posthumous Works of ROBERT HOOKE, M. D., S. R. S.*, 1705, included among other writings about HOOKE printed by GUNTHER, *op. cit. ante*, p. 54, 5, 1—68; see p. XXI of the original or p. 51 of the reprint. In HOOKE's diary as published by GUNTHER in the same volume, the arch is mentioned in the entries for December 8 and 15, 1670, for January 12 and 19, 1670/1, and for December 14, 1671; HOOKE demonstrated something to the Society but disclosed the proof of it only to the president. In HOOKE's later diary, cited above, p. 54, the entry for June 5, 1675, mentions "my principle about arches", and on September 26, "Riddle of arch, of *pendet continuum flexile, sic stabit grund Rigidum.*" Doubtless there is an error of transcription.

3) GUNTHER, *op. cit. ante*, p. 54.

made of it, that a certain number of threads untwisted proved stronger than so many twisted. Whence Mr. HOOKE concluded, that cables made faggot-wise would be stronger than when twisted.

"To this it was objected, that cables would not then be so manageable; and that certainly people had not been wanting to make trials of this nature, but had doubtless found, that, all things compared, the inconvenience would prove greater in the use of untwisted than twisted threads." [The "inconvenience" depends on the use. It is precisely HOOKE's "cables made faggot-wise" that PHILON had found superior in use on ballistae many centuries before (above, p. 17).]

"Mr. HOOKE remarked upon this, that the belief of the superior strength of twisted threads to that of untwisted had doubtless proceeded from trials made upon flax, which having but short pieces held not therefore so well untwisted as twisted." [GALILEO had explained the apparent strength of ropes[1]) but had not stated any definite relation between the total and partial strengths. HOOKE's result is to be rediscovered in 1711 by DE RÉAUMUR[2]).]

HOOKE was also a leading proponent of some of the now accepted ideas regarding sound, [but he made no advance beyond BEECKMAN and MERSENNE]. He devised an experiment for producing sound by toothed wheels, [but exactly what he did is hard to ascertain[3])].

1) Pp. 55—58 of *op. cit. ante*, p. 54.

2) "*Experiences pour connoistre si la force des cordes surpasse la somme des forces des fils qui composent ces mesmes cordes*," Mém. acad. sci. Paris 1711, [2nd.] 4to ed., Paris, 6—16 (1730). DE RÉAUMUR reports a sequence of experiments ending with one on a silk cord composed of 832 fibres.

3) In HOOKE's diary, cited above, p. 54, in the entry for January 15, 1675/6, we read, "To Sir Chr. WRENS, Dr. HOLDER and I discoursd of musick, he read my notes and saw my designs, then he read his which was more imperfect. I told him but *sub sigillo* my notion of sound, that it was nothing but strokes within a Determinate degree of velocity. I told them how I would make all tunes [*i. e.* tones] by strokes of a hammer. Shewed them a knife, a camlet coat, a silk lining. Told them that there was no vibration in a puls of sound, that twas a puls propagated forward, that the sound in all bodys was the striking of the parts one against the other and not the vibration of the whole. Told them my experiment of the vibrations of a magicall string without sound by symphony that touching of it which made the internall parts vibrate—caused the sound, that the vibrations of a string were not Isocrone but that the vibration of the particals was. Discoursd about the breaking of the air in pipes, of the musick of scraping trenchers, how the bow makes the fidle string sound, how scraping of metall, the scraping the teeth of a comb, the turning of a watch wheel &c., made sound." *Cf.* also the entry for January 8.

The records of the Royal Society, as published by GUNTHER, *op. cit. ante*, inform us that on July 27, 1681, HOOKE "showed an experiment of making musical and other sounds by the help of teeth of brass wheels; which teeth were made of equal bigness for musical sounds, but of unequal for vocal sounds." On p. xxiii of the original edition of WALLER's life of HOOKE, p. 57 of GUNTHER's reprint, it is stated that in July of 1681 HOOKE "shew'd a way of making *Musical and other Sounds*, by the striking of the Teeth of several Brass Wheels, proportionally cut as to their numbers, and turned very fast

9. Mariotte and Leibniz on elastic beams (1684). [Before taking up the more important work of Leibniz to which it apparently gave rise, we must mention the attempt of Mariotte, especially since he is one of those writers who, for some unaccountable reason, has been read and cited often.] The second discourse of Part V of Mariotte's *Treatise on the motion of water and other fluid bodies*[1]), published two years after his death in 1684, concerns "the force of pipes of conduct, and the thickness which they ought to have, according to their matter, and the height of the reservatories." [This seems to be the first treatise on the experimental strength of materials; it describes many intelligent experi-
ments carried out with some care.] Mariotte says that his tests on wood and glass do not 372—373
conform to Galileo's proposition (11); instead of the factor $\frac{1}{2}$, he finds a value between
$\frac{1}{3}$ and $\frac{1}{4}$. He undertakes to derive a better result by starting from the assumption that the 373—374
"Fibres and Ramous Particles" of a body "may be extended more or less by different Weights: And, *Lastly,* That there is a Degree of Extension which they can't bear without breaking." [Thus Mariotte, like Pardies, considers the *deformation* of a beam prior to rupture; his criterion for failure is the *magnitude of the elongation.*]

As a model, Mariotte proposes a rigid lever tied down by little strings which break 375
when they suffer a certain elongation (Figure 18). [His reasoning is incomprehensible; 377—379

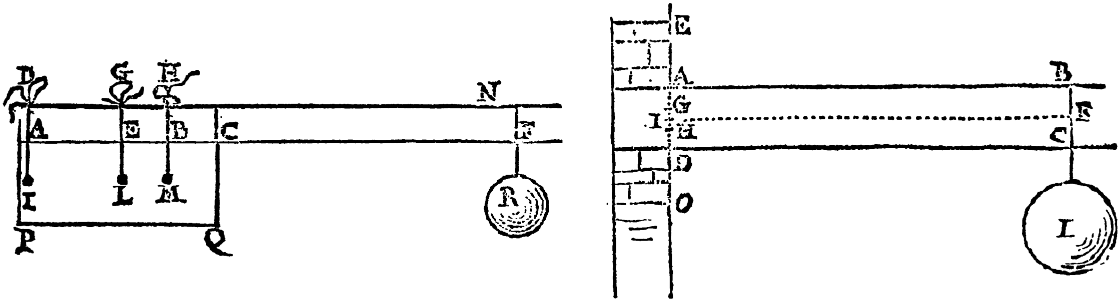

Figure 18. Mariotte's figures supposedly representing the forces in a terminally loaded beam (1684)

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round, in which it was observable, that the equal or proportional stroaks of the Teeth, that is, 2 to 1, 4 to 3, &c, made the musical notes, but the unequal stroaks of the Teeth more answer'd the sound of the Voice in speaking."

1) *Traité du mouvement des eaux et des autres corps fluides*, [xiv] + 408 + [xx] pp., Paris, Estienne Michallet, 1686. The date of the permit is 4 July 1685. This posthumous work is edited by de la Hire; particularly the last parts were not in order. Our page references are to the first edition. There is a "New corrected edition," xii + 390 + xiv pp., Paris, Jean Jombert, 1700. A new edition, "corrected and augmented by rules for fountains," same publisher, 1718, xii + 414 + xiii pp. is reset but seems to carry no changes in the part described above; a reprint from the Paris memoirs of 1693 is added. In the Œuvres of Mariotte, 2 vols. paginated as one, xii + 701 + xxxiii pp., Leiden, Pierre Vander Aa, 1717, the *Traité* occupies pp. 321—476. Our quotations are taken from the English translation by J. T. Desaguliers, *The motion of water, and other fluids, being a treatise of hydrostaticks,* London, J. Senex, 1718, xxiv + 290 pp.

apparently the fact that the little *transverse* strings are stretched in proportion to their distance from C in the first figure is intended to justify the assumption that] the [longitudinal?] fibres "resist in Proportion to their Distance from the Point D" in the second figure. By some mysterious juggling[1]) with a numerical progression appropriate to a special case, MARIOTTE concludes that $\frac{1}{3}$ should replace $\frac{1}{2}$ in (11).

379—380 In a further discussion MARIOTTE says "you may conceive that from D to I, which is half the Thickness AD, the Parts are pressed together by the Weight L; those that are near D, more than those toward I; and that they are extended from I to A, as has been before explain'd; and the same Reasoning about the little Cords may be applied to the Part I A . . . and it is very probable that these Compressions resist as much as the Extensions . . . whence will follow the same thing as if all the Parts were extended . . ." [It is still not clear whether transverse or longitudinal fibres are intended. In the traditional interpretation[2]) of MARIOTTE's work, it is the latter; if so, then MARIOTTE implies but does not state that there is an unextended or *neutral fibre* within the beam and infers that assuming the central fibre to be the neutral one yields the same resistance to bending as when the lowest fibre is neutral. This is false. Nevertheless, MARIOTTE's dubious or false calculation may be considered as some advance beyond the clearer though unsupported statement of HOOKE (above, p. 55)].

380—383 MARIOTTE's experiments show that in fact all materials, even glass, deform before breaking; moreover, a glass rod returns to its original length when the stretching weight
384—388 is removed. Several of GALILEO's assertions resting on the assumption that a given moment,
388 however applied, suffices to break a body, are verified by MARIOTTE's experiments. "These Rules are of use for brittle solids, as dry Wood, Glass, Marble, Steel, etc. But for supple and pliable Substances, that are broken by Traction alone; as Paper, Tin, Ropes, etc. other Rules are necessary . . ." *E. g.* "lists [*i. e.* bands] of Paper, Tin, and such kinds of Bodies
390—391 break equally whether they be long or short." An experiment with a spiral spring not only verifies [HOOKE's law of] proportionality between elongation and stretching weight but shows also that this rule applies as well to a part of the spring as to the whole of it.
394—400 For the rupture of vessels under water pressure, MARIOTTE asserts that the breaking strength is proportional to the thickness of the walls.

[From the remarks in the paper to be discussed now, it is plain that LEIBNIZ knew of MARIOTTE's work before it was published.] LEIBNIZ is the first to attain a mathematical theory taking account of the elastic tension of the fibres of a beam. His *New proofs concerning the resistance of solids*[3]) begins by considering a cubical beam, for which GALILEO's

1) PARENT, § 17 of *op. cit. infra*, footnote 1, p. 111, finds "an error of geometry" here.
2) Deriving from VARIGNON and BÜLFFINGER, *op. cit. infra*, pp. 102, 103.
3) "*Demonstrationes novae de resistentia solidorum,*" Acta erudit. Leipzig, July 1684, 319—325 =

formula (11) yields $P_b = \frac{1}{2}P_t$. LEIBNIZ interprets GALILEO as supposing the resistance [*i. e.* moment] of the fibres varies linearly with their height above the lower edge. Integration over a square base then indeed yields the factor $\frac{1}{2}$. But, says LEIBNIZ, experiments show that $P_b < \frac{1}{2}P_t$. GALILEO's reasoning is correct, but his hypothesis is false. "The cause of this can be nothing else than that he considered the beam as perfectly rigid, so as to break off entirely in one moment at the place where its resistance is exceeded, while in fact all bodies . . . give way considerably before they can be ruptured." This was observed by MARIOTTE, who by "an ingenious calculation" concluded that[1] $P_b = \frac{1}{4}P_t$, "but as soon thereafter as I found leisure to search the matter more deeply and to subject it to the laws of the geometers, I found the true proportions . . ."

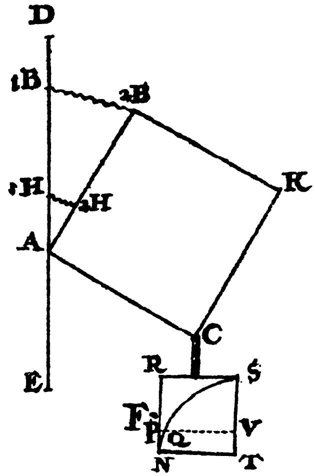

Figure 19. LEIBNIZ's figure for calculating the bending moment acting on the cross-section of a terminally loaded beam (1684)

To consider the elasticity of a beam, LEIBNIZ supposes each fibre acts as a spring (Figure 19) connecting the beam to the wall. "From the hypothesis elsewhere substantiated, that the extensions are proportional to the stretching forces[2])," he concludes that the resistances [moments] of the fibres are as the squares of their distances from the lower edge, since (a) the weights required for stretching a given amount are pro-

LEIBNIZens math. Schriften 6, 106—112. The account of this work given by PEARSON, § 11 of *op. cit.*, p. 11, so little squares with the contents that I am tempted to conjecture he saw some other version of it.

1) While MARIOTTE's work is obscure, the result he seems to conclude by his *theory*, if such it may be called, is $P_b = \frac{1}{3}P_t$. See above, p. 60. Since the publication of MARIOTTE's work is subsequent to LEIBNIZ's, LEIBNIZ may be citing an earlier version, or he may be citing from memory. *Cf.* the criticism of BÜLFFINGER, § 11 of *op. cit. infra*, p. 103.

2) Since he cites no source for the linear elastic law, later Continental writers often named it after him. It is plain, however, that LEIBNIZ considered the linear relation neither as his own nor as important.

An attitude very different from that often attributed to LEIBNIZ is revealed by his long correspondence with HUYGENS concerning the experimental laws of elasticity. In October 1690 LEIBNIZ writes: "I am not yet entirely content with the elastic laws which are given out, since it seems that experiment does not sufficiently agree with the rule that the extensions of strings (for example) are as the stretching forces. For this reason I should like to know your opinion." 2 March 1691: "Mr. NEWTON has not discussed the laws of spring; it seems to me that I have heard you say formerly that you had examined them and that you had proved the isochrony of the vibrations." Also: "I prefer a LEEUWENHOEK who tells me what he sees to a CARTESian who tells me what he thinks. But it is necessary to join reasoning to observation." HUYGENS replied on 26 March 1691: "I have a proof of the isochrony of the vibrations of a spring, supposing that it yields in proportion to the force that presses it, as experience shows constantly." The preliminary note for this letter adds: "HOOKE has discussed it portional to the distance from the fulcrum A, and (b) the extension varies as the force. [We should now say that since the force varies as the extension, and the extension of the fibres at a cross-section pivoted about the fulcrum varies linearly with the distance, the moments about the fulcrum vary as the square of the distance.] Integration yields $\int_0^1 y^2 dy = \frac{1}{3}$, so

$$P_b = \frac{1}{3} P_t \tag{19}$$

(for a cube). [While LEIBNIZ is somewhat hard to follow, we see he is not only the first to apply HOOKE's law in a correct calculation of the equilibrium of moments but also the first to obtain, in a special but typical case, the celebrated formula

$$\mathcal{M} \propto I \qquad \begin{aligned} \mathcal{M} &= \text{Bending Moment}, \\ I &= \text{Geometrical Moment of Inertia of the Cross-Section}. \end{aligned} \tag{20}$$

This, indeed, is the product to be expected from the first application of calculus to the theory of continuous bodies.

For understanding of later developments, we may describe LEIBNIZ's procedure as *taking account of the elastic tension* of the fibres while *neglecting the bending* which accompanies the tension[1]). GALILEO, it may be recalled, had neglected *both* the deformation and the variation of tension to which it gives rise. Since very large forces produce very small deflections in bodies used for structural ends, LEIBNIZ's approach is natural, though of course later experience will reveal it to be insufficient.]

LEIBNIZ finds that according to his theory, as according to GALILEO's, the cylindrical solid of equal resistance to end load is parabolic; to uniformly distributed load, linear. He attacks the problem of a beam of arbitrary cross-section and gives geometrical construction for its resistance. He asserts that the surface of revolution forming a solid of equal resistance is a paraboloid. For most of these propositions he gives no proofs, but he observes "that

fallaciously." LEIBNIZ on 20 April 1691: "In England they have published a little book on springs, I believe by Mr. HOOK[E], but it seems to me I found something wrong in it. I beg you to tell me the experiments you say you have made on this subject." On 5 May 1691 HUYGENS replied: "I have seen earlier the treatise of HOOKE on the spring, and I noticed a paralogism in it, which I could find among my papers." No such paper has been found. HUYGENS agrees with HOOKE's result, but only for slight extension. "But in the spring of air the proportion is always perfect, for which there are experiments in the books of Mr. BOYLE." For HOOKE's error, see above, p. 56. The nearest approach to a statement and proof that has been found in HUYGENS' manuscripts is described above, p. 61.

The foregoing exchange makes it plain that in 1691, *after* the dissemination of calculus and *after* the publication of NEWTON's *Principia*, simple harmonic motion was not thoroughly understood even by the foremost scientists.

I have been unable to find any early correct proof of isochrony referred to an elastic context.

Later views of LEIBNIZ on elasticity are given below, pp. 96, 127—128.

1) That LEIBNIZ fully understood what he was doing is shown by his letter quoted below, p. 64.

by these few considerations all this matter may be reduced to pure geometry, which in physics and mechanics is uniquely to be desired."

LEIBNIZ states also, [contrary to MERSENNE's inference from his experiments,] that *the elastic and acoustic properties of bodies are connected.* "And that there is nothing so rigid but that it is bent a little by the lightest stroke follows from the nature of sound, which is a certain trembling or reciprocal bending of the parts of the sounding body. The more rigid and indiscernible is the restitution, the higher is the sound, since the tremulous parts are the shorter and the tenser, and they constitute the harder body[1])."

[This paper establishes LEIBNIZ as the father of the mathematical theory of elasticity.

It had also a second great function in our subject; not only did it excite JAMES BERNOULLI to the study of elasticity but also it was the means that drew him into the higher analysis.] His first letter[2]) to LEIBNIZ, dated 15 December 1687, relates that an expert mechanic of Basel had consulted him regarding the construction of wagons; in LEIBNIZ's paper BERNOULLI had sought and found help. However, he decided to test by experiment LEIBNIZ's hypothesis that the elongations are proportional to the stretching weights. The results of BERNOULLI's experiments on a gut string do not conform to this hypothesis at all. But LEIBNIZ has written that the experiments of others support the linear law. What is the reason for the discrepancy? Was BERNOULLI insufficiently careful? Or are the fibres of which LEIBNIZ considers hard bodies to be composed different from such a string?

But there is another trouble. LEIBNIZ's assumptions imply that the beam is broken or bent at the wall, while the said mechanic asserts that for iron bars the bending (which seems to be nothing else than an incipient break) takes place mainly in the part one third to one half the distance from the built-in end to the free end.

This letter LEIBNIZ, absent on a long journey, received only after a delay of three years. On 24 September 1690 he replies, in effect, that *the relation between extension and stretching force should be determined by experiment;* in particular, the table of values BERNOULLI had sent to him seems to fit a hyperbolic curve. The ratio P_b/P_t, says LEIBNIZ, will be altered if the assumed relation between force and extension is altered. But the dependence of P_b and P_t upon the dimensions of [similar] cross-sections, as he proceeds to show by what would now be called a dimensional argument, is unaltered, and thus in particular his results concerning the solid of equal resistance remain valid[3]).

1) In symbols, ν is an increasing function of E, where E is an elastic modulus. This statement of LEIBNIZ foreshadows the correct and general law $\nu \propto \sqrt{E}$.

2) All letters between LEIBNIZ and the brothers JAMES and JOHN BERNOULLI are cited from LEIBNIZens mathematische Schriften **3**.

3) In an undated letter to v. BODENHAUSEN, reprinted in LEIBNIZens math. Schriften **7**, 356, LEIBNIZ mentions having sent this proof to BERNOULLI. "I have also explained to him what the figure

LEIBNIZ is indeed aware of the bending undergone by a beam prior to its failure. "But in my reasoning I preferred not to consider the bending of the whole beam, or rather I assumed a shape already reduced, through the prior bending caused by the weight, to the [straight] form we attributed to it . . ." [In this latter explanation we first encounter a view that has recently proved most useful in problems of finite deflection : The forces are referred to the actual, deformed condition of the body.] "However, consideration of the bending would furnish a new and by no means inelegant problem."

[JAMES BERNOULLI was not quite ready for this not inelegant problem.] In the years between query and answer, he had pondered and fathomed the LEIBNIZian calculus and had proved his mastery by his own researches, published in the *Acta Eruditorum*, the very journal to which LEIBNIZ had consigned his few enigmatic abstracts of the differential algorithm. Indeed, BERNOULLI had gone further. Four months before receiving the long-delayed letter to which the above is a reply, LEIBNIZ had read in the *Acta* a challenge JAMES BERNOULLI directed to the learned world, but certainly by implication especially to him : *to find the catenary curve*. LEIBNIZ now answers, " . . . I think I can satisfy you regarding the catenary curve as well." In fact he had answered two months earlier, also before receiving BERNOULLI's letter—answered in print. We now step backward four months in this history to follow from the start the discovery of the catenary.

10. The contest to find the catenary (1690). In the *Acta Eruditorum* for May 1690, at the end of a paper on another subject[1]), JAMES BERNOULLI writes, "And now let this problem be proposed : *To find the curve assumed by a loose string hung freely from two fixed points*. I assume also that the string is a line which is easily flexible in all its parts." So begins the great contest to find the catenary.

LEIBNIZ is quick to reply[2]). In the July issue, after restating the problem, he remarks : "It is supposed also that the string remains of the same length, like a chain, rather than being stretched or contracted like a wire. This problem, proposed by GALILEO and famous since his time, has not yet yielded to solution . . . Therefore I should rightly be excused from the burden imposed, especially since I am much drawn into other matters. But the humanity of that most enlightened man is such that I should not wish to fail of his first

of equal resistance must be when the beam is loaded not only by its own weight but also by a foreign weight . . ., which I omitted in my paper, and which he could scarcely find, since it involves the higher analysis."

1) "*J. B. Analysis problematis antehac propositi, de inventione lineae descensus a corpore gravi percurrendae uniformiter, sic ut temporibus aequalibus aequales altitudines emetiantur : et alterius cujusdam Problematis Propositio*," Acta Erud. Leipzig, May 1690, 217—219 = Opera omnia 1, 421—424.

2) "*G. G. L. ad ea, quae vir clarissimus J. B. in mense Majo nupero in his Actis publicavit, responsio*," Acta erud. Leipzig, July 1690, 358—360. Not reprinted in LEIBNIZens math. Schriften.

summons. Therefore I have attacked [the problem], which I had hitherto not attempted, and with my key [*i. e.* differential calculus] happily opened its secret approaches.

"However, this problem is a little more involved than my former one and displays a certain singular use of our method; thus I have thought it worthwhile, before publishing my solution, to give time also to others for exercising their skill. By this as by the Lydian stone we shall know the best methods; which bears much on the improvement of the science; especially since here it is not a matter of elaborate calculation, but rather of artifice. First of all the most noble D. T. [Count TSCHIRNHAUSEN], who promises splendid things of this kind, is to be asked whether he wishes to try the strength of his method here too [1]). But if no one indicates *before the end of the year* that he has found a solution, I will give mine, God willing."

On 9 October 1690 HUYGENS writes to LEIBNIZ, "But to judge better of . . . your algorithm, I await with impatience . . . what you have found regarding the line of the string or hanging chain, which Mr. BERNOULLI has proposed for you to find, for which I am grateful to him, since this line includes singular and remarkable properties. I had considered it formerly, in my youth, when I was but fifteen [*recte* seventeen], and I had proved to Father MERSENNE that it is not a parabola, and had found what the pressure should be in order for it to be a parabola. [See § 6, above.] This has caused me to be tempted now to examine the problem, and here is the cipher of what I have found. I have written it in such a way that you can interpret it somewhat if you have made the same discoveries, and I think to give you more pleasure thus, than if I were to send you everything explained. I beg you to send me your cipher in return, and let us shorten between ourselves the term of a year that you have allowed to the geometers . . ." The cipher follows. On 13 October LEIBNIZ replies, " . . . I find some relation to my calculation, but also some difference"; the difference is one of sign, [and it is plain that LEIBNIZ has unravelled the cipher]. In his letter of 18 November, HUYGENS again requests LEIBNIZ's cipher; for the curve he proposes the name *catenary* [already used by LEIBNIZ].

On 23 February 1691 HUYGENS again demands LEIBNIZ's cipher. On 2 March LEIBNIZ replies that Mr. BERNOULLI also has found the solution. "I think that knowledge of my calculus helped him a little, for although this problem is not one of the most difficult, I suspect it is not too easy to solve without something equivalent to that calculus. I have not seen his solution, but I do not doubt he has succeeded. Mr. TSCHIRNHAUSEN has not bitten . . ." On 26 March and 21 April HUYGENS again demands LEIBNIZ's cipher and BERNOULLI's as well. Finally on 5 May HUYGENS sends his solution, sealed, to LEIBNIZ, to be transmitted to the *Acta* for publication. On 27 May LEIBNIZ replies that he had sent in HUYGENS' solution and his own at the same time.

1) To this challenge TSCHIRNHAUSEN did not reply.

The *Acta Eruditorum* for June 1691 printed not only the solutions of LEIBNIZ and HUYGENS but also one by a new protagonist from whom we are to hear much more, JAMES BERNOULLI's younger brother JOHN, then 24 years old[1]). As the editor explains in a notice titled *Solutions of the problem proposed by J. B.*[2]), "The benevolent reader will have no trouble in remembering the problem proposed by the most enlightened Professor JAMES B. . . . of Basel . . . The most celebrated G. G. L. promised to publish a solution obtained by his method, if no one also had solved it by the end of the year . . . But in fact the brother of the proposer, Mr. JOHN BERNOULLI, candidate in medicine and much versed in these studies, solved it and sent us his solution last December; and through his brother he most kindly required us to add it to that of LEIBNIZ, in its time. Thence it has happened that we have urged the most celebrated man above-mentioned to publish his solution . . . Also Lord CHRISTIAAN HUYGENS has deigned . . . to ornament this our Journal with his solution of the problem. Therefore we shall give you, benevolent reader, the two solutions of these illustrious peers and that of BERNOULLI, but in the order in which they reached our hands."

[For 1690, these three solutions, in the order received, exhibit the mathematics of the future, the present, and the past; therefore we discuss them here in reverse order.]

The note of HUYGENS[3]) gives "only the solutions . . . for special cases, in a desire to avoid prolixity, and since I do not doubt that the learned will sufficiently exhibit the general rules. And if anything further of ours is wished, I will freely send it." [Indeed, it is incomprehensible.] Only special points, often with numerical values, are considered. HUYGENS asserts that the catenary can be constructed by means of the quadrature of either of a certain pair of quartics but does not explain further. The only statement of principle contained seems to be equivalent to $\frac{x}{s} = f\left(\frac{dx}{dy}\right)$, where s is arc length and x and y are rectangular co-ordinates, [but this is not correct]. A little later[4]), however, HUYGENS published something more specific: "it is easy to prove" that the slopes of the segments of a weightless chain with links of uniform length, uniformly

1) Thus our subject includes the problem by whose solution JOHN BERNOULLI established himself, overnight, as the peer of HUYGENS and LEIBNIZ. It was this solution, as Professor SPIESS remarks, that served the young giant as a passport to enter the learned society of Paris in 1691. See p. 136 of *Der Briefwechsel von JOHANN BERNOULLI*, Basel, 1955, where part of JOHN BERNOULLI's autobiographical letter to DE MONTMORT of 21 May 1718 is quoted.

2) Acta erud. June 1691, p. 273.

3) "CHRISTIANI HUGENII, *dynastae in Zülechem, solutio ejusdem problematis*," Acta erud. June 1691, 281—282 = Œuvres complètes 10, 95—98 = LEIBNIZens math. Schriften 5, 251—252.

4) Letter of February 1693 to BASNAGE DE BEAUVAL, Hist. des Ouvrages des Sçavans, Number for Dec. 1692 and Jan.—Feb. 1693, 244—257 = Œuvres complètes 10, No. 2793.

weighted at the junctions, *increase in arithmetic progression*[1]). In the limit as the lengths of the segments approach zero, this becomes $\dfrac{dy}{dx} \propto s$, [which we recognize as the correct

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1) While not attempting to follow HUYGENS' intricate argument, I append here a simple treatment along the lines introduced a half century later by EULER in connection with problems of motion (§ 30, below). With notations as in Figure 20, equilibrium of horizontal and vertical forces acting at the point (x_k, y_k), where W_k is attached, yields

$$T_{k+1} \sin \theta_{k+1} - T_k \sin \theta_k = W_k$$
$$T_{k+1} \cos \theta_{k+1} - T_k \cos \theta_k = 0 \; .$$

Hence

$$T_k \cos \theta_k = \frac{W_k}{\tan \theta_{k+1} - \tan \theta_k} \; ,$$

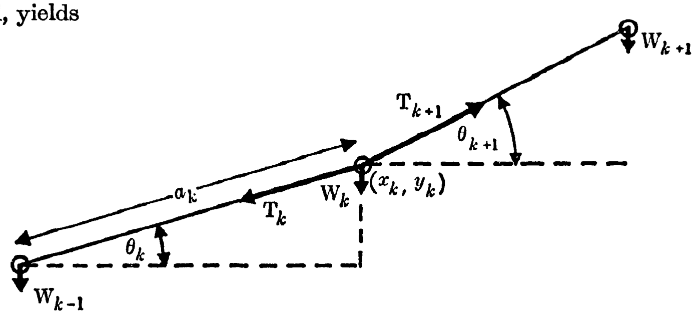

Figure 20. Sketch for modern proof of HUYGENS' theorem

so that

$$\frac{W_{k+1}}{\tan \theta_{k+2} - \tan \theta_{k+1}} = \frac{W_k}{\tan \theta_{k+1} - \tan \theta_k} \; .$$

When $W_{k+1} = W_k = W$ for all k, it follows that $\tan \theta_{k+2} - 2 \tan \theta_{k+1} + \tan \theta_k = 0$. This yields HUYGENS' theorem:

(H) $$\tan \theta_k = Ak + B \; ,$$

which is thus seen to follow from statics alone as a statement that the weights, however they be spaced, are equal.

The geometrical constraints are

(C)
$$x_k - x_{k-1} = b_k = a_k \frac{1}{\sqrt{1 + \tan^2 \theta_k}}$$
$$y_k - y_{k-1} = b_k \tan \theta_k = a_k \frac{\tan \theta_k}{\sqrt{1 + \tan^2 \theta_k}} \; .$$

If $b_k = b$ for all k, then from $(\mathrm{C})_{1,3}$ and (H) follows $x_k - x_{k-1} = b$, $y_k - y_{k-1} = b(Ak + B)$, so that

$$x_k = bk + x_0 \; ,$$
$$y_k = b\,[\tfrac{1}{2}A(k^2 + k) + Bk] + y_0 \; ;$$

therefore the points (x_k, y_k) lie upon a parabola. This is the solution of the suspension bridge problem. If $a_k = a$ for all k, then from $(\mathrm{C})_{2,4}$ and (H) follows

$$x_k - x_{k-1} = a \frac{1}{\sqrt{1 + (Ak + B)^2}} \; ,$$
$$y_k - y_{k-1} = a \frac{Ak + B}{\sqrt{1 + (Ak + B)^2}} \; .$$

HUYGENS' problem is equivalent to summing these difference equations explicitly, or at least to showing that the limiting form of any curve through these points is the ordinary catenary.

differential equation]. HUYGENS' extant notes[1]) enable us to reconstruct his solution. The statical principle is the same theorem of STEVIN as was used in HUYGENS' earlier work described in § 6. A part of the complexity of the analysis lies in HUYGENS' insistence on *first calculating the figure of the equilibrium of the weighted cord*, then passing to the limit. An equal part, however, lies in the geometrical method ; [nowadays we admire a person who could think correctly in such an elaborate way[2]).

1) Appendix 1 to the letter of 9 October 1690 to LEIBNIZ, Œuvres complètes **9**, 500—501, explains the cipher (conjectured date, September 1690); Appendix 2, of the same date, 502—510, explains the solution, but even with the aid of the editors' copious notes it remains extremely difficult to follow. Another fragment of 1690, emphasizing the statement italicized in the text above, is given in Œuvres complètes **19**, 66—68. Another is the appendix to the letter of LEIBNIZ of Oct.—Nov. 1690, Œuvres complètes **9**, 541—543; here the quartics are discussed. There is also a later explanation, written presumably in 1691, Œuvres complètes **10**, No. 2724, and perhaps a first draft for the publication cited in footnote 4, p. 66.

2) It seems pointless to follow in detail the further discussion that fills much of HUYGENS' Œuvres complètes **10**, but we add a summary of it. LEIBNIZ, convinced indeed correctly but as yet without sufficient reason that BERNOULLI has used differential calculus, triumphs in the power of his "key". Also, his solution and BERNOULLI's, unlike HUYGENS', do not presuppose the quadrature of any curve [except the hyperbola] (see especially his letter of 24 July 1691). HUYGENS at first expresses great admiration for the work of LEIBNIZ and BERNOULLI. In the notes for his letter of 1 September 1691 he writes, "The additional properties you and Mr. BERNOULLI have discovered I did not even search for . . . since I thought them incomparably more difficult to find than in fact they are." He would like to follow their methods. He begins to think that after all the differential calculus may have some advantages. In time, however, he grows suspicious that LEIBNIZ had achieved the solution only after getting a prior hint of BERNOULLI's method—a suspicion that would be the last to enter a modern reader's mind. HUYGENS begins to consider his own solution, using only "ordinary geometry", as the best, but he continues to beg to see LEIBNIZ's and BERNOULLI's methods. For LEIBNIZ's final response, see p. 71 below.

A great part of the discussion concerns special cases and reflects a passion for special properties of special curves that the modern reader is unable to share. The mechanical principles on which the three solutions rest are scarcely mentioned.

LEIBNIZ seized the opportunity to advertise his calculus by publishing in three countries his summaries of the results and the methods the several authors had used to obtain them: "*De solutionibus problematis catenarii vel funicularis in actis Junii* A. 1691, *aliisque a Dn. J. B. propositis*," Acta erud. Sept. 1691, 435—439; "*De la chainette, ou solution d'un problême fameux proposé par Galileï, pour servir d'essai d'une nouvelle analise des infinis, avec son usage pour les logarithmes, et une application à l'avancement de la navigation*," Journal des Sçavans **20** (1692), Amsterdam ed. 218—226 (1693); "*Solutio illustris problematis a Galilaeo primum propositi de figura chordae aut catenae e duobus extremis pendentis, pro specimine novae analyseos circa infinitum*," Giornale de' Letterati, Modena, 1692, 128—132; all three are reprinted in LEIBNIZens math. Schriften **5**, 255—266.

In the first of these, LEIBNIZ says that HUYGENS' method rests on use of the radius of curvature, but this must have been a conjecture, since LEIBNIZ had not seen HUYGENS' proof or any real explanation of it—in fact, in his correspondence with HUYGENS he showed no curiosity of his elderly friend's line of thought, which must surely be based on the "ordinary geometry" LEIBNIZ wished to supplant. As we shall see below, the intrinsic equations were found later by JAMES BERNOULLI but not published.

It is curious that the catenary was both the first and the last problem HUYGENS attacked; but it is not without parallel that his departure from the world of mathematics fell below the brilliance of his entry. This is less a measure of the man than of the method: While HUYGENS' notes show that this problem strained his mathematical equipment, which was limited to "ordinary geometry", to the utmost, we shall see now that for the possessors of the new calculus the determination of the continuous catenary, while not trivial, fell quickly before a determined attack. In fairness to HUYGENS we must admit that he solved first, at least in principle, the more difficult problem of determining the form of the weighted string.]

In his paper, LEIBNIZ[1]) writes that JAMES BERNOULLI had "publicly asked me to try whether our kind of calculus could be applied to this kind of problem . . . Having tried the matter for his sake, not only did I have so great success as to be the first . . . to solve this illustrious problem, but also I found that this line has extraordinary uses . . ." The solution is "geometrical, without help of a thread or chain, and without assuming any quadratures, by a kind of construction for transcendents, than which nothing more perfect nor more appropriate for analysis exists, in my opinion."

In Figure 21, $\odot N$ is horizontal. ξ and (ξ) are points on the "logarithmic line",

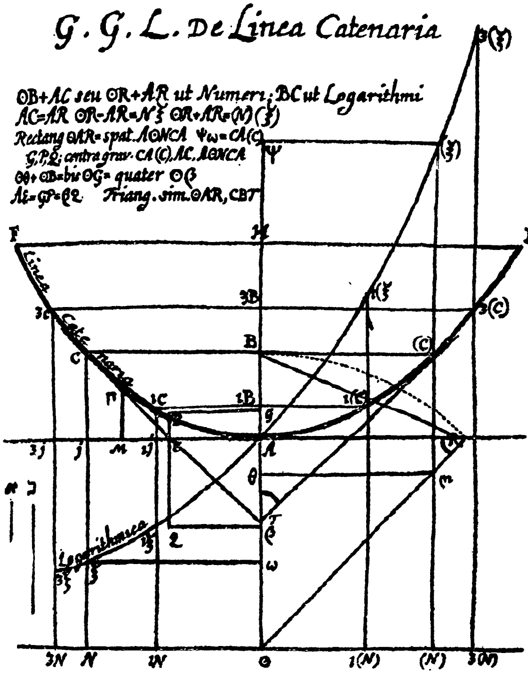

Figure 21.
LEIBNIZ's published figure for the catenary (1690)

In all three notes, LEIBNIZ reproaches HUYGENS with having supposed "the quadrature of a certain figure", in the third one going so far as to remark that the quadrature is "very complicated, and the author does not give its nature or reduction, and besides it is not consonant with the nature and degree of the problem"; it is curious to contrast this with HUYGENS' criticism of JAMES BERNOULLI's elastica (below, p. 97). In the second, LEIBNIZ implies that he has found that a light chain really assumes this form, while a string, being both extensible and somewhat stiff, does not. It is amusing to read that such a chain may be used inversely, by aid of LEIBNIZ's solution, for calculating logarithms, and "this may help, since on long trips one may lose his table of logarithms . . ."

JOHN BERNOULLI esteemed HUYGENS' solution lightly, found LEIBNIZ's "very pretty", but was unable to see cause for LEIBNIZ's boasting of its superiority over his own. (See JOHN BERNOULLI's letter of 29 September/9 October 1691 to JAMES BERNOULLI in *op. cit. ante*, p. 66, footnote 1.)

1) "*De linea in quam flexile se pondere proprio curvat, ejusque usu insigni ad inveniendas quot-*

[which we should now write as $\dfrac{y}{a} = b^{x/a}$, where b is a dimensionless quantity and $a = \odot A$]. "Now taking $\odot N$ and $\odot(N)$ as equal, above N and (N) erect NC and $(N)(C)$, respectively, both equal to half the sum of $N\xi$ and $(N)(\xi)$; then C and (C) will be *points of the catenary line* . . ." [Thus LEIBNIZ's solution is

$$\frac{y}{a} = \tfrac{1}{2}c(e^{x/a} + e^{-x/a}) = c \cosh \frac{x}{a}\,; \tag{21}$$

the mechanical problem requires in fact that $c = 1$.]

LEIBNIZ's paper contains a good deal of explanatory material, especially concerning logarithms, but he neither derives his solution (21) nor proves its validity. He states that the triangles $\odot AR$ and CBT are similar. He states also that $AR =$ the arc length from A to the point $C(x, y)$. [That is,

$$\frac{dy}{dx} = \frac{s}{a}\,.] \tag{22}$$

LEIBNIZ gives a construction for the center of gravity of any arc: ". . . the tangent CT cuts at E the horizontal line through A; let the rectangle $GAEP$ be completed..." [As we shall see, this is the key to the solution.] He concludes, "Thus . . . will be had the greatest possible descent of the center of the string or chain or any flexible and inextensible line, hung up from its two ends . . . and having a given length . . ." [This is the extremal principle first used by HUYGENS (above, p. 45); it is not justified by the foregoing construction.]

Among other results is the series (appropriate to the case $a = 1$)

$$\begin{aligned} x &= s - \tfrac{1}{6}s^3 + \tfrac{3}{40}s^5 - \tfrac{5}{112}s^7 + \dots \\ &= \operatorname{Arg\,sinh} s\,. \end{aligned} \tag{23}$$

"So as to avoid prolixity, I refrain from supplying the proofs, especially since to him who understands the calculi of our new analysis explained in this journal they will come of themselves."

A letter of 26 October 1690 from LEIBNIZ to v. BODENHAUSEN[1]) reveals that LEIBNIZ had "looked back at Father PARDIES' treatise . . .; I find his assumptions correct, but well known . . ." LEIBNIZ gives a just résumé of PARDIES' work and remarks that the case of the elastic cord furnishes "an entirely new and more complicated problem." As explanation,

cunque medias proportionales et logarithmos," Acta erud. June 1691, 277—281 = LEIBNIZens math. Schriften **5**, 243—247. LEIBNIZ' letter of 24 July 1691 to HUYGENS gives a summary of the published paper and a carefully drawn sketch.

1) LEIBNIZens math. Schriften **7**, 356—357 = (in more accurate transcription) Œuvres complètes de HUYGENS **10**, 157—158, footnote 7.

perhaps, of the cryptic nature of his publication, LEIBNIZ writes[1]), "Es ist aber guth, daß wann man etwas würklich exhibiret, man entweder keine demonstration gebe, oder eine solche, dadurch sie uns nicht hinter die schliche kommen."

In a fragment[2]) from this time LEIBNIZ writes, "The fundamental assumption so as to put the nature of the catenary curve into equations, as HUYGENS, Father PARDIES, and others noted long ago, is the following property of the tangents," and he then states the theorem of PARDIES (above, p. 51).

To learn LEIBNIZ's full course of thought, we turn to the magnificent letter[3]) of 14 September 1694 with which he finally answered a long sequence of requests, complaints, and accusations from HUYGENS. We reproduce the passage intact, in the original notations[4]):

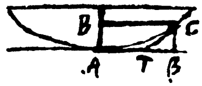

Figure 22.
LEIBNIZ's figure for explaining to HUYGENS his solution of the catenary problem (1694)

"Mais pour vous donner un example d'un probleme Geometrique, prenons celui de la Chainette : et je vous donneray en meme temps l'analyse dont je me suis servi autres fois pour le resoudre, puisque vous avés temoigné de la desirer aussi. Soit AB, x ; BC, y ; AT, retranchée par la tangente, est la distance entre l'axe et le centre de gravité de l'arc AC. Or, $C\beta$ ou AB est à $T\beta$, comme dx à dy; donc $T\beta$ sera $x\overline{dy:dx}$, et AT sera $y - x\cdot\overline{dy:dx}$. L'arc AC soit appellé c et par la nature du centre de gravité il est manifeste, qu'AT sera

$$\int \overline{ydc} : c(1) = y - xdy : dx \quad \text{ou bien} \quad \int ydc(2) = cy - cxdy : dx;$$

et differentiando

$$ydc(3) = cdy + ydc - \overline{xdy : dx}dc - cdy - cxd,\overline{dy : dx}\ .$$

Et rejettant ce que se détruit, il y aura $dc\overline{dy : dx} + cd,\overline{y : dx}(4) = 0$. Supposons que les y ou $\dfrac{BC}{A\beta}$ croissent uniformement, ou que dy soit constante et $ddy(5) = 0$, nous aurons $d\cdot\overline{dy : dx}(6) = -dyddx : \overline{dxdx}$, et au lieu de 4 il y aura $dcdx - cddx(7) = 0$, c'est-à-dire summando $dx : c(8) = dy : a$ (car cette equ. 8. estant differentiée rend l'equation 7) ou bien $adx(9) = cdy$ et differentiando $addx(10) = dcdy$. Or generalement en toute courbe $dcdc(11) = dydy + dxdx$ et differentiando $dcddc = dyddy + dxddx$, donc icy (par 5) $dcddc(12) = dxddx$, et (par 10 et 12) $addc(13) = dxdy$ et summando $adc(14) = xdy + bdy$. Soit $x + b(15) = z$, fiet $dx(16) = dz$ et $adc = zdy$, et

1) In his report to v. BODENHAUSEN on the catenary, LEIBNIZens math. Schriften 7, 359—361.

2) LEIBNIZens math. Schriften 7, 372.

3) Œuvres complètes de HUYGENS 10, 679. Essentially the same material is contained in LEIBNIZ' letter to v. BODENHAUSEN of about 1691, printed in LEIBNIZens math. Schriften 7, 370—372.

4) LEIBNIZ's x, y, c are the variables y, x, s in the notation used elsewhere in this work.

(par 11 et 16) $dcdc = dzdz(17) + dydy$. Donc par 14, 15, 17, nous aurons $aadzdz + aadydy(18) = zzdydy$, et enfin[1]) $y(19) = aa \int dz : \sqrt{zz - aa}$, c'est-à-dire il ne faut que chercher la quadrature d'une figure, dont l'ordonnée est $aa : \sqrt{zz - aa}$."

LEIBNIZ's statical principle, a corollary of PARDIES' theorem, is stated in the first line: *The distance AT is the y co-ordinate of the center of mass of the arc AC.* Once this statical principle is granted, we have the integro-differential equation (in our usual notation)

$$\frac{1}{s}\int_0^s x\,ds = x - y\frac{dx}{dy}, \tag{24}$$

which is the equation numbered (1) by LEIBNIZ. We multiply by s and then differentiate with respect to s; the resulting differential equation is at once integrable to yield (22). LEIBNIZ's analysis, which goes further and derives a quadrature from which (21) is immediate, [seems brilliantly clear to a modern reader. The impression it made on HUYGENS, to whom differential calculus was foreign, may be imagined.] Indeed, on 27 December 1694 he called LEIBNIZ's argument "a strange route."

In the young JOHN BERNOULLI's *Solution of the funicular problem*[2]) we read, "It is almost a year since in conversation with my enlightened brother we happened to speak of the nature of the curve that is assumed by a string hung freely between two points. We marvelled that a thing daily present to the eyes and hands of everyone should not as yet have drawn the attention of anybody. The problem seemed extraordinary and useful, but because of its apparent difficulty we preferred not to touch it; we decided thus to propose it publicly to the learned, to see if anyone would dare to try, for we did not know that it had been discussed among the geometers since the time of GALILEO . . . I have found moreover that our funicular curve is not geometrical but rather of the type called mechanical, since its nature cannot be expressed by any determinate algebraic equation . . ." JOHN BERNOULLI states his results, without proofs, in the form of two constructions.

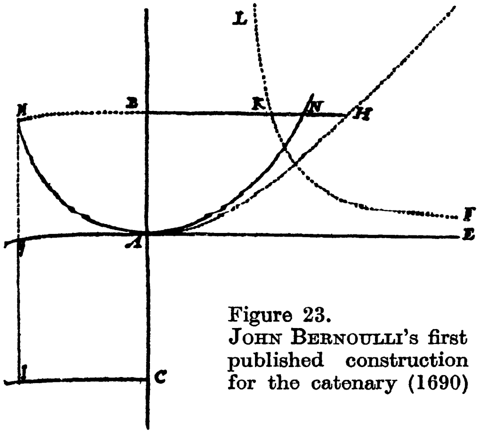

Figure 23.
JOHN BERNOULLI's first published construction for the catenary (1690)

First construction (Figure 23). Let AH be an equilateral hyperbola with center at C; [thus its equation is $X^2 = y^2 + 2ay$, where A is the origin and $a = AC$.] Holding y fixed, let KF be so constructed that $X\mathfrak{X} = a^2$, where $\mathfrak{X} = BK$. Let $x = AG$, and construct x so that

1) In this formula and the next, the first a should be deleted.

2) "*Solutio problematis funicularii,*" Acta erud. June 1691, 274—276 = Opera omnia **1**, 48—51 = LEIBNIZens math. Schriften **5**, 248—250.

$$(25)\qquad \begin{aligned} -xa &= \text{Area}\quad EABKF \\ &= \int_0^y \mathfrak{X}dy\,. \end{aligned}$$

Then x, y is a point on the catenary. [Indeed, we find that

$$(26)\qquad \begin{aligned} \mathfrak{X} &= \frac{a^2}{\sqrt{y^2+2ay}}\,, \\ -\frac{x}{a} &= \int_0^y \frac{dy}{\sqrt{y^2+2ay}}\,; \end{aligned}$$

integration yields $\frac{y}{a} = \cosh\frac{x}{a} - 1$, differing from (21) only in choice of origin.]

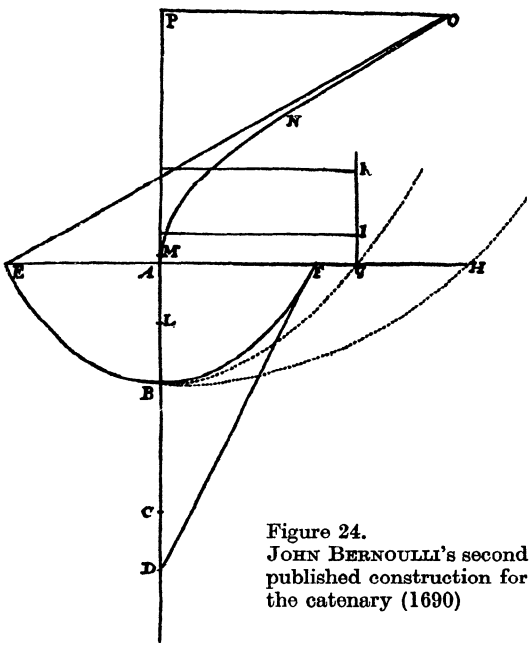

Figure 24.
JOHN BERNOULLI's second published construction for the catenary (1690)

Second construction (Figure 24). Let BG be an equilateral hyperbola; [its equation is $X^2 = y^2 + 2ay$]. Let BH be a parabola whose latus rectum is four times the latus rectum of the hyperbola; [*i. e.*, $\mathfrak{X}^2 = 8ay$]. Then if we lay off $GE = BH$, the point E lies on the catenary. [This last means

$$(27)\qquad X - x = \int_0^y \sqrt{1+\mathfrak{X}'^2}\,dy\,,$$

or

$$(28)\qquad -x = -\sqrt{y^2+2ay} + \int_0^y \sqrt{1+\frac{2a}{y}}\,dy\,,$$

whence $(26)_2$ follows.]

JOHN BERNOULLI then lists thirteen properties of the catenary. The first of these, referring to Figure 24, reads: "Let FD be a tangent; then $AF:AD = BC:BF$." [Analytically expressed, this is (22); as we shall see, it gives the key to the solution.] The thirteenth is an awkward expression for the variational principle[1]) asserted simultaneously by LEIBNIZ (above, p. 70) and used earlier by HUYGENS (above, p. 45).

"My honorable brother has begun to extend this thought to strings of non-uniform thickness, when the thickness stands in a relation to the length which is expressible by an algebraic equation." JAMES BERNOULLI has noted a special law for the density which leads to a simple solution, and JOHN BERNOULLI has shown that this funicular is the evolute of that for the case of uniform thickness. For experimental tests, "one should take a fine chain rather than a string, which sometimes because of too much lightness, sometimes too much rigidity, we have found unsuitable.

1) In his comment in Lesson 37 of the work discussed just below, JOHN BERNOULLI adds, "This is proved by the axiom that the center of gravity descends as far as it can," but this is a mere restatement.

"For the rest, whoever wishes to perfect and extend this subject may investigate the nature of the curve . . . in the case when the string is a finite distance from the center of the earth, or if also it is supposed extensible by its own weight or loaded in any other way; or, vice versa, how it should be loaded in order to assume the form of a parabola, hyperbola, circle, or any other given curve. The matter is altogether within reach."

John Bernoulli's concepts and methods are given in his *Mathematical lessons on the method of integrals and other subjects, written for the use of the illustrious Marquis* de l'Hôpital *while the author was at Paris in the years 1691 and 1692*[1]); while these were not published until 1742, their content was certainly widely diffused in the teaching, both direct and indirect, of the great Bernoulli who dominated the productive mathematics of the first half of the eighteenth century. In Lesson 37, *On funicular or catenary curves*, the following principles are set down as self-evident for any hanging curve.

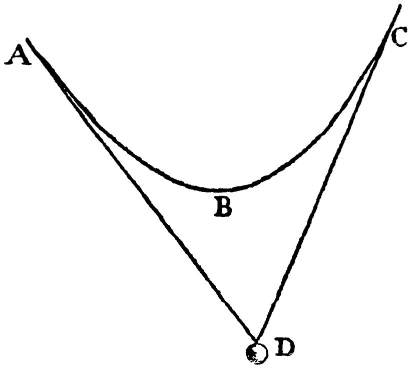

Figure 25. John Bernoulli's figure for stating Pardies' theorem (1691–1692)

(1) In Figure 25, the forces which must act at A and C in order to support the cord are the same as those that must be applied along the tangents AD and CD in order to support at D a weight equal to the weight of the cord. [This is the principle of Pardies, above p. 51.]

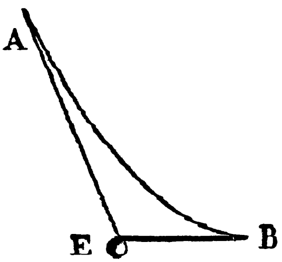

Figure 26. John Bernoulli's application of Pardies' theorem (1691–1692)

(2) Applying No. 1 to portion of the cord between A and the lowest point B yields the (horizontal) force at B (Figure 26).

(3) If the cord is hung from any intermediate point, such as F, the remaining portion FC retains its previous figure.

(4) In the case mentioned in No. 3, the forces acting on each portion of the cord between F and C are the same as before. In particular, the force acting at B is unaltered.

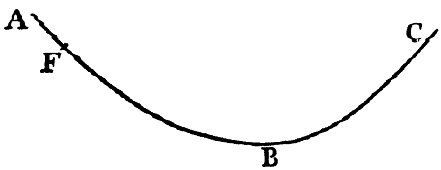

Figure 27. John Bernoulli's figure for isolating a portion of the catenary (1691–1692)

(5) Forces may be resolved according to the vectorial rule.

1) "*Lectiones mathematicae de methodo integralium aliisque, conscriptae in usum ill. Marchionis* Hospitalii *cum auctor Parisiis ageret annis 1691 et 1692*," Opera omnia **3**, 385—558 (1742) [1743]. I cannot forbear remarking that these lessons together with those on differential calculus, lost until 1922, form the most beautiful treatise on calculus ever written. It is ironical that this masterly exposition by one of the discoverers had to wait over 200 years for full publication.

JOHN BERNOULLI's elegant proof is easier to follow if we introduce the inclination θ at an arbitrary point A (Figure 28) and the tension T acting at that point. Consider the equilibrium of the portion of the cord between A and B. By No. 3 we may consider the cord suspended by the tension T at A, and by No. 4 the tension at B is independent of the choice of A; call this constant ka. By No. 2, the tension at A equilibrates the horizontal force ka and the vertical force ks, where s is the length of AB. By No. 5, in order that these two forces be equilibrated by a tangential force at A we must have $\frac{ks}{ka} = \tan\theta = \frac{dy}{dx}$, which is (22). [Thus JOHN BERNOULLI's statical principle is the *equilibrium of forces*, applied to a finite segment beginning at the lowest point. Indeed, balance of vertical and horizontal forces yields

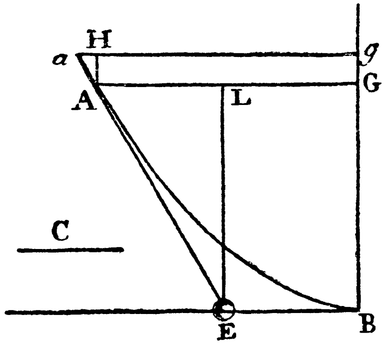

Figure 28. JOHN BERNOULLI's figure for explaining to L'HÔPITAL his solution of the catenary problem (1691–1692)

$$T\sin\theta = ks, \quad T\cos\theta = ka, \tag{29}$$

where T is the tension at A; elimination of T yields (22); an alternative form of $(29)_2$ is

$$T = ka\frac{ds}{dy}\ .] \tag{30}$$

Manipulation of (22) easily yields $(26)_2$.

[Evidently JOHN BERNOULLI did not find LEIBNIZ's form (21) of the solution[1]).]

11. JAMES BERNOULLI's researches on the general theory of flexible lines (1691—1704), and later work to 1717. There is no evidence that the deep and enigmatic JAMES BERNOULLI had a solution to the problem of the catenary in 1690[2]). His next mention of it

1) Lesson 37 purports to give LEIBNIZ's solution but of course does not reveal to us how LEIBNIZ reasoned; rather, JOHN BERNOULLI merely verifies that (21) satisfies LEIBNIZ's differential equation (22).

2) In annotating the above cited paper as republished in JAMES BERNOULLI's Opera in 1744, the editor, GABRIEL CRAMER, wrote "whether the method of our author was entirely dissimilar from that of his brother, which I am going to explain, we dare not guess." The method then presented is indeed essentially that of JOHN BERNOULLI but applied in generality sufficient to obtain JAMES BERNOULLI's later results (41).

JOHN BERNOULLI in later years asserted that his brother had been unable to solve the problem. He tells his recollection of the discovery in a letter to DE MONTMORT on 29 September 1718 (quoted in part on pp. 97—98 of *op. cit. ante*, p. 66, footnote 1):

"But it is time, Sir, that I draw you forth from your astonishment. I astonish you, you say, by saying that my brother was unable to solve the problem of the catenary: Yes, I tell you again, for it is an uncontestable truth, of which I will give you proofs which put an end to your astonishment . . . You say that my brother proposed this problem; that is true, but does it follow that he had is in an addition to a paper[1]) published in 1691. § 1 of the addition states the form of the

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a solution of it then? Not at all. When he proposed this problem at my suggestion (for I was the first to think of it), neither the one nor the other of us was able to solve it; we despaired of it as insoluble, until Mr. LEIBNIZ gave notice to the public in the Leipzig journal of 1690, p. 360, that he had solved the problem but did not publish his solution, so as to give time to other analysts, and it was this that encouraged us, my brother and me, to apply ourselves afresh.

"The efforts of my brother were without success; for my part, I was more fortunate, for I found the skill (I say it without boasting, why should I conceal the truth?) to solve it in full and to reduce it to rectification of the parabola. It is true that it cost me study that robbed me of rest for an entire night. It was much for those days and for the slight age and practice I then had, but the next morning, filled with joy, I ran to my brother, who was still struggling miserably with this Gordian knot without getting anywhere, always thinking like GALILEO that the catenary was a parabola. Stop! Stop! I say to him, don't torture yourself any more to try to prove the identity of the catenary with the parabola, since it is entirely false. The parabola indeed serves in the construction of the catenary, but the two curves are so different that one is algebraic, the other is transcendental. I have unfolded the whole mystery. Having said that, I showed him my solution and explained the method that had brought me to it.

"It pleased him at first, and he saw straightaway (although that was no longer difficult after the method was found) that this method was applicable to all kinds of catenaries of non-uniform thickness. There is the reason for the words, 'My honorable brother has begun to extend this thought' *etc.*

"But then you astonish me by concluding that my brother found a method of solving this problem . . . I ask you, do you really think, if my brother had solved the problem in question, he would have been so obliging to me as not to appear among the solvers, just so as to cede me the glory of appearing alone on the stage in the quality of first solver, along with Messrs. HUYGENS and LEIBNIZ? You knew the disposition of my brother. He would sooner have taken away from me, if he could have done so honestly, the honor of being the first to solve it, rather than letting me take part by myself, let alone ceding me the place, if it had really been his." JOHN BERNOULLI goes on to explain the wording used by LEIBNIZ and the editor of the Leipzig *Acta* in respect to this question of priority, and to give other evidence that the solution of the catenary was not due to JAMES BERNOULLI.

While claims of this sort by JOHN BERNOULLI were formerly taken lightly by historians, most of them have been substantiated in all essentials by concrete evidence. In the case of the catenary, JOHN BERNOULLI's account is supported by such evidence as there is, not only that presented in the text above but also by the "*Remarques de Mr. LEIBNIZ sur l'art. V. des nouvelles de la république des lettres du mois de février 1706*," Nouv. Rép. Lettres 1706 = LEIBNIZens Math. Schriften II 1, 389—392. LEIBNIZ writes, ". . . Mr. [JAMES] BERNOULLI . . . asked me, at the suggestion of his brother, who was already far advanced in these matters, to reflect whether by the same analysis one could not [find] . . . the curve that a chain would form, supposing it to be perfectly flexible, [the curve] that GALILEO had thought to be a parabola, although they did not yet know he had worked on the problem. I reflected about it, and I succeeded at once, but instead of publishing my solution, I encouraged Mr. BERNOULLI to try to find it. Doubtless my success was the reason that the two brothers applied themselves vigorously to this problem and that the younger . . . prevailed with entire success (eut l'avantage d'y réussir entièrement). To get so far by the means I had up to then communicated required extraordinary skill and some practice, which application and the desire for distinction gave them so as to make good use of this new calculus." It is unlikely that LEIBNIZ knew as much about the matter as did JOHN BERNOULLI, but he was always just and equally desirous for the success of each of the brothers.

1) "*Specimen alterum calculi differentialis in dimetienda spirali logarithmica, loxodromiis nautarum, et areis triangulorum sphaericorum; una cum additamento quodam ad problema funicularium,*

catenary curve corresponding to certain particular non-uniform densities. § 2 considers the case of an extensible cord of uniform thickness. "I suppose, moreover, that the extensions are proportional to the stretching forces, even though I doubt that that hypothesis be sufficiently congruent to reason and experiment. Let us be allowed to retain it, however, since we know none truer." The result stated is

$$x = \int \frac{b\,dy}{\sqrt{2a^2 + 2by - 2a\sqrt{a^2 + b^2 + 2by}}} \tag{31}$$

where b is an elastic constant. [When $b = 0$, (31) becomes indeterminate and does not immediately reduce to $(26)_2$, and it is difficult to make anything out of this paper.]

For explanation, we turn again to JOHN BERNOULLI's *Lessons*, which may be presumed to reflect JAMES BERNOULLI's views on these topics[1]). Lesson 38, *On the curvature of a string of non-uniform thickness*, begins by observing that if the weight of the arc AB is not ks but $kp(s)$, then the same argument[2]) as for the uniform case leads to

$$\frac{dy}{dx} = \frac{p(s)}{a}, \tag{32}$$

generalizing (22). [This is the continuous analogue of HUYGENS' theorem (above, p. 67).] In the special cases treated by JAMES BERNOULLI, the quadrature is relatively easy.

Lesson 39 first considers the case when the weight p is known as a function of x rather than of s. Then (32) yields at once

$$ay = \int_0^x p(x)\,dx\ . \tag{33}$$

For example, if $p = bx$, we have $y = \frac{1}{2}\frac{b}{a}x^2$, the ordinary parabola; [this is the solution of the suspension bridge problem obtained long ago by BEECKMAN, HUYGENS, and PARDIES (above, pp. 24, 45—46, 51—52)]. After working out two other special cases, JOHN BERNOULLI takes up the *inverse problem*: If $y = y(x)$ is the *given* shape of the funicular, then

aliisque," Acta erud. June 1691, 282—290 = Opera omnia 1, 442—453. The addition occupies pp. 449—453 = LEIBNIZens math. Schriften 5, 252—254.

1) JAMES BERNOULLI, as we have seen, claims the results. JOHN BERNOULLI in his letter to DE MONTMORT, quoted above, pp. 75—76, when vehemently defending his sole priority over his brother with respect to the ordinary catenary does not make any reference to these problems except to say that they had become "no longer difficult".

2) Indeed, in the copy of JOHN BERNOULLI's Opera in the Basel University Library, at Remark 13 in Lesson 37 a correction lettered in an old hand emends "the distances of those points" to read "the distances of the centers of gravity of those points," which is an awkward way of stating (32).

the density is $p = a\dfrac{dy}{dx}$. Let t be the weight per unit horizontal length. Then

$$t = \frac{dp}{dx} = a\frac{d^2y}{dx^2}\,. \tag{34}$$

Therefore whenever the catenary is "geometrical" [*i. e.*, an algebraic curve], t is also geometrical. For example, if the catenary is the parabola, $y \propto x^2$, then $t =$ const., so that the horizontal load is uniform. [It now seems more natural to consider the line weight σg per unit length of cord,

$$\sigma g = \frac{dp}{ds} = \frac{dp}{dx}\,\frac{dx}{ds} = \frac{a}{\sqrt{1+\left(\frac{dy}{dx}\right)^2}}\,\frac{d^2y}{dx^2} = a\left[1+\left(\frac{dy}{dx}\right)^2\right]\varkappa\,.] \tag{35}$$

Lesson 40 considers the case when $p = p(y)$.

Lesson 41, the most interesting after Lesson 37, is *On the curvature of extensible strings.* JOHN BERNOULLI, as suggested by his brother JAMES, adopts "the axiom of LEIBNIZ [*i. e.* HOOKE's law] that the extensions are proportional to the pulling forces[1])." [The analysis is difficult to follow[2]) but is important because the special devices used for the inextensible catenary are not sufficient here; BERNOULLI must face not only the complication introduced by the elasticity of the cord but also the fundamental statical problem.]

BERNOULLI again considers the equilibrium of the section of cord from the lowest point B to A; again the weight of the cord equilibrates the horizontal tension ka at B and the tangential tension T at A. Let s denote arc length in the deformed cord [no longer the same as arc length in the undeformed cord]. Let the elastic law be that a force T produces a local extension $\dfrac{b}{a}\cdot\dfrac{T}{ka}$ in the cord. If $d\alpha$ is the original length of the element ds at A, we have then $ds = d\alpha\left(1+\dfrac{b}{a}\cdot\dfrac{T}{ka}\right)$. The weight density $-F_y$ is related to that in the undeformed cord, k, by $-F_y ds = k\,d\alpha$. Hence

$$-F_y = \frac{k}{1+\frac{b}{a}\cdot\frac{T}{ka}}\,. \tag{36}$$

For statical principles, first we have (30) [which was implied by BERNOULLI's earlier

1) In addition, he supposes the cord to be incompressible and concludes that the areas S, s and lengths L, l before and after deformation satisfy $SL = sl$. These areas are infinitely small, and the curve considered is that "in the middle" of the cord. These assumptions, however, do not appear to be used.

2) Somewhat clearer is CRAMER's version, given as an annotation on p. 451 of JAMES BERNOULLI's Opera 1. Our presentation does not reproduce either source but rather attempts to bring out clearly what JOHN BERNOULLI's steps seem to imply.

analysis but apparently not noticed until now]. In addition, BERNOULLI infers from the balance of forces the principle

$$\frac{d}{ds}\left(ka\,\frac{dy}{dx}\right) = -F_y\,, \tag{37}$$

expressed partly in words. Combining (30), (36) and (37) yields

$$\frac{d}{ds}\left(a\,\frac{dy}{dx}\right) = \frac{1}{1+\frac{b}{a}\frac{ds}{dx}}\,. \tag{38}$$

When $b = 0$, this reduces to (22); when $b \neq 0$, it may be manipulated into the form of JAMES BERNOULLI's equation (31).

[More important than the clever solution of this problem that LEIBNIZ had regarded as hopelessly difficult is the method. We now write the statical equations for a flexible line as

$$\begin{aligned} \frac{d}{ds}(T\cos\theta) &= \frac{d}{ds}\left(T\,\frac{dx}{ds}\right) = -F_x\,,\\ \frac{d}{ds}(T\sin\theta) &= \frac{d}{ds}\left(T\,\frac{dy}{ds}\right) = -F_y\,, \end{aligned} \tag{39}$$

where F_x and F_y are the components of applied force per unit length in the directions of x and y. In all problems considered so far, $F_x = 0$, and integration of $(39)_1$ yields (30). The resulting expression for T, when put into $(39)_2$, yields BERNOULLI's result (37). What is important is that BERNOULLI obtains (37) by the fully general statical argument which we should now use to obtain $(39)_2$. That is, while he still expresses the equilibrium of horizontal forces in an integrated form valid only in special cases, his result (37) for the vertical forces is a condition of equilibrium in *differential form*. For the first time, the *resultant force acting on an infinitesimal element* has been calculated. This is *the first step in continuum mechanics*, and it is also the first advance toward the theory of stress since GALILEO's simple argument concerning the strength of a rope (above, p. 37) and PARDIES' remarks on the tension in a catenary (above, p. 51).

The result (37), as it stands, is of great value, for it is *the general equation of equilibrium for a flexible line subject to load parallel to a fixed direction.* The difference between the mastery of mechanical principles in 1690 and today is strikingly illustrated by the fact that the modern student may read off, by inspection of (37), the equation of small transverse oscillations of a taut string, for one has only to put $ds \approx dx$, $T = ka =$ const., and take the transverse force F_y as merely inertial, $F_y = -\sigma\frac{\partial^2 y}{\partial t^2}$, whence follows $\sigma\frac{\partial^2 y}{\partial t^2} = T\frac{\partial^2 y}{\partial x^2}$, but in fact a full fifty years of mechanics lay ahead before this equation was to appear in the work of D'ALEMBERT and EULER. See §§ 33—34, below.]

Returning to JAMES BERNOULLI's *Addition* (above, p. 76), in § 4 we find stated the problem of the *velaria:* To find the curve assumed by the base of a cylindrical sail. As formulated by BERNOULLI, the velaria is the figure of a perfectly flexible cord loaded by a uniform normal pressure; this curve, [determined incorrectly by HUYGENS, above, p. 46,] JAMES BERNOULLI asserts to be a circular arc. After some controversy, it was decided the proper loading is a uniform force per unit length parallel to a fixed direction; in this case, the curve was shown to be the ordinary catenary. About this time was proposed also the problem of the *lintearia*, the form of a cylindrical cloth filled with water; this was shown to be an elastic curve of the type to be discussed in our § 12 below. These same problems could be regarded in an alternative light. *E. g.*, as had been known to HOOKE (above, p. 57) and as was pointed out anew by GREGORY[1]), the catenary turned upward gives the solution for an arch sustaining its own weight through tangential compression alone, thus needing no cement. While these problems called forth considerable ingenuity, mainly in respect to differential manipulations, and occasioned the great quarrel between the brothers BERNOULLI, nevertheless, so far as I can learn, they gave rise to no additional enlightenment of mechanics, so they shall not be considered further here[2]).

Whether or not JAMES BERNOULLI had a method for deriving the catenary in 1690, it is nearly certain[3]) that by June 1691 his slow but mighty intellect had found a *second approach*, differing more from those used by LEIBNIZ and JOHN BERNOULLI than do those two from one another. This approach rests on the concept of *curvature* (see below, pp. 90—91). While he never published this method, we may follow some of his ideas in his remarkable notebook, *Thoughts, notes, and remarks on theology and philosophy, condensed and collected from the year* 1677 *onward by me J. B.*[4]). No. CLXV, dating probably from 1691, concerns

1) Corollary 6 to Prop. 2 in *op. cit. infra* p. 85.

This was observed also by PARENT in a work which appeared in 1700; see pp. 810—815 of vol. 2 of his *Essais*, cited on p. 110. The passage reprinted on pp. 494—499 of vol. 2, if it actually appeared in 1700, is the first correct derivation of the ordinary catenary to be published. The difficulties in connection with PARENT's publications are mentioned in footnote 1, p. 109 below.

2) A definitive original treatment is given in JOHN BERNOULLI's *Integral Calculus* (cited above, p. 74), Lessons 42—45, except that the identity of the lintearia with the elastica is not shown. Published expositions of inferior quality are to be found in the books of HERMANN and TAYLOR, cited below, p. 86.

3) In June 1691 he gave the solution for the elastica as an anagram (below, p. 88); on publishing this solution in 1694 (below, p. 89), he says that it rests on the second of the "two keys" to the catenary, namely, the formula for the radius of curvature. In the work of JAMES BERNOULLI *every sentence* must be weighed by the reader.

4) *Meditationes, annotationes, animadversiones theologiae & philosophiae, a me J. B. concinnatae & collectae ab anno 1677,* Basel Univ. Library MS Ia 3. As its title indicates, this is not a diary, and for many matters where the interest would be greatest there is no entry at all. In particular, and consistently with JOHN BERNOULLI's claims (above, pp. 75—76), there is nothing regarding the catenary prior to 1691.

the velaria. The load, which is normal, is resolved into rectangular components; the process is lengthy and obscure, and it seems that the radius of curvature is brought in *a posteriori* by looking at the equations derived. The statical principle seems to be the theorem of PARDIES or something akin to it; in any case, a finite arc rather than a differential element is considered[1]. Much later[2], probably in 1695, there is a thorough analysis of the string subject to various concentrated loadings. What is new is the concept of *tension*[3] (firmitas) of the string. By its aid, a straightforward balance of forces acting on the weighted string leads to results generalizing STEVIN's theorem (above, p. 45). When he comes to the continuous string[4], however, JAMES BERNOULLI turns aside from this line of thought and again considers a finite segment. He calculates the "line of mean directions" of the load, *i. e.*, the line such that the resultant force may be regarded as directed along it and concentrated at a point upon it. [In generalization of the theorem of PARDIES,] this line must pass through the point of intersection of the tangents from the ends of the arc, and its direction follows by integrating the forces. [JAMES BERNOULLI is still close to the methods successful in treating special cases.] These results are checked against the catenary and the elastica, visualized as the lintearia.

In a note[5] from 1697—1698 JAMES BERNOULLI finally obtains *the general equations for a flexible line*. This is made possible by systematic use of the tension, which is now the main tool in arguments applied either to a finite segment or to an infinitesimal element[6]. Let the small angle between the tangents at the two ends of an element be $d\theta$. Then the tensions exert a resultant force *normal to the element* of amount $T\,d\theta$, and this must balance the normal load $F_n ds$. Since $d\theta/ds = 1/r$, we have[7])

$$\frac{T}{r} = F_n = \text{density of normal load.} \tag{40}$$

(This result was discovered independently by SAUVEUR in 1703[8]).

1) No. CLXXXVII demonstrates the identity of the velaria and the catenary; No. CLXXXIX, of the lintearia and the elastica. The former of the sections numbered CCXXVIII concerns a construction of the catenary which BERNOULLI himself noted to be false.

2) Nos. CCXIII—CCXXVII, addition to No. CCXXVIII.

3) Of course the tension was present implicitly in the earlier solutions. It is its explicit recognition that is new and important.

4) No. CCXXIX and the immediately following No. CCXXXI.

5) No. CCXLV, printed in slightly expanded form as No. XI, pp. 1036—1048, of the "*Varia Posthuma,*" Opera 2 (1744).

6) Here BERNOULLI refers back to No. CLXV (above, p. 80).

7) The argument is given in words in the middle of p. 1037 of the printed version; one must supply an equality sign reading downward to realize that "firmit. fili in B" is prz/a.

8) "*Du frotement d'une corde autour d'un cilindre immobile*" (14 July 1703), Mém. acad. sci. Paris 1703, 2nd. 4to ed., Paris, 305—311 (1720). Prop. I states (40) only for the case of a rope lying on a circular cylinder, but the reasoning is general.

After a long detour concerning the mean line of the load, JAMES BERNOULLI balances the forces acting on a finite part of the string ; the argument, [in reality, the argument of JOHN BERNOULLI put in more general form[1]),] yields[2])

$$T\frac{dx}{ds} = T_0 - \int_0^s F_x ds\ , \tag{41}$$
$$T\frac{dy}{ds} = -\int_0^s F_y ds\ ,$$

where T_0 is the tension at the lowest point, $s = 0$. [These are integrated forms of (39).] The intrinsic equation companion to (40) is

$$\frac{dT}{ds} = -F_t = \text{tangential load per unit length.} \tag{42}$$

This result, also in integrated form, BERNOULLI obtains by some rather mysterious manipulations.

The result is rediscovered, at least in part, by JOHN BERNOULLI at the conclusion of his "*Solution du problême . . . sur les isoperimetres,*" Mém. acad. sci. Paris 1706, [2nd.] 4to ed., Paris, 235—245 (1731) = 12mo ed., Amsterdam, 304—318 (1708) = Opera 1, 424—435.

It is again rediscovered by TAYLOR, Prop. XXI, Prob. XVI of *op. cit. infra*, p. 86 (see also his proof of Lemma 9) and by HERMANN, § 93 of *op. cit. infra*, p. 86.

VARIGNON attributes (40) to SAUVEUR and to BORELLI, *De motu animalium* 2, Lugduni Batavorum, 1685; new ed., *ibid.*, 1710. BORELLI's Prop. 56 reads, "If a rope wound around a globe and [*recte* or] cylinder is pulled uniformly along its whole length, the power pulling the rope will be to the resistance of the globe or cylinder as its radius to the circumference of the rope." This result follows from (40), since it asserts that $\frac{T}{cF_n} = \frac{r}{c}$, where $c =$ the circumference. However, despite its correctness, even this corollary may not be attributed to BORELLI without reservation, since he adduces a fantastic argument about the velocities with which the parts of the cylinder or globe are contracted as the rope is pulled tighter.

VARIGNON himself spins out numerous corollaries; see his "*Pressions des cylindres et des cones droits, des spheres et des spheroïdes quelconques, serrés dans des cordes roulées autour d'eux, et tirées par des poids ou des puissances aussi quelconques,*" Mém. acad. sci. Paris 1717, 4to, Paris, 195—210 (1719), also Hist. *ibid.*, 68—70.

1) Perhaps it is on this account that in obtaining the quotient of $(41)_2$ by $(41)_1$ by an argument of this kind the editor of JAMES BERNOULLI's works on pp. 424—426 attributes the proof to JOHN BERNOULLI, though nothing so general is to be found in the latter's printed works. In Lesson 42 of *op. cit. ante*, p. 74, there is a start, and in Lesson 44 there is a near miss in connection with the lintearia, but in fact all of JOHN BERNOULLI's work rests on special integrated forms possible because of the specially simple loads considered.

2) In "*Extrait d'une lettre de Monsieur BERNOULLI de Bâle [à Mr. VARIGNON], du 26. juin 1698. Contenant l'examen de la solution de ses problêmes, inserée dans le Journal du 2. décembre, 1697,*" J. des Sçavans. 26 (1698), Paris ed. 355—360 = Amsterdam ed. 560—569 (1699) = JACOBI BERNOULLI Opera 2, 829—839 = JOHANNIS BERNOULLI Opera 1, 222—229, JAMES BERNOULLI in conjecturing the nature of an unpublished proof by his brother writes out results equivalent to (41) for the case of normal loading.

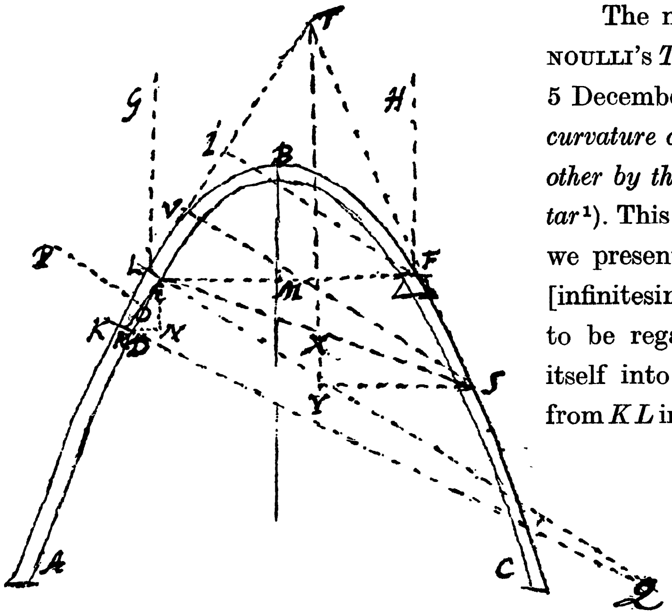

Figure 29. JAMES BERNOULLI's figure for calculating the form of the general catenary by use of the principle of virtual work (1704)

The next to last entry in JAMES BERNOULLI's *Thoughts, notes, and remarks,* "solved 5 December 1704," is called *Problem of the curvature of an arch whose parts support each other by their own weight, without use of mortar*[1]). This introduces a third method, which we present in much abbreviated form. The [infinitesimal] stone KL in Figure 29 " . . . is to be regarded as a wedge trying to force itself into the triangle DQE. As it comes from KL into the position DE, that is, while it traverses the space KD, it pushes back the force pressing along IL by the distance $KL - DE$." Then the *virtual work* done by the normal force $-F_n$ pointing inward equals that done against the compression $-T$. That is,

$$(43) \qquad -F_n \cdot KD = -T \cdot (KL - DE) \ .$$

From the geometry of the figure follows $KL/(r + KD) = DE/r$, so that

$$KD : (KL - DE) = r : ds \ .$$

Substituting this last into (43) yields (40). The argument is given by BERNOULLI only subject to the special assumptions appropriate to the arch; the result is $1/r = \frac{dy}{dx} \Big/ s\left(\frac{ds}{dx}\right)^2$, which is integrated to obtain (22).

JAMES BERNOULLI here considers also a second hypothesis: Friction being assumed sufficient to prevent the stone from slipping forward, it "tries to rotate" about its lower edge. While JAMES BERNOULLI now obtains a differential equation like (22) but with a factor 2 on the right-hand side, the "subtle paralogism" in his argument is pointed out in two annotations by his nephew NICHOLAS I BERNOULLI[2]): With correct analysis, this hypothesis leads to just the same result as the first. [Thus JAMES BERNOULLI introduces yet a *fourth method*: the *balance of moments* on a differential element. While for this problem

1) No. CCLXXXV, published in slightly expanded form as No. XXIX, pp. 1119—1123, of "*Varia Posthuma,*" Opera 2 (1744).

2) The BERNOULLIS we shall encounter in this history, along with our principal associations with

the outcome is the same, it is possible that JAMES BERNOULLI had the insight to grasp the *independence of the balance of moments from the balance of forces* in a continuous body.

Thus by 1698 JAMES BERNOULLI had wrung out the general equations of equilibrium for a plane flexible line. To this end, he had to abandon the special devices used for the ordinary catenary by his brilliant younger brother and by LEIBNIZ and to purify and deepen the problem until it was reduced to its essential : The action of any part of the line upon its neighbor is purely tangential.

By 1704, moreover, JAMES BERNOULLI had succeeded in grasping and using *four independent approaches:*

1. Balance of forces resolved in two fixed orthogonal directions.
2. Balance of forces normal and tangential to the line.
3. Virtual work.
4. Balance of moments.

Even today, there are scarcely any more.

Elegant as were the quick solutions of LEIBNIZ and JOHN BERNOULLI for the ordinary catenary, these achievements of JAMES BERNOULLI are of a different order of worth. Far from being easy extensions of what had been done before, they required a kind of intense fundamental thinking in rational mechanics that JAMES BERNOULLI alone, of all those we

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them, are shown in the following table:

- JAMES (general catenary (1691—1704), elastica (1691—1694), elastic laws (1695—1705)) 1655—1705
  - NICHOLAS I (Editor of Opera of JAMES, 1744) 1687—1759
- JOHN (catenary (1690), vibration of loaded or weighted string (1713, 1742)) 1667—1748
  - NICHOLAS II (correspondent of JAMES RICCATI) 1695—1726
  - DANIEL (frequencies and modes of vibrating systems (1733—1771), correspondence with EULER (1733—1763), elastic laws (1738—1742), co-existence of small oscillations (1753)) 1700—1782
  - JOHN II (small oscillations, conical strings (1736)) 1710—1790
    - JOHN III (intermediary between EULER and DANIEL BERNOULLI (1763—1766), laws of elasticity (1766—1768)) 1744—1807
    - JAMES II (vibrations of plates (1787)) 1759—1789

have so far encountered, had the insight and the stamina to pursue. It is from JAMES BERNOULLI's ideas that the further development for this part of the theory of deformable bodies grew.]

While the foregoing account of the first researches on the catenary is complete, the reader may note with some astonishment that *nearly everything that concerns principle is taken from sources that lay unpublished for fifty to one hundred and fifty years*. Indeed, the original papers consist in little else than "constructions", *i. e.* the explanation of a desired curve in terms of properties of possibly more familiar ones. [From the standpoint of mechanics, at least, the first researchers concealed everything they ought have published[1]) and published only what they had better discarded. Nothing illumines more surely the little band of proud, possessive, and mutually suspicious giants who reared the new calculus than that they were content to withhold proofs *indefinitely* while continuing to publish assertions, hints, and quarrels regarding ever broader new researches that even with full explanations would have been understood by at most fifty men in all Europe. Thus it was quite proper] for DAVID GREGORY seven years after the great contest to publish a paper[2]) whose expressed purpose was to supply proofs, using the method of fluxions, for the propositions of HUYGENS, LEIBNIZ, and JOHN BERNOULLI. However, as JAMES BERNOULLI[3]) and LEIBNIZ[4]) hastened to say with respectively characteristic gloom

1) In the case of JOHN BERNOULLI this was surely not from choice but from the terms of the monopoly he had sold to L'HÔPITAL, who from the material bought from BERNOULLI chose to publish under his own name only the parts concerning differential calculus. See O. SPIESS, pp. 135—153 and especially p. 152 of *op. cit. ante*, p. 66, footnote 1.

2) "*Catenaria,*" Phil. trans. **19**, No. 231, August 1697, 637—652 (1698) = Acta erud. July 1698, 305—321. English translation, Phil. trans. abridged **4**, 184—196.

3) The seventh of the "Epimetra" at the end of *Positionum de seriebus infinitis . . . pars quarta,* Basel, 1698 = Opera **2**, 849—867, reads: "DAVID GREGORY's analysis of the catenary curve, recently published in the *Leipzig Acta* for July, shows neatly how it is possible for us to be misled through an inevident and false though plausible argument to a true conclusion."

4) See LEIBNIZ' anonymously published "*Animadversio ad DAVIDIS GREGORII schediasma de catenaria, quod habetur in Actis Eruditorum an. 1698,*" Acta erud. Feb. 1699, 87—91 = LEIBNIZens math. Schriften **5**, 336—339. It is curious to see, in a reversal of the roles traditionally attributed, that while in later parts of the paper the calculus is more or less rightly manipulated by NEWTON's follower, LEIBNIZ has to correct him in the principles of statics. GREGORY's pitiful attempt to salvage his proof is included in "*Responsio ad animadversionem ad DAVIDIS GREGORII catenariam, Act. Eruditorum Lipsiae, Mense Februarii A. 1699,*" Phil. trans. **21**, No. 258, 419—426 (1699) = Acta erud. July 1700, 301—306. English translation, in part, Phil. trans. abridged **4**, 456—458.

As appears from other writings (*e. g.* LEIBNIZens math. Schriften **5**, 418), LEIBNIZ unjustly but understandably attributed the gross errors of GREGORY to the insufficiency of NEWTON's method of fluxions. Indeed, after the long silence of the English regarding the great problems being solved on the continent by LEIBNIZ's method, nothing could have made a poorer appearance than this piece, where the author shows himself unable even to prove correctly results long since obtained, mastered, and improved by the users of the differential method. Whether or not anything on the catenary is to be and ebullience, his argument is wrong[1]) : The attempt to calculate the force acting on a differential element is a failure. [This is one more example to show that the local balance of forces, which nowadays we are all taught to regard as the simplest approach to the mechanics of continuous media, is in fact not an obvious concept.] The first correct proofs to be published, *a full quarter century after the great contest*, are those of HERMANN[2]) and TAYLOR[3]), both of whom treated a wider class of problems. TAYLOR's work, while not as general or as efficient as it might have been, and also not exempt from error, is pleasant to

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found among NEWTON's papers, I do not know, but no modern reader who has followed in detail any of the disguised fluxional proofs in the *Principia* would doubt for a moment NEWTON's own power to solve this problem, and quickly, by fluxions. It would be my conjecture, judging especially by his later performance with the Brachistochrone, that NEWTON found the catenary too easy to distract him from his other occupations. What is most abundantly proved by all this is that unlike LEIBNIZ, NEWTON had no BERNOULLIS.

1) Everything rests on Prop. 1, which derives (22) by means of a fallacious balance of forces on an infinitesimal element, cancelled by an incorrect expression for the tension. We may conjecture that (a) for GREGORY as for anyone who knows calculus, all that was needed was a differential equation; (b) GREGORY searched the papers of 1690 for a differential equation, thus finding (22), which was stated by LEIBNIZ and BERNOULLI but not emphasized by either; (c) GREGORY tried to apply the parallelogram rule to yield (22), but he did not isolate the differential element correctly (failing in fact to see that it is the *difference* of tensions on the two ends that balances the gravity of the element), whereupon he adjusted the tension so as to give the right answer.

2) Lib. I, Sect. I, Ch. III; Lib. II, Ch. IV and Ch. XIII; and also § V of the Appendix in *Phoronomia, sive de viribus et motibus corporum solidorum et fluidorum*, Amsterdam, Rod. & Gerh. Wetstenios, 1716, [xx] + 401 + [ii] pp. The copy in the Basel University Library, the gift of the author, is corrected by him. In § 462 HERMANN says "the solution, or more properly the analysis" of the velaria had not been published up to that time.

It is possible that PARENT published a correct proof in 1700; see footnote 1, p. 80.

According to JOHN BERNOULLI, HERMANN's treatment of flexible curves is faulty. See "*Solutio problematis catenarii generaliter concepti, per methodum Hermanni ab errore repurgatam,*" Opera 4, 234—241 (1742). Here JOHN BERNOULLI obtains the equations in polar co-ordinates. While HERMANN may have made mistakes in his applications (which I have not tried to follow), I can find none in his principles or main equations; the difficulty may lie in failure to realize that his polar co-ordinate diagram must be drawn over again at each point. In § 93 he obtains the general intrinsic equations (40) and (42) by JAMES BERNOULLI's second method (above, p. 81). The equation of normal forces is expressed in terms of the angle of contact, without mention of the radius of curvature, and this may be a further difficulty.

In the EULER collection at Basel is a manuscript (MS III 29 [16c]) dating from some time after 1713 but before 1728, in which JOHN BERNOULLI constructs a catenary subject to the attraction of a fixed center.

3) See Problems XIII—XVI, Props. 18—21 of *Methodus incrementorum directa et inversa*, Innys, London, 1715 and 1717, [vi] + 118 pp. The work was complete in April 1713 (see JOHN BERNOULLI's Opera 2, 474).

read. HERMANN's, though thick and ugly[1]), has the virtue of JAMES BERNOULLI's influence[2]), as shown by the following definition[3]):

"The *tension* or *compression* (tenacitas vel firmitas) of a thread or body at any of its points or at an element of the curve is that force of the thread or body which resists that power or force growing from all the applied powers [*i. e.* loads] and tending by pulling the thread in opposite directions to tear it apart. This tension exactly equals or is equipollent to that tearing force resulting from all the powers applied to the body." [Especially if shortened by the omission of alternative words, this is a perfect definition of the general *line stress*, to be introduced by EULER fifty years later (below, pp. 391—392). However, HERMANN's statement is not so general as it sounds, since he tacitly supposes the tension to be tangent to the curve, as is appropriate to the perfectly flexible case only.

This late and merely derivative publication had its effect on the further development of our subject. On the one hand, the historian, looking at (40), (41), and (42), both in general and in special cases, and regarding their derivations, may say that *the problem of the catenary led almost immediately to sufficient principles and indeed to the general equations, both for fixed and for intrinsic co-ordinates, for a flexible line subject to any loading.* On the other hand, *almost none of this material was generally available*, and much of it had to be rediscovered, especially since TAYLOR's book was incomplete and HERMANN's obscure.]

To finish with the early history of perfectly flexible cords we must note that in a short time the *variational principle* known to all the first investigators (see above, pp. 45, 70, 73), that the center of gravity hangs as low as possible, was reduced to mathematics and shown to yield the same solution, *viz* (32), as obtained by direct methods. This was

1) Not to everyone, for upon receiving the manuscript on 17 September 1715, before the book was published, LEIBNIZ wrote, "I could not restrain myself from rushing through your work with the greatest enjoyment, as if it were a book of stories or romances."

2) In his letter of 29 September 1718, cited above, p. 75, JOHN BERNOULLI writes that HERMANN several times had free access to his teacher JAMES BERNOULLI's posthumous papers and was able to make any use of them he pleased. By his own admission, however, JOHN BERNOULLI was not able to witness any such use, and nothing specific should be concluded.

HERMANN's correspondence with LEIBNIZ certainly gives the impression that HERMANN evolved his results on the catenary slowly and by himself, though they were of course based on the instruction he had received from JAMES BERNOULLI. After passing remarks on 19 March 1707 and 11 January 1711, finally on 27 October 1712 HERMANN writes with pride of having established "a most general proposition, of which the problems of the catenary, velaria, *etc.*, are but special cases." Again on 22 December 1712, ". . . I do not even require the tendencies or impulses, to which the points of curves of this sort are subject, to be only perpendicular to the curves or parallel to an axis, but oblique in any way . . ."

Had HERMANN obtained this material from JAMES BERNOULLI's notes in 1705, he surely would have had no cause to withhold it until 1712.

3) *Phoronomia* Lib. I, Sect. I, Ch. III.

an achievement of JAMES BERNOULLI[1]). [We do not follow it here for two reasons. First, its development is properly a part of the history of the calculus of variations, which has been written by others[2]). Second, as in most cases of variational principles, it furnishes only a detour for mechanics : By the time it was successfully used, the problem of the flexible line had already been solved correctly by direct methods in cases when there is a horizontal as well as a vertical load, and in these cases the variational principle does not hold.]

12. JAMES BERNOULLI's first researches on the elastica (1691—1694). In § 3 of JAMES BERNOULLI's *Addition*[3]) appears an "equally outstanding problem", to which LEIBNIZ had drawn his attention in private letters (above, p. 64) : "the bendings or curvatures of beams, drawn bows, or of springs of any kind, caused by their own weight or by an attached weight or by any other compressing forces . . ." (Figure 30). "But this problem, whether because of the uncertainty of the hypothesis or the manifold variety of cases, seems to be more difficult than [that of the hanging cord], although here it is not a question of lengthy calculation but rather of industry [?]. I have opened the approach to this problem by the fortunate solution of the simplest case (at least, under the aforementioned hypothesis on the elongation). In imitation of that most excellent man [LEIBNIZ], I too will allow others time to try their analysis; I will suppress my solution for the present, and I shall conceal it in an anagram, the key to which, along with the demonstration, I will communicate at the harvest festival."

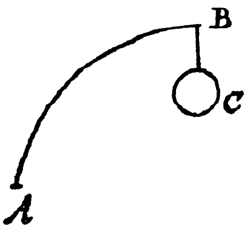

Figure 30. JAMES BERNOULLI's drawing to announce the problem of the elastica (1691)

[The problem of the *elastic band*, or *elastica*, is indeed of a deeper difficulty than that of the catenary[4]).] Not merely a few months but three full years JAMES BERNOULLI held his

1) *Q. D. O. M. B. V. analysin magni problematis isoperimetrici, in actis erud. Lips. mens. Mai. 1697. propositi, sub praesidio* JACOBI BERNOULLI . . ., Basel (1701) = Acta erud. Leipzig, May 1701, 213—228 = JACOBI BERNOULLI Opera 2, 895—920 = JOHANNIS BERNOULLI Opera omnia 2, 219—234. See Problema III. See also No. CCXXXIX of the *Thoughts, notes, and remarks*, cited above, p. 80.

2) R. WOODHOUSE, *A treatise on isoperimetrical problems and the calculus of variations*, Cambridge, 1810. C. CARATHÉODORY, "*Basel und der Beginn der Variationsrechnung*," Festschrift zum 60. Geburtstag von Prof. Dr. ANDREAS SPEISER, Zürich, Füssli 1945, pp. 1—18.

3) Cited above, p. 76. An annotated German translation of § 3 by H. LINSENBARTH is given on pp. 3—4 of *Abhandlungen über das Gleichgewicht und die Schwingungen der ebenen elastischen Kurven*, Ostwalds Klassiker No. 175, Leipzig (1910).

4) In addition to LEIBNIZ's remarks we have HUYGENS' comment in his letter to LEIBNIZ of 16 November 1691: "I cannot wait to see what Mr. BERNOULLI the elder will produce regarding the curvature of the spring. I have not dared to hope that one would come out with anything clear or elegant here, and therefore I have never tried."

secret while no one, not even his brilliant brother[1]), put forward a word on the mathematical theory of elasticity[2]).

In 1694 JAMES BERNOULLI published his solution, *The curvature of an elastic band. Its identity with the curvature of a cloth filled out by the weight of the included fluid. The radii of osculating circles exhibited in the most simple terms; along with certain new theorems thereto pertaining, etc.*[3]). "After a silence of three years I keep my word; but in such a way as right richly to compensate for that delay, which else the reader might have borne with annoyance, since I exhibit the curvature of springs not in one way only (as I had promised in the beginning) but generally for any hypothesis on the elongations; which, unless I err, I am the first to achieve, after the problem was attempted in vain by many." After pointing out the erroneous opinion of GALILEO[4]), the "pure fallacies" of PARDIES, and the "plainly preposterous" argument of DI LANA[5]) on this subject, BERNOULLI continues. "I said . . . that this problem is more difficult than the funicular one, and not without reason. Not to mention other things, I remark that in investigation of the catenary there are two keys, which lead to two different equations, one of which expresses the nature of the curve through its relation to its co-ordinates, the other through a relation between the thread and its evolvent, while for probing the nature of the elastic curve, only the latter key opens the way. Thus, plainly, it is possible that a person might overcome the difficulties

1) JOHN BERNOULLI wrote to DE MONTMORT on 15 June 1719 that he had shown to L'HÔPITAL in 1691—1692 "a very individual analysis of the elastic curve much different from my brother's." According to SPIESS, p. 137 of *op. cit. ante*, p. 66, footnote 1, there exists a paper of this period which served as the first draft for the note JOHN BERNOULLI published fifty years later: "*Solutio problematis curvaturae laminae elasticae a pondere appenso*," Opera omnia 4, 242—243 (1742). The published note interprets the [HOOKE-] LEIBNIZ hypothesis as asserting that the normal relative displacement of infinitely near particles is proportional to the moment of applied force. This is a mere *ex post facto* affirmation of the law (46) in the linear case, leaving nothing to prove.

2) At the end of a paper printed in May 1692, JAMES BERNOULLI wrote, "very soon I will give the curvature of a spring."

3) "*Curvatura laminae elasticae. Ejus identitas cum curvatura lintei a pondere inclusi fluidi expansi. Radii circulorum osculantium in terminis simplicissimis exhibiti; una cum novis quibusdam theorematis huc pertinentibus, etc.*," Acta erud. June 1694, 262—276 = Opera 1, 576—600. Part of this work is translated into German and supplied with helpful annotations by LINSENBARTH, pp. 5—17 of *op. cit. ante*, p. 88, footnote 3.

4) BERNOULLI attributes to GALILEO the contention that the elastic curve is a parabola, but nowhere in GALILEO's works have I been able to find any mention of elastic curves. However, the parabolic form of a beam is included among the "pure fallacies" of PARDIES (above, p. 53).

5) The book of the Jesuit FRANCESCO DI LANA TERZI, *Magisterium naturae et artis*, Brixia, 1684—1692, is long; a cursory search did not reveal either anything concerning elastic beams or anything at all of a definite nature. According to MUSSCHENBROEK, DI LANA "took virtually everything from GALILEO and FABRI, except for certain physical observations of little worth"; also, his experiments he "extracted from his own head, performing none at all" (pp. 427 and 506 of *op. cit. infra*, p. 151).

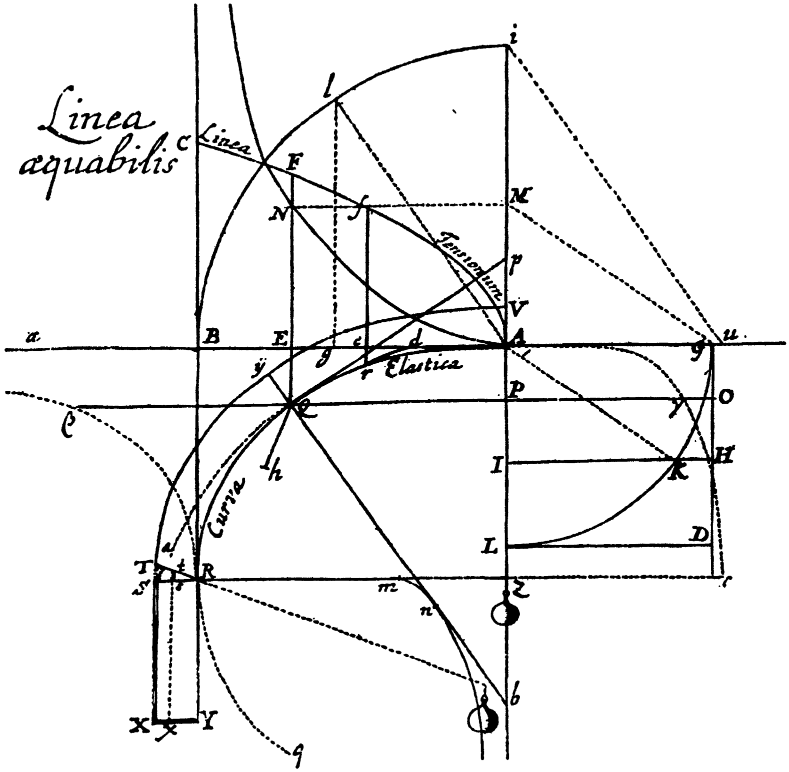

Figure 31. JAMES BERNOULLI's first publication of the elastica (1694)

of the first problem, yet fail to emerge as victor of the second — a person, namely, who lacked the second of the keys, which exhibits ... in simplest and purely differential terms the relation of the evolvent of radius of the osculating circle of the curve. This was already known to us at the time we speculated upon the rope, and on his travels my brother communicated it soon after to some others [*i.e.*, to L'HÔPITAL, VARIGNON, *etc.*]. Meanwhile, since the immense usefulness of this discovery in solving the velaria, the problem of the curvature of springs we here consider, and other more recondite matters, makes itself daily more and more manifest to me, the matter so stands that I cannot longer deny to the public the golden theorem ..."

The "golden theorem" is the general formula for the radius of curvature of a curve[1]).

1) To this both HUYGENS and LEIBNIZ reacted with some sarcasm, since both had been in possession of the "golden theorem" for some time. HUYGENS, for example, had published a statement and proof, quite clear though synthetic, of a result equivalent to the formula in rectangular Cartesian co-ordinates; see Pars Tertia, Prop. XI of *op. cit. ante*, p. 47. JAMES BERNOULLI obtains the forms $\frac{1}{r} = \frac{d^2x}{dy\,ds} = -\frac{d^2y}{dx\,ds}$ and $\frac{1}{r} = -\frac{dx\,d^2y}{ds^3}$ according as s or x is the independent variable. In defense of BERNOULLI's boasting, however, must be adduced the remarks of HUYGENS and LEIBNIZ cited above (pp. 64, 88); both,

For his solution of the problem of the elastica, JAMES BERNOULLI gives a geometrical construction described in terms of the elaborate figure above (Figure 31). There is no proof, but the explanation tells us that the theory is applicable to "a rather long hoop, a stay, a rod, a switch, or any weightless elastic band $AQRSyVA$, of uniform breadth and thickness RS, AV, of length RQA, with one end at RS fixed vertically, and if at the other end AV there acts a force, or if a weight Z is attached there, that is sufficient to curve the band until its tangent at A, namely AB, is perpendicular to the direction of the weight AZ, then the concave side of the band will take on the curvature RQA that we have constructed. The convex side SyV is parallel to it . . ." The "line of elongations" AFC is "any straight or curved line, whose abscissae AE represent the stretching forces, while the ordinates EF give the elongations." [That is, JAMES BERNOULLI introduces an arbitrary *single-valued functional dependence of elongation upon stretching force*[1]). The little springs drawn in the figure at TS and ts suggest that BERNOULLI, following LEIBNIZ, regards the fibres of the beam as extensible, but, unlike LEIBNIZ, he is taking account of the bending which accompanies this extension.]

For explanation of BERNOULLI's ideas we turn to a paper[2]) he published in the next

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despite their knowledge of curvatures, considered the problem of the elastica impossibly difficult. As BERNOULLI replied in § I of *op. cit. infra*, Note 2, "Indeed I knew that that most acute man had not refrained from study of bending, as he himself once mentioned to me in private letters [above, p. 64], and to it the notice of my solution published in June 1691 might have inclined him [*i. e.* again]. I saw indeed that not only was he himself the author of the principle used by me [*i. e.* the elastic law], but also that my calculation built upon it (with the sole exception of the above mentioned theorem [on the radius of curvature]) was so simple, so easy, as will appear from the analysis I subjoin presently, that I should have wronged him much, had I thought he had known the theorem but not gotten the solution."

JAMES BERNOULLI's solution is indeed a masterpiece of higher order than anything published concerning the catenary.

1) This has been remarked by PEARSON, Appendix, Note A (1) of *op. cit. ante*, p. 11. With his usual ability to miss the point of fundamental researches in elasticity, PEARSON criticizes BERNOULLI for not using "the curve obtained by measuring the strains produced in the same rod by a continuously increasing stress." In fact, like most modern investigators of finite deformation, BERNOULLI uses *the actual force in the deformed state.*

2) "*Explicationes, annotationes et additiones ad ea, quae in actis sup. anni de curva elastica, isochrona paracentrica, et velaria, hinc inde memorata, et partim controversa leguntur; ubi de linea mediarum directionum, aliisque novis,*" Acta erud. Dec. 1695, 537—553 = Opera 2, 639—663. See § I. The same argument is given in somewhat clearer and more general form in a note by CRAMER on p. 581 of JAMES BERNOULLI's Opera.

In JAMES BERNOULLI's *Thoughts, notes and remarks* (cited above, p. 80) is no explanation of how he attained the basic idea of the elastica. No. CLXX, probably from late 1691 or early 1692, concerns the quadrature of (49), which, he says, is the elastic curve, "as I will show in due time." Thus JAMES BERNOULLI's published claim of 1691 is substantiated. No. CLXXX contains his first attempt to calculate the numerical bounds (52).

year (we change notations to conform to Figure 31; BERNOULLI's new figure is somewhat clearer in that a differential element of the band at y, with a small spring there, is indicated). "... I consider a lever with fulcrum Q, in which the thickness Qy of the band forms the shorter arm, the part of the curve AQ the longer. Since Qy and the attached weight Z remain the same, it is clear that the force stretching the filament at y (or, what according to the usual hypothesis amounts to the same thing, the elongation itself) is proportional to the segment QP ..." [That is, BERNOULLI regards the entire action of the part $QRSy$ on the part $QyVA$ as equivalent to that of a single spring of tension F at y;] therefore equilibrium of moments requires

$$Fc = xZ \ , \tag{44}$$

where $c = yQ$, the thickness, and $x = QP$. Since c and Z are constant, we have $F \propto x$. If the elastic law relating elongation t to stretching force is $t/b = f(F)$, where b is the length AR of the whole band, we may thus write $t/b = g(x)$, and this is BERNOULLI's "curve of elongations". "And since ... the elongation [of the fibre at y] is reciprocally proportional to Qn, which is plainly the radius of curvature, it follows that Qn ... is also reciprocally proportional to ... x." That is, $(tds/b) : c = ds : r$, or

$$\frac{1}{r} = \frac{t}{bc} \ . \tag{45}$$

[Thus BERNOULLI carefully separates the basic statical principle (45) from the particular elastic law $t/b = g(x)$. Since he replaces the action of all the fibres of a cross-section by that of a single spring on the outer edge, and since (44) gives the moment exerted by this spring about Q as xZ, we may write his combined result in the form

$$\frac{1}{r} = f(\mathcal{M}), \qquad \mathcal{M} = \text{Bending Moment}, \tag{46}$$

defining *a general, non-linear theory of elastic bands*. The form (46), however, is not that in which BERNOULLI presents his result, nor was it at first so interpreted. BERNOULLI uses the form (45), in which appears the *extension* t/b of the outermost fibre, not only independent of the extensions of the other fibres but in disregard of them. Contrary to the expectation raised by the second spring in the figure, he does *not* integrate over the cross-section of the band. Thus (45) expresses the curvature of the innermost filament in terms of the extension of the outermost. Comparing BERNOULLI's own form (45) with the alternative (46), we may say that he wrought better than he knew. For to introduce the radius of curvature, he considered the extension of one particular fibre. This is sufficient to derive (46), but not convincing. What is lacking is an *integration* over the cross-section, such as that LEIBNIZ had effected in a context he interpreted either as neglecting the bending or as applying to a beam that assumes a straight form when loaded (above, pp. 62, 64).

TODHUNTER[1]) has criticized JAMES BERNOULLI for considering only the equilibrium of moments while neglecting the equilibrium of forces. This criticism is just in one context, ill taken in another. Indeed, the tragic flaw of BERNOULLI's conception, the flaw which will cause him time and again just barely to fail of establishing his theory properly and fully, is his vacillation between the one-dimensional elastic *curve* and the three-dimensional elastic *beam*. From the one-dimensional standpoint, a law such as (46) must be *postulated;* by the principle of moments, the form of the band is then determined; by EULER's general equations (562), below, to consider the equilibrium of forces serves only to determine the line stress, in which we have no great interest, and TODHUNTER's criticism falls. From the three-dimensional standpoint, (46) is to be *derived* from the nature of the forces acting within the beam, and in this context TODHUNTER's criticism is pertinent. JAMES BERNOULLI, as we shall see, was never willing to face this second problem squarely even though the special work of LEIBNIZ might have served as a hint. Upon this point will be focused later researches by PARENT, EULER, JOHN III BERNOULLI, and COULOMB.]

JAMES BERNOULLI substitutes the general formula $\frac{1}{r} = -\frac{d^2y}{dx\,ds}$, s being the independent variable, into (45) and obtains

(47) $$-\frac{dy}{ds} = \frac{S}{bc}\,, \qquad S \equiv \int_0^x t(\xi)\,d\xi\,,$$

since it is assumed that $\frac{dy}{ds} = 0$ when $x = s = 0$. Hence

(48) $$dy = \frac{S\,dx}{\sqrt{b^2c^2 - S^2}}\,.$$

From this formula, the geometrical construction is easily derived [but is of no interest].

Returning to the paper of 1694, the unfortunate reader of which had to create for himself all the essential principles we have just described, we find a number of remarks:

1. If the band is clamped at any point Q and the part RQ is cut away, the remaining part AQ retains its figure.

2. If RQA is rotated about RZ and clamped at any point q, the same force Z causes the resulting band to retain its figure.

3. If any section AQ is rotated about the normal Qn, the resulting band, composed of two congruent parts, is caused to retain its figure by equal and normal forces Z applied at its ends, provided it is held at Q. The same holds for staves obtained by rotating the whole

1) § 24 of *op. cit. ante*, p. 11.

curve AQR or the curve as supplemented by Rq. "Thus one obtains three kinds of staves: the diminished, the complete, and the extended . . . For the diminished stave, the tangents at the ends intersect on the convex side, for the extended stave, on the concave side, while for the complete stave, they are parallel.

"4. This same curvature is proper to the staves from which barrels are made. Thence it follows that no one has correctly measured the capacity of barrels, since these are usually taken as ellipsoids of revolution . . .

"5. If the direction of the weight . . . is skew to the elastic band . . ., there results a curve a little different from AQR, and this curve I can determine just as easily. But I do not wish to dilate.

"6. The rectangle made by the radius of curvature Qn and the corresponding abscissa EF equals the constant area $ABC = AG^2$." [This we recognize, in BERNOULLI's typical style hidden in the midst of "scholia and corollaries", as a verbal statement of the basic statical principle (45). It is stated again in a special case as the fourth remark following (49).] Since $t(0) = 0$, we see that the curvature is zero at the free end and greatest at the clamped end, "at least in the case when the elongations increase with the stretching forces . . ."

7. If we know the law of elongation and are to find the elastic curve, "in abstract geometry this is nothing else than to determine the curve AQR from the given curve AFC." By (45), the inverse problem is trivial.

JAMES BERNOULLI gives some attention[1] to the parabolic law[2] $t = kx^m$; then he takes up the linear case, $m = 1$. [Though these laws as stated seem artificial, recall that x is proportional to the stretching force F, as shown above, and hence BERNOULLI is in effect assuming that strain $\propto$ (force)^m.] Then (48) becomes

$$dy = \frac{x^2 dx}{\sqrt{c^4 - x^4}}, \qquad c = \text{const.} \tag{49}$$

This quadrature may be achieved by a construction.

After futile attempts to express this curve in terms of exponential functions, "I have

1) While the modern reader will admire BERNOULLI's careful separation of the particular elastic model from the general principles of the problem, TODHUNTER (§ 24 of *op. cit. ante*, p. 11) typically describes the investigation as "more elaborate than necessary" because BERNOULLI does not descend at once to the linear case.

2) Historians of elasticity do not seem to have noticed that this is the first non-linear law of elasticity to be proposed in print; *cf.* the suggestion of LEIBNIZ, above, p. 63. The most extensive list of special elastic laws is given by R. MEHMKE, "*Zum Gesetz der elastischen Dehnungen*," Z. Math. Phys. 42, 327—338 (1897). MEHMKE mentions only three from our period: HOOKE's law (18), the parabolic law above (which MEHMKE attributes to BÜLFFINGER, *cf.* below, p. 103), and an inexact form of RICCATI's law (81).

heavy grounds to believe that the construction of our curve depends neither on the quadrature nor on the rectification of any conic section." There follows a list of eighteen properties of the curve, mainly geometric. No. 2 is that described in the anagram published in 1691. No. 16 gives series for the displacement $y(c)$ and the arc $s(c)$ at the end, c being the length:

$$(50)\qquad \begin{aligned} \frac{y(c)}{c} &= \int_0^1 \frac{\xi^2 d\xi}{\sqrt{1-\xi^4}} = \tfrac{1}{3} + \sum_{n=1}^{\infty} \frac{(2n-1)!!}{2^n(4n+3)n!}\,, \\ \frac{s(c)}{c} &= \int_0^1 \frac{d\xi}{\sqrt{1-\xi^4}} = 1 + \sum_{n=1}^{\infty} \frac{(2n-1)!!}{2^n(4n+1)n!}\,. \end{aligned}$$

(From his manuscript notes[1]) and from a later publication[2]) we know that JAMES BERNOULLI had integrated term by term in the power series expansion of the integrands, obtaining

$$(51)\qquad \begin{aligned} \frac{y(x)}{c} &= \tfrac{1}{3}\left(\frac{x}{c}\right)^3 + \sum_{n=1}^{\infty} \frac{(2n-1)!!}{2^n(4n+3)n!}\left(\frac{x}{c}\right)^{4n+3}, \\ \frac{s(x)}{c} &= \frac{x}{c} + \sum_{n=1}^{\infty} \frac{(2n-1)!!}{2^n(4n+1)n!}\left(\frac{x}{c}\right)^{4n+1}.) \end{aligned}$$

From (50), BERNOULLI has calculated the bounds

$$(52)\qquad 0{,}598 < \frac{y(c)}{c} < 0{,}601\,, \quad 1{,}308 < \frac{s(c)}{c} < 1{,}316\,.$$

Remark No. 18 states the identity of the lintearia with the [rectangular] elastica and asserts five properties, of which the last is a variational principle: "... among all curves of a given length drawn over the same straight line the elastic curve is the one[3]) such that the center of gravity of the included area is the farthest distant from the line, just as the catenary is the one such that the center of gravity of the curve is the farthest distant ...

"It would remain now, under the common hypothesis regarding the elongations, to investigate the kind of curves engendered when the elastic band is bent by its own weight in addition to the suspended weight; if it is bent simultaneously at each end; if its thickness or breadth is not uniform or, for example, if it is of triangular shape or any other and

1) No. CLXXV of the *Thoughts, notes, and remarks* (cited above, p. 80), written late in 1691 or early in 1692. The numerical bounds are obtained in No. CCXVII; the quadratures are studied in No. CCV, the end of No. CCVII, and the second of the sections numbered CCXXVIII. The elastica as a variational problem is mentioned but not properly treated in No. CCXXXIX.

2) § LVI of *Positionum de seriebus infinitis ... Pars quinta*, Basel, 1704 = Opera 2, 955—975.

3) As BERNOULLI remarked later, he means here to restrict attention to curves of a fixed length. See p. 836 of the reprint in JAMES BERNOULLI's Opera, p. 227 in JOHN BERNOULLI's, of *op. cit. ante*, p. 82, footnote 2.

if the bending force is applied first at the apex, then at the base. Also, what should be the curvature of the band in order that from an attached load or from its own weight or from both together it assume the form of a straight line (this would be useful in designing the arms of balances and axles, where it is required that the centers of the motion and of the suspended bodies be collinear). Also, what shape should be given to a band in order that through bending it take on a given curvature, and a thousand other things of this kind. Of all these curves I can exhibit the characteristic properties, and of some even the constructions . . . but many things I have not yet assimilated, nor is it given to one person to work at all things. Besides, something should be left to the industry of our readers, for whom there is thus ample opportunity to complete our discovery."

[It is difficult to find words to describe the power and beauty of this paper. Among other researches on materials published in the seventeenth century, only NEWTON's essays on fluids might be compared to it. By this, JAMES BERNOULLI at once regained the superiority he had temporarily lost when overtaken not only by LEIBNIZ and HUYGENS but also by his quick and brilliant younger brother JOHN in the matter of the catenary. The form of the elastic band, the deepest and most difficult problem yet to be solved in mechanics, is his alone.]

13. JAMES BERNOULLI's attempts toward a theory of the neutral fibre (1695—1705). LEIBNIZ, generous as usual, recognized at once what JAMES BERNOULLI had done; in particular, he praised him for avoiding special hypotheses and considering a general law of elongation[1]).

HUYGENS was not enthusiastic. In a letter to LEIBNIZ of 24 August 1694, part of which, with its expression somewhat softened, was quickly published[2]), he wrote, " I find Mr. BERNOULLI's three years' work quite considerable, provided that all he contends is true; also he boasts much over it. As for the principle of the spring, I think he has used it well, and that it is true that the rays which measure the curvature are in the inverse ratio of the forces that bend the spring, although, in my opinion, it is not only the exterior surface that extends but also the interior one simultaneously shortens . . . If this principle were not the unique and true one, but rather the line AFC were a curve depending on

1) See the second paragraph of LEIBNIZ's "*Constructio propria problematis de curva isochrona paracentrica,*" Acta erud. August 1694, 364—375 = JACOBI BERNOULLI Opera **2**, 627—637. In his letter of 27 July 1694 to HUYGENS, with which he inclosed BERNOULLI's paper in print, LEIBNIZ writes, "I think it is always true that the elongations are as the forces, but it is not always right to take the elongations as the changes of length in the body, because they depend rather on the changes of solid content . . .," but instead of pursuing this penetrating line of thought, which might have led to a concept of local strain, he gives reasons for being personally unwilling to study elasticity any further.

2) "*Excerpta ex epistola C. H. Z. ad G. G. L.,*" Acta erud. Sept. 1694, 339—341 (second pagination) = JACOBI BERNOULLI Opera **1**, 637—638 = Œuvres complètes de HUYGENS **10**, No. 2874.

infinitely many experiments, I should find all his research very vague and little worthy of time spent. And even now all he has found seems of no use to me, but only such very beautiful and subtle pastimes as one finds when one has nothing on which to employ mathematics more fruitfully.

"It is a strange assumption to take the quadratures of every curve as given, and if the construction of a problem ends with that (apart from the quadrature of the circle and the hyperbola), I should think nothing accomplished, since even mechanically one does not know how to carry anything out . . .

" . . . Mr. BERNOULLI has determined the curvature of the arc A only in the case when the tangents at the ends E, F are parallel, which I consider joined by the string EF (Figure 32). It would remain to give the form of the true arc B; again, of C, the extremities of which point toward one another; of D, where they come together, and of G, where they pass beyond and are held by a rod HI." He goes on to express his doubts of BERNOULLI's results until he sees the proofs[1]).

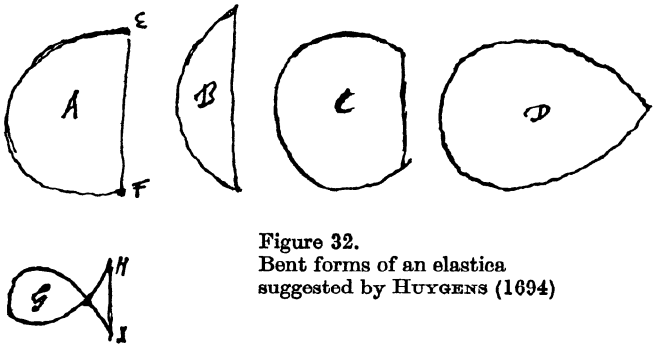

Figure 32.
Bent forms of an elastica suggested by HUYGENS (1694)

To these criticisms BERNOULLI responded in his usual gloomy and massive style[2]). "Since what I published in recent years . . . the illustrious geometers Mr. HUYGENS and Mr. LEIBNIZ have deigned to subject to special examination, where some parts they have approved, others, more hidden in statement, they have augmented by conjectures, while here they have raised scruples and there they have expressed their open disagreement, I have decided to add some later thoughts to the former ones, and to explain with order and candor the several matters as they appear to me, so as both to satisfy the wishes of those most famous men and also to bring the purer sparks of truth from the hidden recesses of nature more and more into the daylight . . .

"That the radii of curvature are inversely as the stretching forces (more truly, as the elongations), which both those very famous men consider me to have used as a beginning, is learned from the equation first discovered [*i. e.* (45)] and is a conclusion rather than the beginning, as I said distinctly among the corollaries, see Corollary 6 of the first construction

1) In his letter of 14 September 1694 to HUYGENS, and also in an addition to the publication of HUYGENS cited in the preceding footnote, LEIBNIZ suggests that cases $BCDG$ can be obtained by extending the curve A or by taking only a part of it. This is false.

2) *Op. cit. ante*, p. 91, footnote 2.

and the fourth Corollary of the third construction (above, p. 94). The principle I in fact used assumes that any point on the concave surface of the spring may be regarded as the fulcrum of a certain lever. This principle is the same as that introduced by the most acute LEIBNIZ . . . (see p. 61, above). Thus if Mr. HUYGENS felt some doubts concerning it, thinking that not only is the outer surface extended but also the inner contracted, he should have made this objection to Mr. LEIBNIZ, not to me, who only adopted this ten years later from the author of the principle. But I admit that when I first thought about this matter the same objection came to mind, since anything susceptible of extension should be susceptible also of compression."

JAMES BERNOULLI proceeds to analyse the bending of a beam such that the upper fibres are extended, the lower ones contracted. In his figure (Figure 33) we see that TSA,

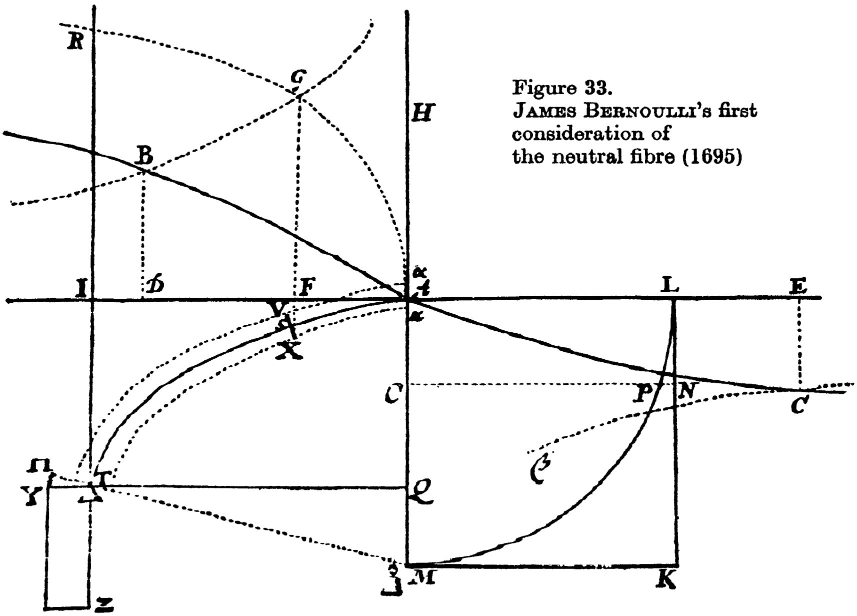

Figure 33.
JAMES BERNOULLI's first consideration of the neutral fibre (1695)

"the line of fulcra", is the *neutral fibre*[1]), while the curve BAC is the curve giving the elongation BD as a function of the stretching force AD and the contraction EC as a function of the compressing force AE. " . . . the part AC should have an asymptote parallel to the axis AE, since nothing can be compressed more than its total length; thus, plainly, all sorts of parabolic or hyperbolic lines and the straight line itself are excluded."

1) Historical writers always fix upon someone else to whom to attribute the concept of the neutral fibre. This is its first *explicit* appearance. However, the existence of such a fibre is implied by the statement that the outer fibres are extended while the inner ones are contracted, which had been positively asserted by BEECKMAN (1620), HOOKE (1678) (above, pp. 27, 55), and HUYGENS (1694) (above, p. 96), possibly also by MARIOTTE (1684) (above, p. 60), and is in any case sufficiently obvious, though difficult (for that time) to use profitably in a mathematical theory.

In this paper JAMES BERNOULLI gives only his conclusions, without analysis. He assumes that half of the bending moment is used for extending the upper fibres, the other half for compressing the lower ones[1]). "It can be established . . . that if the curve of extensions *A B* and the curve of compressions are similar and like curves . . ., as for example if *BAC* were a straight line . . ., the construction from here on will agree with that which I published in June 1694, excepting only for the quantity of the applied weight, and that the line of fulcra *AS*, which was there put on the concave side of the spring, is now in the very middle . . ."

[These conclusions are clear and entirely correct: (A) The lever arm of the applied weight, at each cross-section of the beam, is its *distance from the neutral line*, and (B) If the fibres respond symmetrically to push and pull, *the neutral line is the central line*. These results are usually attributed to later authors, perhaps with some justice, since BERNOULLI has obtained them, as we shall see now, only in consequence of a wrong hypothesis, namely, that the stretched and shortened fibres contribute equal shares to the bending moment.]

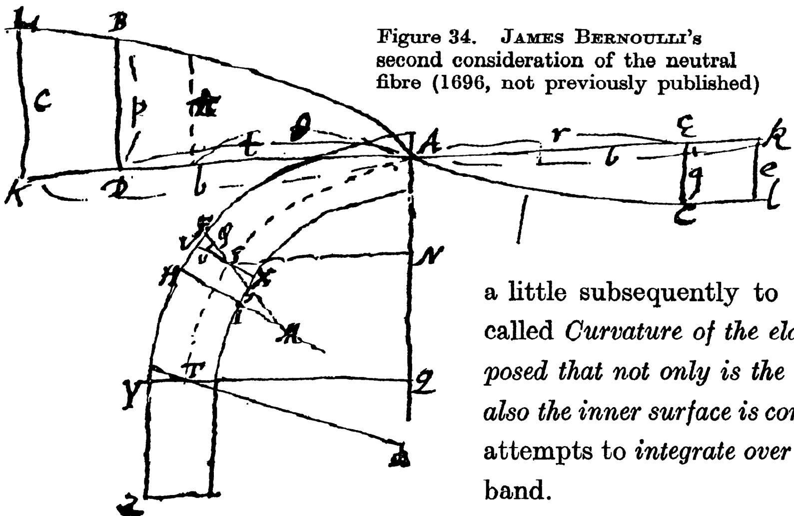

Figure 34. JAMES BERNOULLI's second consideration of the neutral fibre (1696, not previously published)

To learn JAMES BERNOULLI's reasoning, we turn to No. CCL of his *Thoughts, notes, and remarks*[2]); this note, written after 1696 and hence a little subsequently to the published paper, is called *Curvature of the elastic band when it is supposed that not only is the outer surface extended but also the inner surface is contracted*. Here BERNOULLI attempts to *integrate over the fibres* making up the band.

In Figure 34, "Let the band *TSA* be curved

1) This appears to be the only basis for the explanation supplied by CRAMER on pp. 643—644 of JAMES BERNOULLI's *Opera*. In this note all the extended fibres are represented by one spring on the convex side of the beam, all the contracted ones by another on the concave side: "Many things induce me to suspect that the analysis of the author was not very different from this. Nevertheless, I should not like to assert it positively, lest I should seem to attribute to him a solution laboring under not one defect only, such as is that substitution of the [outermost] fibre *rV* for all the extended ones and of the [innermost] fibre *qZ* for all the compressed ones. When he noticed this, he undertook to consider the problem again" [in the memoir analysed below, pp. 105—109]. In the surviving papers of JAMES BERNOULLI the only indication that he used so crude a model is a sentence we quote below (p. 105) from his paper of 1705.

2) Cited above, p. 80.

by the weight ab attached at A, and the little convex part HV extended to F, while the concave IX is contracted to G, so that VSX acquires the location FSG, which produced meets HI [produced] at the center of curvature M. The point to be found, or S, lies in the line VSX, with the property that as much force [*i. e.* moment] is available for extending the part of the band HS through $\triangle VSF$ as for compressing the other part through $\triangle SGX$." The segment $v\varphi$ is the elongation of a typical fibre at the element ds; thus $v\varphi = \dfrac{\pi\, ds}{a}$, where a is the thickness HI, and where π is a certain function of the stretching force θ given by the upper curve: $\pi = \pi(\theta)$. Setting $f \equiv VS$, the thickness of the stretched part, we have $Sv \equiv \xi = \dfrac{f\pi}{p}$, where p is the value of π at the convex side; with q the counterpart for the concave side, we have $VS \equiv f = \dfrac{ap}{p+q}$, $GS \equiv g = \dfrac{aq}{p+q}$. The moment exerted by the fibre $v\varphi$ about S is then $\theta\xi\, d\xi = \dfrac{f^2}{p^2}\,\theta\pi\, d\pi$. Thus for the total moment $\mathcal{M}_s$ exerted by the stretched fibres we have

$$\text{(53)} \qquad \mathcal{M}_s = \frac{f^2}{p^2}\int \theta\pi\, d\pi = \frac{a^2}{(p+q)^2}\int \theta\pi\, d\pi \ ,$$

and a similar result holds for the moment $\mathcal{M}_c$ of the compressed fibres. BERNOULLI'S hypothesis, stated above, is $\mathcal{M}_s = \mathcal{M}_c$. Whatever the form of the curve of tensions, this fixes the point S [which he is later to call the *center of tension*] where the neutral fibre meets the cross-section; in particular, in the case of an odd curve of tensions the point S is the midpoint. To relate these results to the radius of curvature, BERNOULLI observes that $VF/VS = HV/SM$; this gives $SM = r = \dfrac{a^2}{p+q}$.

[To understand this near miss and the complexity of the analysis, we remark first that BERNOULLI wishes *to avoid any hypothesis regarding the tensions of the fibres.* Every word shows that he is thoroughly familiar with the linear case and its properties. Instead, however, of balancing the force, as is necessary (*cf.* the criticism of TODHUNTER, above, p. **93**), he proposes the special condition $\mathcal{M}_s = \mathcal{M}_c$. On the one hand, there is nothing to recommend his condition; on the other, no modern treatment attempts this degree of generality. Far from being unaware of the problem of location of the neutral line, BERNOULLI attempts to solve it! (Recall that in modern theories of bending, such as ST. VENANT'S, the location of the neutral line is in effect assumed, not demonstrated.)

Disregarding the condition $\mathcal{M}_s = \mathcal{M}_c$, let us retrace JAMES BERNOULLI'S argument. If $t\,d\xi = f(\epsilon)\,d\xi$ is the stretching force on a typical fibre of height $d\xi$ in the cross-section, ϵ being the strain, from the above equations we infer the correct result

$$\text{(54)} \qquad \mathcal{M} = \int t\xi\, d\xi \ .$$

But this is not enough. What is missing is the geometrical relation $\epsilon = \xi/r$, by which the strain ϵ of a *typical* fibre, not merely that of a fibre on the surface, is related to r. From (54) now follows

$$\mathcal{M} = \int f\left(\frac{\xi}{r}\right)\xi\, d\xi = K r^2 F\left(\frac{a}{r}\right), \tag{55}$$

where K is a modulus having the dimensions of [Force]/[Length], and where F is a dimensionless function which in the case of the linear elastic law reduces to a multiple of $\left(\frac{a}{r}\right)^3$. Thus JAMES BERNOULLI's argument again leads to a *general, non-linear theory of elastic bands*, for (55) is the inverse of (46). This time, however, his program is more ambitious; an essential step is missing, and the result (55) may not be regarded as fully established by him.

No other problem we shall discuss in this history is as difficult as this one, which remains today unsolved[1]), nor shall we encounter any other scientist who approaches it. The results JAMES BERNOULLI obtained here, while incomplete and partly unjustified, may serve as measures of the man.]

In the paper published in 1695 BERNOULLI remarks that as regards the forms of elastic curves in Figure 32 HUYGENS, [as sometimes happens with senior scientists,] had not read carefully the work he criticized. "But since I see that those most acute men have expressed so many conjectures on this subject, it will be worthwhile to treat the whole matter openly." From the very beginning (above, p. 94), BERNOULLI claimed to give only a special case, and in his [third and] fifth scholia he mentioned expressly the other possibilities. Moreover, his general argument is easily adjusted to cover these cases: We need only supply in (47) the constant of integration there set equal to zero. [Thus follows

$$dy = \frac{(S + C)dx}{\sqrt{a^4 - (S + C)^2}}, \quad] \tag{56}$$

which BERNOULLI gives in the linear case, *viz*

$$\pm\, dy = \frac{(x^2 \pm ab)dx}{\sqrt{a^4 - (x^2 \pm ab)^2}}. \tag{57}$$

[On this differential equation are founded all later researches on the inextensible elastica.] BERNOULLI notices another possible form (Figure 35), for which the bending force is applied not directly to the band itself but at the ends of the rigid rods AF and CG; this loading is suggested in the interpretation of the lintearia, where fluid fills not only the cloth ABC but also the space between the rigid walls AF and CG. [This device allows the moment to be adjusted inde-

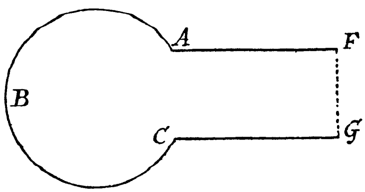

Figure 35.
JAMES BERNOULLI's conception of a band bent by couples (1695)

1) *I. e.*, to determine the law of bending and to locate the neutral line when the fibres obey an arbitrary stress-strain relation.

pendently of the force; *i. e.*, it visualizes a couple in addition to the moment of a force[1]).]

Ten years after the appearance of JAMES BERNOULLI's second paper was published a work by VARIGNON[2]), [where we find no new ideas but a somewhat fuller and clearer mathematical treatment. VARIGNON follows JAMES BERNOULLI] in using at first an arbitrary law of the tension as a function of the distance from the lower side of the beam; [thus it is his merit to separate, explicitly and clearly, the purely statical problem from the particular elastic hypothesis. The subject, however, is only GALILEO's problem of rupture (above, p. 38), not BERNOULLI's problem of bending,] and all VARIGNON does is to carry out more explicitly the integration over the cross-section [which LEIBNIZ executed in a typical though not clearly explained special case (above, pp. 61—62). At that, the analysis of VARIGNON is unnecessarily complicated; the same line of thought was later put

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1) Here we describe some subsidiary researches given at about this time in JAMES BERNOULLI's *Thoughts, notes, and remarks* (cited above, p. 80).

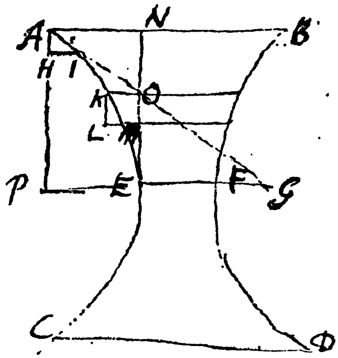

Figure 36. JAMES BERNOULLI's sketch for a theory of an elastic fabric or membrane (c. 1695)

No. CXCVIII, "To find the shape of a stretched rectangular cloth AD," contains the earliest attack upon a two-dimensional elastic problem. A fabric of threads (Figure 36) is pulled apart by the rigid rods AB and CD. The hypotheses are not clear. The equation is "$HI:LM = AN:KO$, that is, the differentials are as the integrals." With ANB as the y-axis, the reasoning seems to consist in observing that the y component of tension in the thread AE is then $T\dfrac{dy}{ds}$, and this is equated to a force of extension of the cross threads of amount Ky, or, more reasonably, $K(a+y)$, where a is a constant. This gives $T\dfrac{dy}{ds} = K(a+y)$. This note is prior to JAMES BERNOULLI's development of the general idea of tension; he seems to take T here as a constant, which it cannot be.

No. CCLI and an entry added later to No. CCLXXII were printed in slightly expanded form as No. IX, pp. 1030—1032, of JAMES BERNOULLI's *Varia Posthuma*, Opera 2 (1744). The question is, "whether a taut spring when the stretching force is released restores itself simultaneously to straightness in all its parts, or whether in some parts more quickly, in others more slowly?" While NICHOLAS I BERNOULLI, the editor, seems to follow the argument, I cannot. The beam is regarded as a set of parallel and not coupled fibres, which are extended by different amounts; the stretching forces are arbitrary functions of the elongations. BERNOULLI concludes that "the bow springs back more quickly in the parts enjoying a greater curvature . . ."

No. CCLXXIII, *Discovery of the center of tension*, appears as *Varia Posthuma* No. XXVI, pp. 1105—1108. Here the line of fibres is attached to a rigid hinge. The center of tension is defined as above, p. 100. The argument, connected with the foregoing, is obscure.

2) "*De la résistance des solides en général pour tout ce qu'on peut faire d'hypothêses touchant la force ou la tenacité des fibres des corps à rompre; et en particulier pour les hypothêses de* GALILÉE *et de* M. MARIOTTE," Mém. acad. sci. Paris 1702, 4to ed., Paris, 66—94 (1704) = 2nd. 4to ed., Paris, 66—100 (1720).

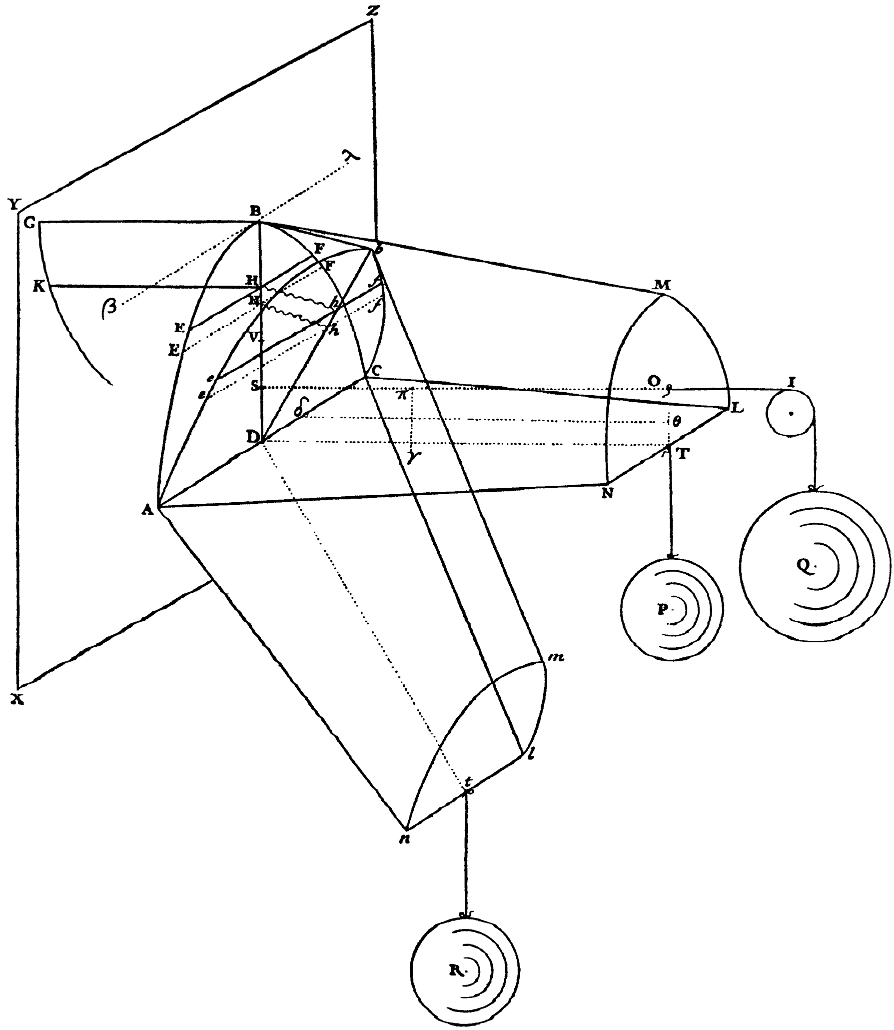

Figure 37. Varignon's analysis of the forces acting upon a cross-section of a loaded beam (1702)

more directly by Bülffinger[1]).] In Varignon's figure (Figure 37) the typical fibre Hh is extended, and it is assumed that at a certain load $K = BG$ a unit fibre will break. Integration yields Galileo's result (12) for the absolute resistance P_t. To calculate P_b, which Varignon later (§ XIII, 4°) calls the "relative resistance", set $HH = dx$, $EF = y$, II—IV
$DH = x$, and let $KH = F(x)$ be the tension in a unit fibre at the height x. Since the

1) "*De solidorum resistentia specimen,*" Comm. acad. sci. Petrop. 4 (1729), 164—181 (1735). See § 13, where Bülffinger criticizes the "detours" of Varignon. Bülffinger's paper is a just, scholarly, and critical exposition of the subject as it stood in 1729, with the unfortunate omission of Parent's best work. In the current Russian literature Bülffinger is often cited as the author of the power law of elasticity that was in fact introduced by Leibniz in a special case, by James Bernoulli in general, and was used by Varignon.

uppermost fibre $x = D$ breaks when the tension is K, we have $F(D) = K$, as indicated in the drawing. Balance of moments about the fulcrum AC yields

$$P_b l = \int F y x dx = \int F x dA \ , \tag{58}$$

where $l = DT$, the length of the beam. These results constitute VARIGNON's "Fundamental Rule".

VII For GALILEO's hypothesis of rigidity up to fracture, we have $F = K$, and (58) becomes

$$P_b l = K x_0 A \ , \tag{59}$$

where x_0 is the distance of the center of gravity from the axis AC. Therefore

$$\frac{P_b}{P_t} = \frac{x_0}{l} \ , \tag{60}$$

yielding GALILEO's formula (11) in all cases when the base is symmetrical about the horizontal line through the midpoint of DB, so that $x_0 = \frac{1}{2}D$.

VIII—X On the [HOOKE-] MARIOTTE-LEIBNIZ hypothesis we have $F = Kx/D$, and (58) then yields LEIBNIZ's formula (20) in the explicit and general form[1])

$$M = P_b l = \frac{K}{D} I \ . \tag{61}$$

For a rectangular cross-section this reduces to LEIBNIZ's result $P_b/P_t = \frac{1}{3}D/l$.

[True, VARIGNON tacitly supposes that *the fibres on the concave side are unextended,* but this error does not invalidate the argument : placing the neutral fibre where we will, we still derive (61), where I is taken with respect to the unstretched fibre.

XII Following JAMES BERNOULLI,] VARIGNON considers also the power law $F = K(x/D)^m$. Under this assumption, (58) becomes

$$P_b l = K D^{-m} \int x^{m+1} dA \ ; \tag{62}$$

[as was to be remarked by BÜLFFINGER[2]), for a rectangular cross-section this yields

$$\frac{P_b}{P_t} = \frac{1}{m+2} \cdot \frac{D}{l} \tag{63}$$

and thus by choice of m yields any numerical factor desired[3]).

1) VARIGNON expresses this result in terms of the center of percussion.

2) § 14 of *op. cit. ante* p. 103.

3) BÜLFFINGER seems to be the first since LEIBNIZ (above, p. 63) to have tried to compare a non-linear elastic relation with experimental data. He says that $m = \frac{3}{2}$ fits the experiments of MARIOTTE for wood; $m = \frac{4}{3}$, for glass. The former value fits also JAMES BERNOULLI's experiments on

Most of the rest of the paper consists in applications of the fundamental rule. In particular, at least for rectangular cross-sections we have $P_b \propto AD/l$ according to both hypotheses, and hence the forms of solids of equal resistance will be the same for both, [as LEIBNIZ had asserted. Indeed, we now see that this result follows by dimensional analysis, so long as bending is neglected.] XIII—XXIV XXI

Similar reasoning yields a rule for a beam broken by loads applied at both ends, and similar corollaries follow. XXV—XXXIV

[Thus VARIGNON succeeds only in putting into somewhat more explicit and general form the ideas of LEIBNIZ, applying statical principles correctly but neglecting the bending of the beam.]

Like HUYGENS before him, JAMES BERNOULLI leaves our scene in the grasp of the same problem with which he entered it; moreover, his last work concerns the topic which first drew him into the higher analysis, namely, the strength of a beam. A few months before his death he finished his last paper, *True hypothesis on the resistance of solids, with a proof of the curvature taken on by bodies acting as springs*[1]). He writes that his own work of 1695 is "rather imperfect, considering ... only the fibres on the exterior of the bent surface, while in fact one must take account of all the fibres going to make up its thickness ...

"Lemma I. Fibres of the same material and the same greatness, or thickness, drawn or pressed by the same force, stretch or shrink proportionally to their lengths." [*I.e.*, if l is the length and Δl the elongation or change in length, we have

$$\epsilon \equiv \frac{\Delta l}{l} = f\,(\text{material}, A, F), \quad \begin{array}{l} A = \text{cross-sectional area,} \\ F = \text{stretching weight.} \end{array} \tag{64}$$

gut strings. See §§ 17—20 of BÜLFFINGER's paper. In §§ 25—33 he goes on to suggest (1) the laws of compression and extension may be different, [as had been contended but later retracted by JAMES BERNOULLI, see pp. 99, 106—107,] (2) the position of the neutral line should enter the theory as a parameter, to be adjusted so as to fit measured values of P_b/P_t, and (3) the resistance of the cross-section should be measured by the moment of the stretched fibres about still a third point, neither the bottom fibre nor the neutral one. *Cf.* the prior researches of PARENT, § 14, below.

1) "*Veritable hypothese de la resistance des solides, avec la démonstration de la courbure des corps qui font ressort ... Lettre du 12 mars 1705*, Mém. acad. sci. Paris 1705, 4to ed., Paris, 176—186 (1706) = 2nd 4to ed., Paris, 176—186 (1730) = 12mo ed., Amsterdam, 230—244 (1707) = Opera 2, 976—989. No. CCLXXX of the *Thoughts, notes, and remarks* (cited above, p. 80) is a preliminary version; our Figure 38 is taken from it.

No. CCLXXXII, which just precedes a piece dated "1 9ber 1704", applies the theory of the elastica to determine the form of a cam such a thread wound around it and attached to the end of a leaf spring exerts a constant torque on the cam while unwinding. This piece appears as No. XXVIII, pp. 1115—1118, of the *Varia Posthuma*. On p. 337 of his work on the spring (cited above, p. 54), HOOKE had written, "It will be easie to calculate the proportionate strength of the spring of a Watch," *etc.*, but of course HOOKE, who in any case was given to pronouncing as "easie" calculations quite beyond his own powers, assumed a linear relation between force and deflection.

This is the first explicit appearance of the *strain* since BEECKMAN's assertion (7), but JAMES BERNOULLI, as we have seen, has used (64) implicitly and without comment in his earlier proofs. The proposition (64), of course, is a postulate; the alleged "proof", while circular, is plausible, akin to that given by GALILEO and MARIOTTE to show that a long cord and short one break under the same load.]

"Lemma II. Fibres that are homogenous and of the same length but of different greatness or thickness are stretched or shrunk equally by forces proportional to their greatness." [*I. e.*,

$$\Delta l = f\left(\text{material}, l, \frac{F}{A}\right) . \tag{65}$$

This is the first explicit appearance of the *mean elastic stress* F/A; *cf.* GALILEO's formula (12) for rupture. The "proof", again, but restates the lemma. Combining (65) and (64) yields the assertion that for a given material

$$\epsilon = f(\tau), \quad \text{where} \quad \epsilon \equiv \frac{\Delta l}{l}, \quad \tau \equiv \frac{F}{A}; \tag{66}$$

thus BERNOULLI is the first to introduce a *stress-strain relation* as distinct from a formula such as HOOKE's for elongation Δl as a function of applied force F. This, too, must not be exaggerated, since still more than a century ahead lie the *local* concepts of stress and strain used in modern theories of materials. BERNOULLI refers here only to simple push or pull, and his explanations indicate also that he regards the phenomenon as occurring *homogeneously* over the length and cross-section of the specimen. But Lemmas I and II together assert that *there is an elastic law*, viz (66), *which is common to all specimens of a given material*, be their lengths and areas what they may. It is the first time since GALILEO's formula for rupture that a *material property* appears in rational mechanics. JAMES BERNOULLI's insistence upon full generality, however, prevents him from exploiting (66) in the linear case, when it becomes $\tau = E\epsilon$, the modern "HOOKE's law" relating stress and strain, nor does he comment that it implies the existence, as for dimensional reasons in fact it does, of a *material constant* E having the dimension of τ, *i. e.*, of stress.]

Lemma III asserts in effect that if $\frac{1}{E} = \frac{\partial \epsilon}{\partial \tau}\Big|_{\tau=0}$, *i. e.*, if $E\epsilon \approx \tau$ for small values of ϵ, then

$$E\epsilon < \tau . \tag{67}$$

The reasons given are (A) $\epsilon \geqq -1$, since it is absurd for a fibre to be compressed more than its entire length[1]), (B) "It ought to be the same for the extensions, since an extension

1) This plainly correct observation, which JAMES BERNOULLI had phrased in other terms in 1695 (above, p. 98), is called "rather an idle argument" by PEARSON, § 22 of *op. cit. ante*, p. 11; it is approved by D'ALEMBERT (Encycl. 5 (1753), art. "Elasticité"). TODHUNTER (§ 20) describes Lemma III as "strictly true, but . . . not of great practical importance for our subject . . ." In fact Lemma III is not always true; *cf.* p. 115, below.

is nothing else than a negative compression" [thus BERNOULLI has retreated from his correct opinion of 1695 that compression and extension follow different proportions], (C) the relation (67) is borne out by BERNOULLI's experiments on the stretching of a gut string a yard long [the same as he reported to LEIBNIZ eighteen years earlier (above, p. 63). The quantity E is what is now called the "tangent modulus of elasticity"; it does not appear explicitly in BERNOULLI's wording of Lemma III, which amounts to an assertion that bodies which remain elastic respond more stiffly beyond the linear range.]

Lemma IV asserts, in effect, that the moment required to bend a beam a given amount is independent of the position of the neutral axis. [This, as has been remarked many times[1]), is false, and the two proofs BERNOULLI presents are fallacious.]

Problem I is GALILEO's problem. Here BERNOULLI finds VARIGNON's formula (58), except that he uses the extension rather than the altitude as independent variable. In applying the result, however, he employs (67) to show that $P_b < \frac{1}{3} P_t$, as is confirmed by MARIOTTE's experiments. The simple proof rests on [VARIGNON's] assumption that the

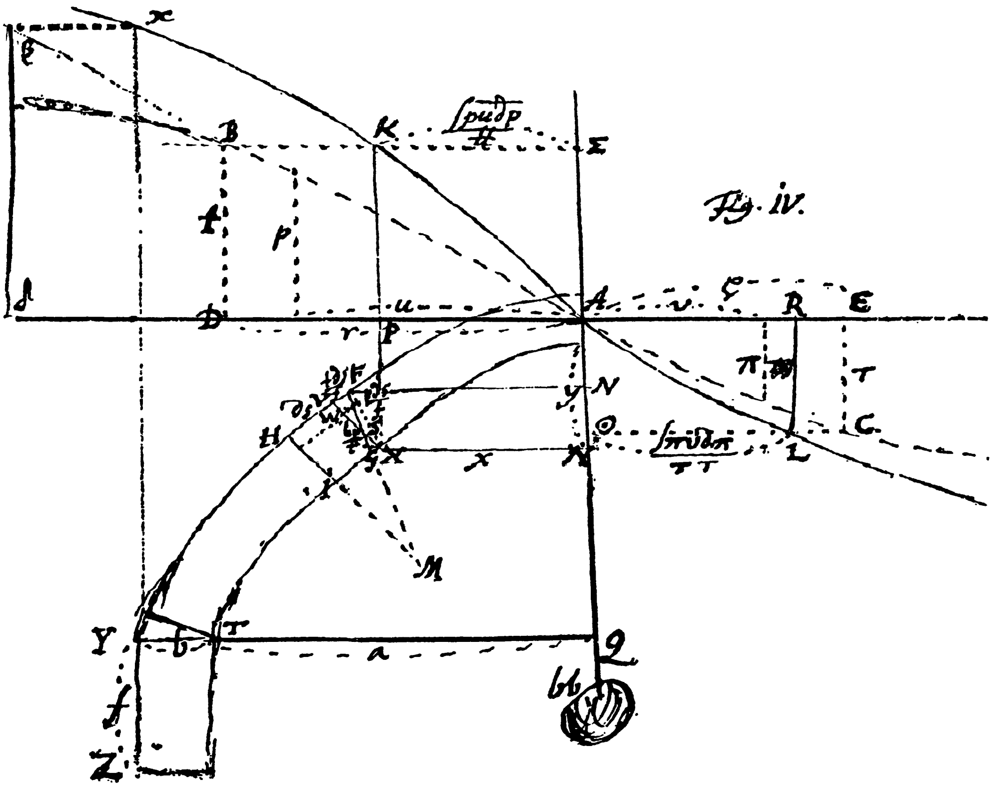

Figure 38. JAMES BERNOULLI's last analysis of the elastica (1704)

1) *E. g.* on pp. 983—984 of JAMES BERNOULLI's Opera 2 by CRAMER, the editor, who says "it would require a volume" to treat the following two problems correctly.

beam breaks in bending when the uppermost fibre suffers the stress just sufficient to break that same fibre in simple pull.

Problem II gives JAMES BERNOULLI's final treatment of the bending of an elastic beam (Figure 38).

[He is now somewhat further from the right approach than he was in the unpublished note explained above, pp. 99—101.] Again he calculates the moments acting on the cross-section, but then he invokes the [false] Lemma IV to relate the extension of the innermost fibre to the contraction of the outermost, and these two are then expressed in terms of the radius of curvature. The result is an equation of the form (56).

The papers published in JAMES BERNOULLI's lifetime do not exhaust his basic contribution to our subject. A note[1]) from about 1694 is called, *To find the curve which an attached weight bends into a straight line; that is, to construct the curve* $a^2 = sR$. Written before his first paper on the elastica, it gives the foundation for his claim there that he could exhibit the "characteristic properties" [*i. e.* the differential equation, *etc.*] of "what shape should be given to a band in order that through bending it take on a given curvature." The text of the fragment concerns only the integration of the differential equation stated in the title,

$$s = -\frac{a^2}{R}\,, \tag{68}$$

and does not mention the elastic band; in publishing the work in 1744, NICHOLAS I BERNOULLI writes, "I have not found this identity established." [Small wonder, since to set up the differential equation for this problem two prerequisites unpublished in 1744 were required: (A) *formulation of the inverse problem* in theories of finite deflection, and (B) the theory of *naturally curved bands*. NICHOLAS I BERNOULLI was an able and well informed mathematician, who annotated his uncle's posthumous fragments with insight and precision. Nothing could show more clearly JAMES BERNOULLI's gigantic dominance of rational elasticity than this incomprehension, nearly forty years after his death, of those parts of his principles he did not publish in detail. For the law of an elastica endowed with natural curvature R is

$$\mathcal{M} \propto \frac{1}{r} - \frac{1}{R}\,; \tag{69}$$

in the inverse problem, r is given and R is unknown. If the elastica is to be straight when loaded, we have $\frac{1}{r} = 0$, and the moment $\mathcal{M} = Wx = Ns$, where x refers to the straight, loaded form. Thus (68) follows. Not only does this show that JAMES BERNOULLI was in

1) No. CCXVI of *Thoughts, notes, and remarks,* cited above, p. 80, published in slightly expanded form as No. XX, pp. 1084—1086, of the "*Varia Posthuma,*" Opera 2, 1084—1086.

possession of (69) but also, as it were, from the very grave he claims his own, for in the same year as this fragment was finally published appeared also EULER's treatise on elastic curves, where (69) is asserted and (68) is derived (see below, pp. 214—215). Both publications have escaped notice in most modern expositions of the theory of the elastica.]

JAMES BERNOULLI obtains a "construction" for (68); [it is not enlightening, as it does not reveal that the curve is a spiral, nor is this indicated by his figure.

We pause to salute the great man who here leaves our history. In our epoch for study, 1638—1788, but one other, EULER, is to build himself a like monument in our subject. JAMES BERNOULLI reached deepest of all the students of continuum mechanics of his century. In the theory of perfectly flexible lines in the plane, he derived the general equations and thus, had his work been known, would have closed the subject. While in the theory of elasticity he attacked but one problem, it is of the deepest conceptual difficulty as well as central, indeed *the* elastic problem for a hundred years. Approximations were abhorrent to him; resolutely he put his entire strength upon problems of *finite deflection*. His solution (56) is correct; today it remains a landmark, the classical specimen for a theory of large deformation. That, as we have seen, his treatment is bound closely to earlier work of GALILEO and LEIBNIZ, does not lessen its originality but rather fastens its relevance. To the ironies and disappointments which filled JAMES BERNOULLI's life must be added that while he originated or assembled all the apparatus sufficient to put (56) on firm ground, he failed to do so, failed because his attempt was on too grand a scale.]

14. PARENT's researches on the neutral fibre (1704—1713). [The researches of PARENT are of greater value for the sciences of elasticity and strength of materials than any others done in France in the 150 years between MERSENNE's day and COULOMB's. Granted scarce notice in PARENT's lifetime, they were forgotten until TIMOSHENKO read and described some of them[1]). PARENT was an unusual scientist in that he performed many experiments yet was able to contribute to the theory as well.

1) § 11 of *op. cit. ante*, p. 11. Here I add a few supplementary remarks.

PARENT (1666—1716) is a scientist of wide attainments and considerable originality, deserving a special historical study. The following remarks are drawn partly from FONTENELLE's frank "Éloge" in Hist. acad. sci. Paris 1716, 88—93 (1718), and partly from his own works. PARENT seems to have been the most active and creative person associated with the Paris Academy in the years 1699—1716. "The great extent of his knowledge, joined to his natural impetuousness, led him often to contradict upon all subjects, sometimes precipitately and with little tact." But a small fraction of the works he presented to the Academy appear in its publications. His writing was obscure; this gave the academicians the excuse of not trying to understand him. That his papers were rejected, however, is more likely because he was justly critical not only of classic writers like DESCARTES and HUYGENS but of eminent but now deservedly forgotten colleagues as well, and perhaps also because his own researches are of a quality superior to most others published by the Paris Academy at the time.

At first PARENT's researches followed closely the work of LEIBNIZ and VARIGNON.] As early as 1704[1]), he noted that [VARIGNON's] formula (59) implies [GALILEO's] proportion

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His papers in the volumes of the Academy are mostly short and uncontroversial summaries of experimental data. Even the Histoire scarcely mentions his activity after the favorable comments in the volume of 1700 (mentioned below, p. 378), just after he was attached to the Academy and presumably before his personality became well known. For his deeper studies, he had to find another issue, so he began in 1705 "a kind of journal, called *Recherches de mathématique et physique*, which appeared anew, much enlarged, in 1713." I have not seen a copy of the first edition; the second is *Essais et recherches de mathematique et de physique*, nouv. éd., Paris, Jean de Nully, 3 vols, [xlvi] + 472 + [84] + 156 + [viii] pp., [ii] + [781] pp., [viii] + 528 + [80] pp., 1713, miserably printed, confusingly paginated, and full of misprints or errors of inadvertence; lists of corrigenda and revisions occur here and there. In these articles PARENT published systematic and critical reviews of the works of others; it was this practice, fraught with peril then as now, that had all but caused the Journal des Sçavans, the first scientific periodical, to founder after its first year (1665). The preface to the *Essais* is an interesting document in the history of scientific independence.

The obituary tells us that PARENT was left in oblivion because of his known obscurity as a writer, "the dislike he drew upon himself by his free criticism, the little order, or rather the disagreeable order of the material, and the awkward form of the volumes . . ." Although he published prolifically, he left many unpublished papers behind him; the obituary states that some of these are complete treatises and names the executor of the estate.

Obscure writing and tactlessness contribute but do not suffice in explanation. PARENT was one of the first writers to use the new mechanics for *practical analysis of machines;* see, *e. g.*, his remarkable paper, "*Nouvelle statique . . .*," Mém. acad. sci. Paris 1704, 4to ed., Paris, 173—197 (1705) = 2nd 4to ed., Paris, 173—197 (1722), and others in his *Essais*. Work of this very applied type, like EULER's on similar topics in the next half century, was not of interest to mathematicians or physicists and was much too difficult to be understood by the engineers who could have used such results but in fact did not begin to do so for about a century.

PARENT lived in retirement, devoted to science, truth, and piety. Though poor, he gave much charity; though straitly pressed for time, he gave freely of it to help others, particularly foreigners, because he was proud of his country. He gave lessons to certain mathematicians, who straightaway drew concrete profit from his teachings; the secret of their names died with him.

He entered the Academy as a "Student" and remained in this rank until it was abolished in the year of his death.

1) In publishing the result in a memoir dated 4 June 1707, PARENT writes that he had explained it more fully in a memoir dated 2 April 1704, but I cannot find this earlier work. See § I of PARENT's "*Des résistances des poûtres par rapport à leurs longueurs ou portées, et à leurs dimensions et situations, et des poûtres de plus grand résistance, indépendamment de tout systême physique,*" Mém. acad. sci. Paris 1708, 4to ed., Paris, 17—31 (1709) = 2nd 4to ed., Paris, 17—31 (1730); also Hist. *ibid.*, 116—123.

A theory of arches is given in the paper, "*Des charges qu'il faut donner aux voûtes, afin qu'elles tendent à s'affermir le plus qu'il est possible,*" Essais . . . 3, 152—175 (1713), dated 7 May 1704.

The resistance of a truncated cone according to GALILEO's theory is calculated in the paper, "*Du point de rupture des arbres causés par l'effort du vent contre leurs feüillages; et de la figure qu'un corps tiré par un point doit avoir, pour résister le plus qu'il est possible à être rompu*, Essais . . . 3, 220—227 (1713), dated 12 April 1704.

PARENT then gives experiments on breaking strength but reaches no definite conclusion: "*Ex-*

(13) for beams of arbitrary but similar cross-section, where B and D are the typical dimensions of the cross-section in the directions normal and parallel, respectively, to the plane of bending. [Of course he supposes tacitly that all fibres are subject to equal tension when the beam breaks.] But at the same time he mentions explicitly the "center of compression" [JAMES BERNOULLI's "center of tension", the point where the neutral fibre meets the cross-section] and states, [as had JAMES BERNOULLI in 1695, see above, p. 98,] that the moment of forces acting on the cross-section is to be taken with respect to this point. After reproaching the wood merchants for disregarding the rule (13), he determines the rectangular beam of greatest strength that can be cut from a cylindrical log of given area. By (13), we are to maximize BD^2 when $B^2 + D^2 =$ const., and hence $D = \sqrt{2}B$. To achieve this proportion, the woodcutter has but to erect oppositely directed perpendiculars upon the points trisecting the diameter; these perpendiculars cut the circumference at the two remaining corners.

PARENT's paper, *Comparison of the resistances of solid cylinders and cylindrical segments with those of hollow ones having equal bases, in the system of Mr. MARIOTTE* [1]), [seems to follow the concepts of VARIGNON closely [2]),] although PARENT denies any connection. [While LEIBNIZ most plainly knew and VARIGNON made it entirely clear that the moment (61) depends upon the cross-sectional shape,] PARENT is the first to give examples. In effect, he calculates I for the annular segment shown in Figure 39. The result, while elementary, is ela-
borate. PARENT gives many special cases and compares 2—15
the strengths of such segments with those of solid or full
beams of equal area. The simplest case is the most in- 16
teresting: For a full solid cylinder we have from (61)

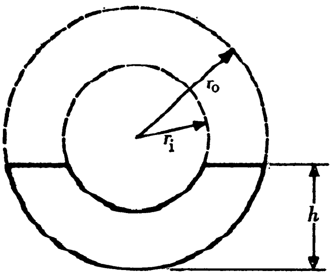

Figure 39. Cross-section for which PARENT calculated the flexural stiffness (1713)

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periences pour connoître la résistance des bois de chêne et de sapin," Mém. acad. sci. Paris 1707, 4to ed., 512—516 (1708) = 2nd 4to ed., 512—516 (1730).

Figures of equal resistance under very general circumstances are calculated in the paper, "*Des points de rupture des figures,*" Mém. acad. sci. Paris 1710, 4to ed., 177—194 (1712) = 2nd 4to ed., 177—194 (1732); also *Histoire* 126—133.

1) "*Comparaison des résistances des cylindres et segments pleins, avec celles des creux égaux en base, dans le systême de M. MARIOTTE,*" Essais ... 2, 567—595. At about this time, the problem is attacked also by HERMANN, Prop. VIII of Lib. II of *op. cit. ante*, p. 86; HERMANN's assertion that the "resistances or firmnesses" [*i. e.* breaking forces] of tubes are directly as the "tenacities" of the material, the thickness, and the length does not seem to be justifiable on the basis of any reasonable assumptions about the nature of the interior forces.

2) PARENT, though a poor writer, is somewhat more explicit; he says that he neglects bending as being "almost imperceptible in experiments on hard bodies."

$P_b l = \frac{5}{64} \pi K d^3$, where K is the breaking tension, *i. e.* the tension [per unit breadth] in the topmost fibre. Division by (12) yields

$$\frac{P_b}{P_t} = \frac{5}{16} \frac{d}{l} . \tag{70}$$

17—18 While Mariotte had compared his experimental data on circular cylinders with (19), that formula is valid only for a rectangular cross-section, so it is no wonder he failed to find agreement; according to Parent, (70) fits Mariotte's measurements very well.

[Although on Galileo's hypothesis there is a simple universal formula to compare the strength of a hollow tube with that of a solid rod, on Leibniz's hypothesis there is not.] Parent gives a table of the values of P_b/P_{b0} and r/r_0 as functions of r_i/r_0, where P_{b0} is the strength of the circular rod of equal cross-sectional area, and r is the radius of the circular rod of equal strength[1]).

Just before this table Parent puts some criticisms of Varignon. The first I do not fully understand; Parent seems to say that a break in a loaded beam always starts on the top side. In the second, he writes that "there is no body insusceptible of extension . . . and compression . . . Therefore at the instant before the body breaks, its base [*i. e.* cross-section] suffers dilatation above and at least a little compression below, although this latter is of scant importance in practice . . . Thus there is a middle [part] where it suffers nothing at all, and the axis of breaking is there. It is true that this axis descends during the breaking until it reaches the edge of the base, where it is located when the breaking is over. But at the instant just before the breaking of the first fibre, the axis is never at the surface. But everything is governed by the breaking of the first fibre, since once it is broken, all the others will give way without fail. Thus all problems of the resistance of solid bodies broken on fixed points [*i. e.* supports] are reduced to finding the force necessary for breaking the first fibre, with the axis being that which we have just determined.

[These ideas are not consistent with the calculations he has just made. They seem to indicate that after having finished this memoir, he achieved a clearer view of the strength of beams, as we see now.

The source of Parent's enlightenment is indicated by] the title of his most important paper: *On the true mechanics of the relative resistances of solids, with reflections on the system*
3—5, 7 *of Mr. Bernoulli of Bâle*[2]). Here he criticizes Bernoulli's Lemma IV and shows its

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1) A later paper, which is apparently the first to recognize the factor I in (61) as being a moment of inertia, concerns similar problems for a trapezoidal cross-section: "*Sur les résistances des prismes dont les bases sont des polygônes réguliers autour d'un axe, et que l'on rompt sur des points fixes proches ou éloignés, suivant le premier système de M. Mariotte*, Essais . . . **3**, 314—335 (1713).

2) "*De la véritable méchanique des résistances relatives des solides, et réfléxions sur le système de M. Bernoulli de Bâle*," Essais . . . **3**, 187—201 (1713). On the basis, apparently, of Parent's paper of

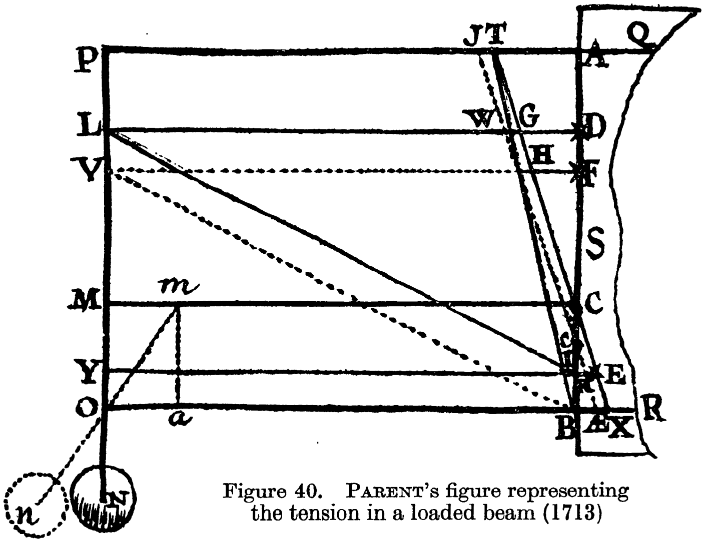

Figure 40. PARENT's figure representing the tension in a loaded beam (1713)

falsity by calculating the moment of tensions represented by the line TCX, where MC is the neutral fibre (Figure 40), and comparing it with the moment of tensions represented by the line TB. PARENT ad- 4
duces a somewhat involved argument to show that "LD will indicate the pressure that the fibres at I suffer perpendicularly to AB, and at the same time the resistance these fibres offer to being compressed parallel to their lengths; and LY, that which they suffer from top to bottom, and at the same time that which they make in virtue of their tenacity against being separated from each other parallel to DL." [In this isolated sentence is the first and only appearance of interior *shear stress* prior to the work of COULOMB at the end of the century.] Moreover, "the resistance of the fibres of the triangle BCX to being compressed along YI is equal to that of the triangle ACT to being stretched along DL, a property of which no one has yet said anything." [*I. e.*, it is not enough to balance moments; one must balance also the *normal forces* acting upon the cross-section (*cf.* above, p. 93).] PARENT's figure is misleading in that TCX is a single line, while all his reasoning refers to allowing different moduli for compression and extension (Figure 41). Hence he concludes that "the resistance at AT is to the resistance at BX . . . reciprocally as their distances BC, AC from the fixed point C . . ., which no one had noticed before." [That is, $K_t/K_c = D_c/D_t$, for this is a statement that the areas, or resultant forces, of the two triangles ACT and BCX are equal.

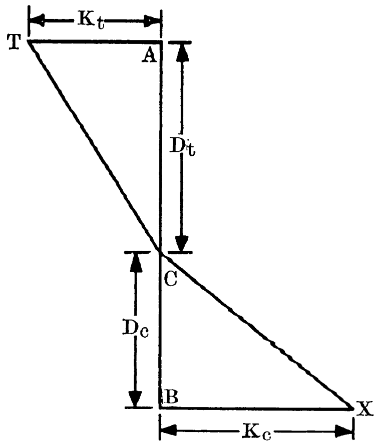

Figure 41. Modern diagram to illustrate PARENT's view of the tensions acting on the cross-section of a terminally loaded beam

While PARENT has been anticipated in part by JAMES BERNOULLI's paper of 1695

1708, BÜLFFINGER (§ 1 of *op. cit. ante*, p. 103) describes PARENT as "a man whose reputation is far below his desert" but does not mention having seen the more important paper described above.

(to which he does not refer) and by BERNOULLI's unpublished notes concerning a deeper problem, PARENT replaces BERNOULLI's incorrect assumption $\mathcal{M}_c = \mathcal{M}_e$ by *a correct means of locating the neutral line*, viz, *the areas under the curves of extension and compression must be equal*, as follows from the balance of normal forces.]

PARENT then gives a simple argument in favor of (67).

Also[1]) $P_b l = \frac{1}{3}(K_t D_t^2 + K_c D_c^2)$ and $P_t = K_t(D_t + D_c)$; hence

(71) $$\frac{P_b}{\frac{1}{3}P_t} = \frac{D_t}{l};$$

from MARIOTTE's experiments it follows [according to this theory] that $D_t/D_c = 9/2$. [Thus PARENT proposes *to infer the position of the neutral fibre by comparison of theoretical formulae with the results of experiment*, at least in part[2]).]

From all this PARENT concludes that "the elastic curve remains to be found." [This is not quite just, since the *curve* is unaffected by these considerations, which concern only its interpretation in terms of the cross-sections.

While the memoir just analysed is far from clear, we see that PARENT was the first *to apply statical principles correctly and completely to the tensions of the fibres of a beam*, and that he recognized the existence of *shearing stress*. However, like all other writers so far except JAMES BERNOULLI, he neglected the bending of the beam.]

15. Researches on theoretical and experimental elasticity by JAMES RICCATI (1720—1723) and others. In a letter[3]) of 29 June 1721 JAMES RICCATI writes

1) These results are given wrongly in the text but corrected in the unpaginated notes at the end of the volume.

2) Since later work on the neutral line prior to COULOMB's failed to reach PARENT's level, we summarize it here.

BÜLFFINGER in §§ 22—35 of *op. cit. ante*, p. 103, illustrates the effect of the law of tension on the position of the neutral line, but his considerations fall short of PARENT's rule of areas. BÜLFFINGER proposes to locate the neutral line by comparing with experiment formulae derived from a general [and hence not equilibrated] linear distribution of tension.

In § 29 δ of *op. cit. ante*, p. 11, PEARSON describes a work of JACOPO BELGRADO, *De corporibus elasticis disquisitio physico-mathematica*, Parma, 1748. PEARSON's claim that although "he gives a geometrical method for determining points on the ... neutral line," which he does not make the mistake of placing on the surface of the beam, "there is little to be learnt" from BELGRADO's work, arouses my suspicion that this may be an important study, but I have been unable to locate a copy.

3) An extract is given in the letter of NICHOLAS II BERNOULLI to GOLDBACH, 16 July 1721, included in the correspondence published by FUSS, *op. cit. infra*, p. 165. The matter is discussed further in the letters of 30 July, 11 September, 15 September, 23 October, 6 December, and 2 January 1722. GOLDBACH points out, in effect, that the increment of force is applied to the deformed, not the initial configuration; also, the laws of compression and extension may be different. He refers mainly to bending rather than to simple pull. These are true observations, but, as BERNOULLI remarks, they do not seem connected with RICCATI's experiment.

to NICHOLAS II BERNOULLI in regard to JAMES BERNOULLI's memoir of 1705, particularly to the inequality (67). "I have repeated [JAMES BERNOULLI's] experiment various times in strings of different material; often I have found true what that famous author says, but often experiment showed me just the opposite . . . But when further equal weights were added until the string broke, the extensions which went on increasing up to a certain point then began to decrease again in inverse order." [This is the experimental discovery that some materials *stiffen*, others *soften* prior to rupture[1]). The two types of behavior, as reported by RICCATI, we represent schematically in Figure 42.]

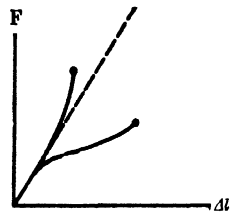

Figure 42. Different types of yield prior to rupture, as reported by JAMES RICCATI (1721)

At about this time JAMES RICCATI wrote the paper entitled *True and appropriate laws of elastic forces proved from the phenomena*[2]). His basic idea, [proposed earlier by LEIBNIZ, above, p. 63,] is that *elastic properties of a body may be inferred from the frequency of its vibration.* Rejecting any kind of empirical law of force *vs.* elongation as "unworthy of geometry", even if borne out by experiment[3]), RICCATI attempts to derive a theory of elasticity with no further basis than the laws of mechanics and the known rules governing the [fundamental] frequencies emitted by vibrating strings. Thus it would seem, as indeed the editor of RICCATI's works later asserted[4]), that RICCATI presumed an analogy between the transverse and the longitudinal oscillation

1) It is typical of PEARSON, § 30 of *op. cit. ante*, p. 11, that while he quotes at length what he misinterprets as a general and unsupported proposal of an empirical philosophy of science by RICCATI (*cf.* footnote 3, below), he does not mention this simple, definite, and illuminating experiment.

2) "*Verae, et germanae virium elasticarum leges ex phaenomenis demonstratae,*" De Bonononiensi sci. art. ist. acad. comm. [1], 523—544 (1748) = Opera 3, 239—257. An editorial note in the reprint (1764) tells us that RICCATI began this work as far back as 1720, that an abstract of it was communicated to NICHOLAS II and JOHN I BERNOULLI in 1721, and that the finished manuscript was delivered to the academy of Bologna before 12 October 1723. This note states also that the first volume of the Bologna memoirs appeared in 1731, but I find no record of such a publication.

3) Here PEARSON (*op. cit. ante*, p. 11, § 30) shows his usual ability to miss the point of theoretical papers: He extols this essay because RICCATI lays down "the true theory of all physico-mathematical investigations," namely, that things to be regarded as known must be sought "from nature itself, and from experiments," rather than from "the imagined hypotheses of the philosophers" (p. 523). What RICCATI is rejecting is the tendency of the physicist ("philosopher") to conjecture or determine empirically what may be proved by *mathematics* ("geometry"). RICCATI's aim is the opposite of that PEARSON attributes to him, and PEARSON might better have taken this work, in fact a failure, as an example of misguided theory, which it is.

4) This editor seems to have been the author's son, JORDAN RICCATI, since the long *Note of the editor*, pp. 258—276, has little to do with the work ostensibly being annotated but rather presents what seems to be a preliminary version of the paper on elasticity published by JORDAN RICCATI in 1767 (see below, p. 384).

of a string, [but in any case the reasoning is tenuous]. The result is a differential equation [which is not dimensionally correct; if corrected and integrated, it yields a relation between stretching force F and length l of the form

$$(72) \qquad F = F_0 e^{\frac{l-L}{l}} \quad \text{or possibly} \quad F_0 e^{\frac{|l-L|}{l}},$$

where F_0 is the force required to maintain the string at the length L.]

The paper ends with a statement that the "elasticities" [*i. e.* force constants] of strings of given length obey the proportion

$$(73) \qquad K \propto \sigma \nu^2 ,$$

where ν is the frequency of oscillation. [If the oscillation is longitudinal, this result is correct, but for transverse oscillation it is false. What RICCATI means and how he reasons I cannot understand.] His application is to the vibrations of elastic spheres; citing experiments of CARRÉ, he infers that $K \propto \varrho r^2 \nu^2$; [while the result is correct, if K is an elastic modulus, the reason is again obscure[1]).]

Writers of the eighteenth century occasionally refer to the work of 's GRAVESANDE[2]); examination of his chapter *On the laws of elasticity* reveals it to be the report of a mass of ill conceived experiments garnished with bold assertions. He claims to establish the proportionality of deflection to load and length, but his experiment, employing specially designed and presumably precise apparatus, is imperfectly described and in any case

1) RICCATI wrote further papers on related subjects: "*Della proporzione, che passa fra le affezioni sensibili, e la forza degli obbietti esterni, da cui vengono prodotte,*" Suppl. giorn. letterati d'Italia **1**, 114—141 (1722) = Opere **3**, 287—297. This is perhaps the first attempt to apply mathematics to physiology. RICCATI assumes that the human body is made up of "fibres" and that all sorts of stimuli are analogous to forces deflecting these fibres. On this basis he discusses the sensations, the effects of age, *etc.* While the paper is fantastically imaginative, it is a most courageous attempt, deserving notice in the history of theoretical biology.

"*Sopra alcune proprietà delle corde elastiche,*" Opere **3**, 276—284 (1764). This paper, which an editorial note asserts to date from 1734, discusses rather inconclusively the vibratory motion of a weight hung by a spring in which the law of restoring force is not specified.

"*Confutazione dell'ipotesi, che due corpi dotati di eguali quantità di moto urtando in due corde del tutto eguali le ripieghino per eguali saette,*" Opere **3**, 284—287 (1764). Here RICCATI studies the difficult problem of determining the form, which he assumes to be triangular, that an elastic string will assume when struck at its center by a ball.

After these three papers is a *Note by the editor*, pp. 299—323, which seems to be a preliminary version of the paper by JORDAN RICCATI described below, pp. 280—281, 384—385.

2) *Physices elementa mathematica, experimentis confirmata. Sive introductio ad philosophiam Newtonianam*, Lugduni Batavorum, Vander Aa, Vol. 1, [xxvi] + 345 + [iv] pp., 2nd ed., 1725. I have not seen the first edition, dated 1720. Page references refer to the second edition, with numbers in parentheses being the references 's GRAVESANDE himself gives to corresponding passages in the first edition. The counterpart of Ch. XXIX of the second edition was Ch. XXVI of the first.

would prove nothing at all[1]). Saying that "an elastic band may be regarded as an agglom- 692—696 (262)
eration of strings," so that results on strings may be applied to it, 's GRAVESANDE finds
that "the bendings of the same band are proportional to the forces which bend it," both
for a straight band loaded at one end and for a curved band pulled out straight[2]). He 697—698 (266)
gives also an imaginative description of the deformation of an elastic sphere dropped upon
a rigid plane[3]).

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1) See §§ 673—680 (249—251). Since this experiment is sometimes cited by historians as a decisive proof of HOOKE's law, I append an analysis of it. An elastic wire is passed over two wedges and held taut by a *specified weight* T; other weights are hung from its center, and the corresponding deflections δ are found to be proportional to those weights. Since the forces exerted on the string by the wedges are not known, the problem is indeterminate.

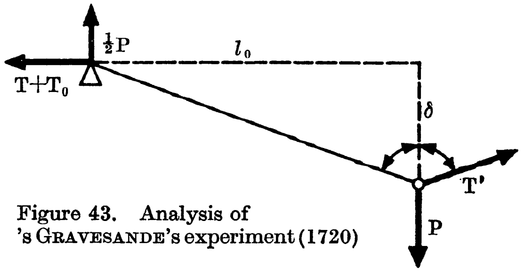


Figure 43. Analysis of 's GRAVESANDE's experiment (1720)

The most general possible system of forces acting on one half of the stretched string is shown in our sketch (Figure 43), where T_0 is the unknown horizontal force exerted by the wedge. For equilibrium, we must have

$$\text{(E)} \qquad \frac{\delta}{l_0} = \frac{\frac{1}{2}P}{T + T_0}\,, \quad P = 2T'\frac{\delta}{\sqrt{\delta^2 + l_0^2}}\,.$$

Thus if T is *held constant,* and if T_0 is constant or is much smaller than T, we must have $\delta \propto P$, *independently of any elastic law.* For this, no experiment is required.

We usually encounter another form of this problem, in which the end of the string is *fixed.* Then we have no concern with T or T_0, but, by HOOKE's law, $T' = K\sqrt{\delta^2 + l_0^2} - L$, where L, the initial length, may or may not equal l_0. Then $(E)_2$ gives

$$P = 2K\delta\left(1 - \frac{L/l_0}{\sqrt{1 + \delta^2/l_0^2}}\right) \approx 2K\delta\left(1 - \frac{L}{l_0} + \frac{1}{2}\frac{\delta^2 L}{l_0^3}\right).$$

Therefore $\delta \propto P$ holds for small deflections if and only if $L \neq l_0$. If $L = l_0$, we get $P \propto \delta^3$ instead. This is a classic example to show that the response of a linearly elastic body may fail, for kinematical reasons, to be linear; it is to be derived by EULER and by DANIEL & JOHN III BERNOULLI (below, pp. 385—386).

Thus 's GRAVESANDE missed his chance twice over: Had he set up the experiment properly, he would have failed to find the linear response he was looking for.

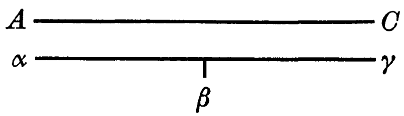
2) In § 701 he writes that results concerning the period of oscillation of a string or the shape of the elastic curve require "use of the direct and inverse methods of fluxions and hence do not seem to me to pertain to the elements of physics." This is perhaps the earliest example of what has become an honored tradition among writers on physical mechanics.

3) Here we mention some minor works of this period.

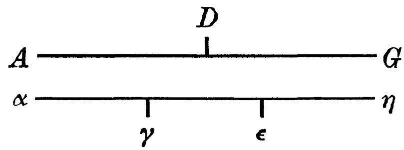
CAMUS, "*Du mouvement accéléré par des ressorts et des forces qui résident dans les corps en mouvement,*" Mém. acad. sci. Paris 1728, 159—196 (1730). This paper gives elaborate statements and proofs of simple theorems on the motion of a body subject to the force of a not necessarily linear spring.

J. JURIN, "*A letter . . . concerning the action of springs,*" Phil. trans. London **43** (1744/5), No. 472, 46—71 (1746); Phil. trans. abridged **9**, 18—20. This paper concerns the motion of a linear spring struck by a body.

16. Experiments on the nodes of vibrating bodies by Noble and Pigot (1674), Sauveur (1696—1701), de la Hire (1709), and Zendrini (1715—1716). In 1677 Wallis wrote as follows to the Editor of the *Philosophical Transactions*[1]). "Sir, I have thought fit to give you notice of a discovery that hath been made here, (about three years since, or more) . . . 'Tis this : whereas it hath been long since observed, that, if a Viol string, or Lute string, be touched with the Bow or Hand, another string on the same or another Instrument not far from it, (if an *Unison* to it, or an *Octave*, or the like) will at the same time tremble of its own accord. The cause of it, (having been formerly discussed by divers) I do not now inquire into. But add this to the former Observation; that, not the whole of that other string doth thus tremble, but the several parts severally, according as they are Unisons to the whole, or the parts of that string which is so struck. For instance, supposing AC to be an upper octave to $\alpha\gamma$, and therefore an Unison to each half of it, stopped at β :



Now if, while $\alpha\gamma$ is open, AC be struck; the two halves of this other, that is, $\alpha\beta$ and $\beta\gamma$, will both tremble; but not the middle point at β. Which will easily be observed, if a little bit of paper be lightly wrapped about the string $\alpha\gamma$, and removed successively from one end of the string to the other." The like holds for the points trisecting or quadrisecting $\alpha\gamma$ when AC is tuned a twelfth or a double octave, respectively, above $\alpha\gamma$. "So if AG be a Fifth to $\alpha\eta$; and consequently each half of *that* stopped in D, an Unison to each third



1) "*Dr. Wallis's Letter to the Publisher concerning a new Musical Discovery; written from Oxford, March 14, 1676,*" Phil. trans. London **13**, No. 134, 839—842 (1677) = Phil. trans. abridged **2**, 380—382.

Almost twenty years later Wallis published this material in Latin in Ch. 107, "*Experiments on musical strings,*" of *De algebra tractatus*, Opera **2**, 1—482 (1693). The English edition of the *Algebra* (1685) does not contain it. The Latin version, written with markedly British constructions, when put back into English emerges as a superior literary performance; in respect to content, it is partly clearer and partly less clear than the letter of 1677. The later version is very careful in respect to priorities. Not only was harmonic resonance not claimed as new, but "I recall that sixty years ago it was shown to me, then a boy" (*i. e.*, at about the time of Mersenne's publication). In 1693 Wallis writes that the nodal phenomena were shown him by Narcissus Marsh in 1676 "as a new thing observed three years before (for the first time, I think) by William Noble . . . and Thomas Pigot . . .; whether by both together or by one [first], I do not know." It is not clear whether Wallis is here correcting or forgetting what he had written in 1677. In the Latin version Wallis is also more careful in describing the behavior of the paper rider: "Then in the middle it will remain unstruck, but elsewhere it will be shaken off."

part of *this* stopped in D; while *that* is struck, each part of *this* will tremble severally, but not the points γ, ϵ; and while *this* is struck, each of *that* will tremble, but not the point D. The like will hold in lesser concords; but the less remarkably as the number of divisions increases.

"This was first of all (that I know of) discovered by Mr. WILLIAM NOBLE, a Master of Arts of *Merton-College;* and by him shewed to some of our Musicians about three years since; and after him by Mr. THOMAS PIGOT, a Batchelour of Arts, and Fellow of *Wadham-College,* who, giving notice of it to some others, found, that (unknown to him) the same had been formerly taken notice of by Mr. NOBLE, and (upon notice from him) by others: and it is now commonly known to our Musicians here."

[Thus it was known to numerous Oxonians by 1677 that *a string may assume a mode of vibration with* $k-1$ *nodes dividing it into* k *equal portions, and in such a mode the tone emitted is the* k^{th} *overtone*[1]).] WALLIS notices also that a string if struck at any nodal point "will give no clear sound at all; but very confused," though "the less remarkable as the number of divisions increaseth. This and the former I judge to depend upon one and the same cause; *viz* the contemporary vibrations of the several Unison parts, which make the one tremble at the motion of the other: But when struck at the respective points of divisions, the sound is incongruous, by reason that the point is disturbed which should be at rest."

A Postscript adds, "A Lute-string or Viol-string will thus answer, not only to a consonant string on the same or a neighboring Lute or Viol; but to a consonant Note in Wind-Instruments: which was particularly tried on a Viol, answering to the consonant Notes on a Chamber-Organ, very remarkably: But not so remarkably, to the Wirestrings of an Harpsichord . . . And we feel the Wainscot-seats, on which we sit or lean, to tremble constantly at certain Notes on the Organ or other Wind-Instruments; as well as at the same Notes on a Base-Viol. I have heard also (but cannot aver it) of a thin, fine Venice-glass, cracked with the strong and lasting sound of a Trompet or Cornet (near it) sounding an Unison or a Consonant note to that of the Tone or Ting of the Glass[2])."

1) In our terminology the fundamental is called the first overtone, or, for a string, the first harmonic.

2) This phenomenon seems to have caught the fancy of several writers of this period. The work most often cited is MORHOF's *Stentor ὑαλοκλάστης sive de scypho vitreo per certum humanae vocis sonum fracto . . . dissertatio,* Kilonis, J. Reumann, 1672. I have seen this work only in the "Editio altera priori longè auctior," *ibid.*, 1683. On pp. 16—17 MORHOF writes, "When I was living at Amsterdam, I grew to know JODOEUS PLUMER, a famous bookseller of that place. He told me one day . . . of a certain wine seller, NICHOLAS PETTER . . . who could break glass beakers with his own voice. . . . I did not leave off urging the bookseller to take me to that man. He did so. When it was requested that the experiment be done in my presence, he brought out certain pot-bellied beakers with knobby feet, the kind we call "Romans", but not exceeding a pint measure. I selected one . . . which seemed very

In 1692 FRANCIS ROBERTS[1]) correctly described the nodal forms of a vibrating string, observed by means of a paper rider, and drew a correct analogy between the trumpet and the marine-trumpet, each having natural frequencies in the ratios 1, 2, 3, 4, . . . and thus being incapable of other "musical" vibration. If the marine trumpet is stopped at any point not making one of the two resulting portions of the string an aliquot part of the other, "the vibrations of the parts will cross one another, and make a sound sutable to their motion, altogether confus'd."

[The existence and the positions of nodes and their relation to frequency, for a vibrating string, were thus well known in England by 1693, though apparently not yet clearly understood on the Continent[2]).]

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strong. Then he, after determining its sound, gave it to me to hold, and bringing up his mouth to the middle part, he sang out a tone which seemed to me an octave above that of the glass. The glass at once resounded almost to screaming, and my hand felt its trembling. When he took a long breath and continued his voice without interruption, the glass broke with a crack so that an orbicular break went crosswise through the belly of the glass and the knobs of the feet from the side opposite his mouth."

MORHOF writes that BARTOLI also described this experiment, but he himself tried it in vain. On pp. 17—18 he writes, "I easily saw that the explanation . . . lay in the equality of the sound; . . . if changed by so much as a comma, or half a one, the effect would be destroyed. [The wine dealer] had learned to control his voice by daily practice, so as never to fail. He had a son, too, who could replace him and do it even more quickly, having a higher voice."

On pp. 19—20 MORHOF writes that he had told all this to BOYLE and OLDENBURG and to a meeting of the Royal Society. "It was decided that the thing should be tried . . ., but, as I learned, it ended in failure." In the records of the Royal Society as published by GUNTHER, *op. cit. ante*, p. 54, we learn that on November 17, 1670, HOOKE reported that he had tried the experiment "but had found no other success, than that the glass had sounded upon the sound of a man's voice." MORHOF's trials with musical instruments also led to failure.

The rest of MORHOF's not slight book is a compendium on sound, containing nothing original but nevertheless being of some interest as displaying the quantity of more or less correct but vague ideas and scarcely correlated facts current just prior to the creation of the first mathematical theories. The same may be said of the diffuse treatise of FABRI, Lib. II of Tract. III of *Physica, id est, scientia rerum corporearum* 2, Anisson, Lugduni, 1670.

1) "*A discourse concerning the musical notes of the trumpet, and trumpet-marine, and of the defects of the same*," Phil. trans. London **17**, No. 195, 559—563 (1692) = Phil. trans. abridged **3**, 467—470.

2) In 1681 MARIOTTE had observed that different parts of the trumpet tremble when different notes are blown, but he gave no evidence of having observed nodes; see Hist. acad. sci. Paris 1666—1699, **1**, 4[to] ed., Paris, 322 (1733).

An attempt to explain the action of the marine trumpet is given by DE LA HIRE, "*Explication des différences des sons de la corde tenduë sur la trompette marine*" (1692), Mém. acad. sci. Paris 1666—1699, **9**, 500—529 (1730), see esp. p. 502. DE LA HIRE is so vague a writer that it is difficult to know what he has seen and what he has not; he has some idea of the nodes. He claims to explain how a slight sound can cause a louder one by resonance; I can make no sense of his explanation, but he adduces an interesting experiment in which a faintly audible string is made to strike a consonant bell, which as a result emits a much louder sound.

In 1704 was published the celebrated paper by SAUVEUR, *General system of the intervals of sounds and its application to all musical systems and all musical instruments*[1]), where in the Preface the name *acoustics* is proposed for "a superior science of music . . ., having as its object sound in general, while music has as its object sound to the extent that it is pleasant to hear." [While SAUVEUR's researches appear to be original, most of what he reports may be found scattered here and there in the publications of MERSENNE (§ 4, above) or in the notes of WALLIS and ROBERTS, just described. The importance of SAUVEUR's paper is nevertheless great, for he wrote clearly and systematically, his short treatise served as a definitive and organizing summary of what was known in 1700 concerning the vibrations of strings, and he introduced much of the terminology gradually accepted in the acoustical researches of the eighteenth century.]

SAUVEUR calls for "a measure common to all intervals of sounds, capable of measuring the least perceptible differences between them, and such that one could select from them those necessary for ordinary music . . ." [With a thought appropriate for his times, he thus seeks to master a *continuous range of frequencies*, not merely the discrete scales used in music.]

"I was made to observe that especially at night one may hear from long strings not only the principal sound but also other small sounds, a twelfth and a seventeenth above; that trumpets have still more such sounds, such that the number of vibrations is a multiple of the number for the *fundamental* sounds . . . I concluded that the string in addition to the undulations it makes in its entire length so as to form the fundamental sound may divide itself in two, in three, in four, *etc.* undulations which form the octave, the twelfth, the fifteenth of this sound. I concluded hence the necessity for the *nodes* and *loops* of these undulations . . ." [The three terms we have italicized above are introduced in this passage.]

Most of the paper concerns construction of musical intervals by different systems, but Section IX is entitled, *On harmonic sounds*. "I call a *harmonic sound* of a fundamental sound that which makes several vibrations while the fundamental sound makes but one. Thus a sound at the twelfth of the fundamental sound is harmonic, since it makes three vibrations while the fundamental sound makes but one . . . Divide a monochord in equal parts, say 5 . . . Pluck this string as you please, it will give out the sound I call the fundamental of this string. Then at one of these divisions D, put a light obstacle C (Figure 44), such as the tip of a feather if the string is a fine one, so that the motion of this string is communicated to either side of the obstacle. It will then give out its fifth

1) "*Systême general des intervalles des sons, & son application à tous les systêmes & à tous les instrumens de musique,*" Mém. acad. sci. Paris 1701, 4to ed., Paris, 297—364 (1704) = 12mo ed., Amsterdam 390—482 (1707) = 2nd ed. Paris, 4to, 299—366 (1719). See also Histoire, 1st Paris ed. pp. 123—139 = 2nd Paris ed., 121—137 and Histoire 1700, Paris, 4to, 131—140 (1703) = 2nd ed., 134—143 (1761).

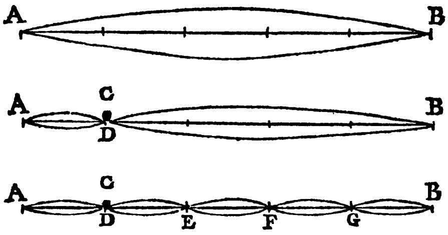

Figure 44. SAUVEUR's observation of the nodes of a vibrating string (1701)

harmonic sound, that is, a 17^{th}. [SAUVEUR's imaginative explanation, however, is unrelated to any mechanical or kinematical principle; while he uses freely the word "undulation", he seems to have no idea of wave propagation.] "I shall call the points A, D, E, F, G, B the *nodes* of these undulations, and the middles of these undulations will be called the *loops* of these undulations . . .

"One will be convinced of these undulations, 1°, by hearing ; for those who have a fine ear will distinguish a harmonic sound proportional to the parts forming these undulations, or indeed one may make sure by tuning a monochord in unison to this harmonic sound, 2°, by the eye ; for if one divides the string in equal parts, *e. g.* in 5, and if one sets a movable bridge C at D or E and bits of black paper on the divisions E, F, and bits of white paper on the middles of these parts, upon striking the part AC one will see that the bits of white paper, which are on the loops of the undulations, will jump, and the black ones on the nodes will stay fast." [Thus the technique of the paper rider, introduced by LEONARDO DA VINCI and by NOBLE and PIGOT, is refined.]

There follow some consequences.

I. The same harmonic results if any one of the nodes is fixed.

II. If, having formed the 5^{th} harmonic, one places an obstacle on a node for the 3^{rd} harmonic, the 15^{th} harmonic will result. [From SAUVEUR's reasoning it is easy to infer the general rule: If we sound simultaneously the m^{th} and n^{th} harmonics, we discern the tone of the p^{th} harmonic, where $p = l.c.m.$ of m and n.]

III. There are other ways to produce a harmonic in a string: 1°, by touching it with another string vibrating in unison with the desired harmonic; 2°, if a string is touched by another, each will give out the harmonic that is the least common multiple of their fundamentals.

IV. The higher harmonics are less sensible than the lower. [*I. e.*, other things being equal, the amplitudes of the harmonics decrease as the order increases.]

V. Bells and other resonant bodies have harmonics [*recte*, overtones which are not harmonious ; see below, p. 124].

In the parallel account given earlier in the volume[1]), the term *beats* is used, but only in connection with rare "encounters of vibrations" [such as are mentioned in GALILEO's explanation of harmony (p. 36 above) ; MERSENNE's concept of beats was clearer (above p. 33).

These phenomena remained long unnoticed by the geometers. As often happens in

1) P. 125.

such matters, a practiser rather than a theorist was eager to employ them before they were understood.] In his *New system of theoretical music*[1]), the great composer RAMEAU sets down as "facts of experience that serve as the principle for this system" the existence of perceptible overtones, citing MERSENNE and SAUVEUR. He says also that overtones occur in the sounds of cymbals and bells and in the lowest tones of a trumpet and of a bass voice. The overtones are heard slightly after the fundamental, and he advises us to imagine them first as an aid to hearing them. While he says the phenomenon of overtones "will serve us as a principle for establishing all our consequences," [I am unable to find any logical connection between it and the various assertions which follow]. However, as we shall see, the geometers were soon to interpret RAMEAU as founding his entire system of harmony on the idea that *tones which can be emitted by the same vibrating body are harmonious*[2]).

[In fact, this is true of the lower overtones of musical instruments, but for most other

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1) Ch. 1 of *Nouveau système de musique theorique, où l'on découvre le principe de toutes les regles necessaires à la pratique, pour servir d'introduction au Traité de l'harmonie*, Paris, viii + 120 pp., 1726. Though RAMEAU's earlier *Traité de l'harmonie reduite à ses principes naturels*, Paris, 1722, is usually cited in this connection, I find in it no reference whatever to the "principle" stated above. Whether from continuing mediaeval tradition, from concession to the ruling mechanistic views of the day, or from honest self-delusion, RAMEAU writes in the preface, "Music is a science which should have secure rules; these rules should be drawn from an evident principle, and this principle can scarcely be known to us without the aid of mathematics. Thus I must admit that despite all the experience I could get in music from practising it for so long a time, nevertheless it is only by the help of mathematics that my ideas have grown clear . . ." Not only is the "mathematics" confined to observations on the nature of subtraction, multiplication, and the arithmetic and geometric progressions, but there seem to be few traces of logical reasoning of any kind. As far as I can ascertain, RAMEAU's system in the *Traité* is based upon an *a priori* preference for certain numerical ratios as against others, while in the *Nouveau Système* he claims to have found a physical basis for that preference.

2) At the end of § IX of *op. cit. infra*, p. 242, DIDEROT in 1748 gives a very guarded statement of RAMEAU's principle: ". . . a sound never strikes our ears by itself; with it are heard other concomitant sounds, which are called its harmonics. It is thence that Mr. RAMEAU started in his harmonic generation; that is the experience which serves as basis for his admirable system of composition, which it may be hoped someone will draw out from the obscurities surrounding it . . ."

Cf. also the remarks of BURNEY, *A general history of music from the earliest ages to the present period*, 1 (1776), 2nd ed. (1789) (see p. 164 of MERCER's ed. (1935)): ". . . the moderns have lately discovered that nature, in every sounding body, has arranged and settled all these proportions in such a manner, that a sound appears to be composed of the most perfect harmonies, as a single ray of light is of the most beautiful colors; and when two concordant sounds are produced in just proportion, nature gives a third, which is their true and fundamental base."

But in Volume 4 (1789) (see p. 968 of MERCER's ed.), BURNEY in describing RAMEAU's system does not say anything about the harmony of all overtones. "After frequent perusals and consultations of RAMEAU's theoretical works, and a long acquaintance with the writings of his learned commentator D'ALEMBERT, and panegyrists, the Abbé ROUSIER, M. DE LA BORDE, &c. if anyone were to ask me to point out what was the *discovery* or *invention* upon which his system was founded, I should find it a difficult task."

bodies it is false. Bodies apt for musical use are specially selected, rare in comparison to other sorts, being in fact those whose audible overtones harmonize. As we shall see, EULER and DANIEL BERNOULLI are to obtain many examples contradicting the harmony of overtones, and HELMHOLTZ[1]), writing 150 years later, after asserting that RAMEAU's theory is based on the "naturalness" of chords, rejects it: "... if RAMEAU had listened to the effects of striking rods, bells, and membranes, or blowing over hollow chambers, he might have heard many a perfectly dissonant chord. And yet such chords cannot but be considered equally *natural.*" But this is unfair, for to determine the pitches of overtones precisely is not so easy as mere "listening", and when RAMEAU wrote, almost the only scientific datum then published was MERSENNE's claim, apparently supported by SAUVEUR, that all overtones are harmonious (above, pp. 31, 122).]

RAMEAU's later writings[2]) describe a greater variety of acoustical experience [but seem

1) In the "Retrospect" at the end of Part II of *Die Lehre von den Tonempfindungen,* 5th ed., Braunschweig, 1896; translation by A. L. ELLIS from the fourth edition, *On the sensations of tone as a physiological basis for the theory of music,* London, 2nd ed., 1885.

2) RAMEAU is an obscure and graceless writer, whose disconnected wordy conglomerates of details and opinions contrast strangely with the precision and elegance of his music. It is difficult to ascertain what he really believes to be the acoustical facts.

In the preface to his *Génération harmonique ou traité de musique théorique et pratique,* Paris, Prault, 1737, [xiii] + 227 + [xvii] pp., RAMEAU speaks of "the sound born from the totality of the sounding body, with which at the same time resound its octave, fifth, and major third ..." The work opens with a series of propositions and experiments, most of which are taken, without mention of their source, from MERSENNE and SAUVEUR, except for a fantastic theory of propagation of sound in air (Prop. III) which RAMEAU acknowledges having adopted from MAIRAN. Prop. V concerns the "commensurable particles" of a body; equally incomprehensible is Prop. VII: "The most commensurable sounds are those which intercommunicate their vibrations the most easily and strongly; therefore, the effect of the greatest common measure among sonorous bodies which intercommunicate their vibrations by the intermediary of the air should prevail over that of any other aliquot part, since this greatest common measure is the most commensurable."

Experiment III mentions that the fifth and eighth harmonics may be audible, and even sometimes the seventh. RAMEAU writes here that the same harmonics are audible in "every other sonorous body, even in the voice." To prove that no other overtones occur, try to imagine them first and then produce them; "even so you will not perceive them."

RAMEAU's closest approach to a realistic appraisal of the sequence of overtones comes in Experiment VI. "Hang up a tuning fork by a slender thread, each end of which you apply to an [ear]. Strike it; you will perceive at first only a confusion of sounds, which will prevent you from discerning any of them; but, the highest ones gradually abating ...," you will hear only the fundamental, the twelfth, and the seventeenth. *Cf.* also his remarks about "insupportable cacophony" in Experiment IV and his reiteration on p. 28.

From this work it is not certain what RAMEAU means by "corps sonore"; it is possible that the only "bodies" of interest to him are those used for musical instruments. While not making his acoustical beliefs correct, such a restriction would render them not obviously ridiculous for the year 1737. RAMEAU has grudgingly admitted the existence of disharmonious higher overtones, such as the seventh, even in to be a confused medley of fact, error, and special pleading]. One of his remarks[1]), however, [anticipates a suggestion to be made later by JORDAN RICCATI (below, p. 280) and to be developed by HELMHOLTZ:] "What has been said of sonorous bodies should be applied equally to the fibres which carpet the bottom of the ear; these fibres are so many sonorous bodies, to which the air transmits its vibrations, and from which the perception of sounds and harmony is carried to the soul."

In 1709 CARRÉ had confirmed MERSENNE's law (9) for vibrating rods, in the form $\nu \propto V^{-1/3}$, where V is the volume[2]). The tones of rods do not obey the law (10) appropriate to

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musical instruments, but they do not fit in with his earlier numerical preferences, so he skirts about them as lightly as possible, claiming that they are indiscernible in practice, unless perhaps in "cacophony".

It is a different matter with the official report of MAIRAN, NICOLE, and D'ALEMBERT, acting as a commission for the French academy: *Extrait des registres de l'académie royale des sciences du 10 decembre 1749*, printed as pp. *j—xlvij* of RAMEAU's *Démonstration du principe de l'harmonie servant de base à tout l'art musical théorique & pratique* . . ., Paris, Durand & Pissot, 1750, xxiij + 112 + *xlvij* pp. This report states that RAMEAU's system is founded upon "the two following experiments:

"1° If a sonorous body is caused to sound . . ., one hears in addition to the principal *sound* two other *sounds*, very high, one of which is the twelfth above the principal *sound*, . . . and the other is the *major* seventeenth . . .

"2° If one brings up to the body . . . four other bodies, the first of which is at the twelfth above, the second at the *major* seventeenth above, the third at the twelfth below, the fourth at the *major* seventeenth below; then in sounding the body . . . one will see the first and second bodies tremble in their entirety. As to the third and fourth, they will divide themselves by a kind of undulation, the one into three, the other into five equal parts . . ."

These experiments are attributed to MERSENNE and WALLIS. The report upholds RAMEAU's views without qualification and concludes that in consequence "harmony . . . has become a science more geometrical . . ."

What may be passed off lightly as inaccurate wording and insufficient knowledge of acoustical facts in a musician writing in 1737, the reader of §§ 23—24, 27 and 29 of this history will agree to be inexcusable in a mathematician or physicist writing in 1749. Rather, this report fits into what seems to be a general policy of D'ALEMBERT, to the rather considerable extent that he controlled or influenced organs for the popularisation of mathematical and physical science in his day, to keep from general knowledge and appreciation the great acoustical discoveries of DANIEL BERNOULLI and EULER. See below, p. 245, Note 3.

1) Prop. XII, *Génération harmonique.*

2) "*De la proportion que doivent avoir les cylindres pour former par leurs sons les accords de la musique,*" Mém. acad. sci. Paris 1709, 4to ed., Paris, 47—62 (1711) = [2nd] 4to ed., Paris, 47—62 (1733) = 12mo ed., Amsterdam, 57—76 (1711). See also the Histoire, 4to eds., Paris, 93—96 (1733) = 12mo ed., 117—121. CARRÉ claims that the vibrations of rods are "circular as well as longitudinal" and tries to replace MERSENNE's rule by one separating the effects of length and surface area (not cross-section). He claims to prove that full geometrical similarity is necessary in order that the tones of two bars harmonize. His theory is no more than guesswork; CHLADNI later pronounced CARRÉ's experiments "set up as incorrectly as they are described;" *cf.* p. 13 of *op. cit. infra*, p. 335.

strings. In commenting upon his work, DE LA HIRE[1]) states that when a wooden cylinder is struck, "there are always toward its two ends two places where the sound is considerably damped and virtually extinguished. It does not matter what are the dimensions of the cylinder . . ." [Thus, apparently, he had observed the two nodes occuring in the fundamental mode of free vibration of a rod with both ends free.] Some years later DE LA HIRE[2]) reported some further experiments, [haphazardly conceived and vaguely described,] in which the same rod gives out different sounds if struck in different ways. [*I. e.*, by accident he observed *two or more different modes* of elastic vibration of a bar.] He finds that when a suspended rule is struck on the flat side, the tone is higher than when it is struck on the edge, but "the place . . . where the sound was damped" is the same. [These are the first vague hints that the nodal ratios are independent of the form of the cross-section, but the frequency depends upon the depth and breadth in different ways.] DE LA HIRE goes off into a physical theory of how sound is caused by air being forced out of the pores of an elastic body.

In a letter[3]) of October 1715 to LEIBNIZ, ZENDRINI criticizes the work of CARRÉ: ". . . I have not been able to agree with his reasoning or experiments. . . . I have tried . . . striking several wooden cylinders and comparing their sounds with musical strings. It turns out that by striking various points of a wooden cylinder I perceived in a certain and determinate spot a higher tone than in the remaining." While ZENDRINI speaks of "a body of any form", his figure and language describe a rod-like body of revolution, and he asserts that there are two and only two circles on which such a body may be struck so as to give out the higher tone. [Since ZENDRINI does not describe how the rod is supported, it is difficult to know what modes he has observed; the two circles suggest the fundamental mode of free-free vibration, but we are left wondering to what mode the previously observed lower tone corresponds.] "The striking excites waves in a solid body none the less than waves are generated in quiet water by the blow of a stone, but with this difference, that in the fluid the waves cleave to the surface, while in solid bodies they penetrate the thickness of the body and diffuse themselves as far as the opposite surface." He then attempts to explain the two different tones on the basis of reinforcement or interference of the waves as they travel through the body and are reflected from its surface. His physical idea seems to be that "mute spots", where the greatest interference of the waves takes place, are the places where a rod should be struck in order to give out a higher tone. In order to get hold

1) Reported in Hist. acad. sci. Paris 1709, 4to ed,. Paris, 96—97 (1711) = [2nd] 4to ed., Paris, 96—97 (1733) = 12mo ed., Amsterdam, 121—122 (1711).

2) "*Experiences sur le son*" and "*Continuation d'experiences sur le son*," Mém. acad. sci. Paris 1716, Paris ed., 4to, 262—268 (1718) = 12mo ed., Amsterdam, 335—342 (1719).

3) The brief but important correspondence between LEIBNIZ and ZENDRINI is given in LEIBNIZENS Mathematische Schriften (I) 4, 227—251 (1859).

on something one can compute, ZENDRINI brings in the cross-section of least resistance according to VARIGNON's formula; rectilinear rays, the normals to the waves, if drawn from this section to the end of the rod will intersect in a point where the waves interfere with one another, rendering the cross-section "mute". In this way he finds the mute spots for a circular cylinder to lie one quarter of the way down the rod, "which answers perfectly to experience." [*Cf.* this value, 0,250, with HUYGENS' value, 0,207 (above, p. 49); the correct value for the fundamental mode of free-free transverse vibration according to the BERNOULLI-EULER theory lies just midway between them.]

ZENDRINI indicates how to find the mute spots in a cone or an egg; for the latter, there is only one plane of mute spots. ZENDRINI's experiments have confirmed all cases he has calculated. He conjectures that the bridge of a stringed instrument is placed at a mute spot and that drums also have such spots.

LEIBNIZ's answer of 4 November 1715 contends that the analogy to waves on water is a poor one, since in water "the waves are only on the surface and arise from gravity, not from elastic force, but I do not deny nevertheless that also this propagation may be called by the name of waves. What you have observed about wooden cylinders might help, perhaps, in explaining the structure of wood itself." LEIBNIZ points out that other kinds of vibration are possible. In a hollow cylindrical tube, for example, the motion resulting from

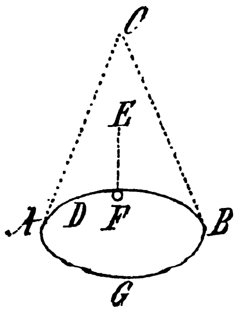

Figure 45. The experiment LEIBNIZ writes in 1715 that he had proposed to MARIOTTE (before 1684) to demonstrate the propagation of transverse vibration around the circumference of a circular ring

a blow on one side is transmitted, not across the cylinder, as ZENDRINI had assumed, but around the surface. Vibrations of this kind "were tried long ago by Mr. MARIOTTE at my suggestion. Hang up a horizontal circle AB (Figure 45) from a point C and then hang a little ball from a point E so that it touches the circle on the inside at F; if, then, you strike the circle with a stick on the outside at G, just opposite to F, the little ball D will come toward the striker, because the circle AB is transformed into an ellipsoid [*i. e.* an ellipse-like curve] with the points F and G approaching one another." [LEIBNIZ's experiment of 1684 or earlier demonstrates the existence, for a curved rod, of transverse vibrations in the plane of the rod.]

To this ZENDRINI replies on 5 January 1715/6 that he does not see that the results have anything to do with the structure of wood; he has done the experiments also with iron cylinders, with the same acoustical results, while the structures of wood and iron are entirely different.

On 15 March 1716 LEIBNIZ writes to ZENDRINI, "I should like it if some outstanding musician who is at the same time a remarkable mathematician would enter that ocean of the subject of sound, scarcely navigated until now, first leaving the shore and little by little coming out into the high seas, that is, beginning with rather simple experiments. Thus I should hope that most things could be reduced to mathematical-mechanical arguments . . . Which sonorous properties are common to bodies of iron, wood, earth (or baked clay), so as not to depend upon the peculiar structure of the body . . ., the difference between bodies continuous and those contiguous or glued together, as again between homogeneous and heterogeneous . . . the sound of a cavity is changed when liquid is poured in. And liquids vary among themselves or if combined with solids; water covered with water sounds much clearer than when a hard body covers it."

[ZENDRINI, who experimented, was ready to base a theory on tenuous hypothecated analogies between sound waves and other waves, while LEIBNIZ, who did not experiment himself, called for a preliminary experimental program so as to classify the kinds of possible vibration. LEIBNIZ seems, however, to have forgotten his earlier remark that the elastic and acoustic properties of bodies must be related (above, p. 63).

This is the last we shall hear of LEIBNIZ. In the history of physics, he has been too little valued. In our subject, despite the small proportion of his effort given to it, we have seen that from 1684 to 1716, the year of his death, his influence was great, his knowledge of particulars was extensive and accurate, and his insight was sound[1]). Each of his two published notes, besides being a landmark for all time, displays a perfect command of the principles of mechanics as then they stood. Had LEIBNIZ written no more, these ten pages would have made him a famous mathematician forever. What is most remarkable is that his private recommendations pertain to limited mechanical objectives as entering wedges toward greater things, rather than *vice versa*, and call for an intelligent interrelation of theory and experiment maintained, within the scope of this history, only by HUYGENS and DANIEL BERNOULLI.

We have seen that little or nothing was known by 1716 about vibrating bodies other than strings; moreover, the two most interesting studies, namely, the incorrect theories of HUYGENS and ZENDRINI, were to remain unpublished until a century after the later

1) The experimenter MUSSCHENBROEK writes of LEIBNIZ, "... from what he confided to the public light it may plainly be inferred that the most noble mathematician had examined not only those things that he brought forth but also innumerable others which he kept back as being of lesser worth" (p. 427 of *prim. op. cit. infra*, p. 151).

triumph of the BERNOULLI-EULER theory. With the vibrations of a string, however, the situation was different, for the main experimental phenomena were known. We now follow two mathematical researches which could easily have explained them, had not their authors, it seems, resolutely closed their eyes to the experiments in a unique concentration upon the fundamental mode.]

17. TAYLOR's analysis of the continuous vibrating string (1713). The calculation of the [fundamental] period of a vibrating string was first achieved in a celebrated memoir of TAYLOR, *On the motion of a taut sinew*[1]). Lemma 1 presents a geometrical argument showing that for two similar curves $y = \alpha f(x)$ and $y = \beta f(x)$, the ultimate ratio of the curvatures as $\alpha \to 0$ and $\beta \to 0$ is α/β. Lemma 2 reads, "In any aspect of its vibration, let the taut sinew between the points A and B take on the form of any curve $Ap\pi B$. Then I say that whatever be the increment of velocity of any point P, that is, its acceleration arising from the tension stretching the sinew, it will be as the curvature of the sinew at that point" (Figure 46). The proof follows. "Imagine the sinew to consist of infinitely small rigid particles pP and $P\pi$, *etc.*, and at the point P erect the perpendicular $PR =$ the radius of curvature at P. It is intersected at t by the tangents pt and πt, at s by the lines πs and ps parallel to them, and at c by the chord $p\pi$. Then, by the principles of mechanics, the absolute force by which the two particles pP and $P\pi$ are drawn toward R will be to the tension of the wire as st to pt, and the half of this force, which acts on the one particle pP, will be to the tension of the sinew as ct to tp, that is (on account of the similar triangles ctp, tpR) as tp or Pp to Rt or to PR. Therefore, on account of the given force of tension, the absolute accelerating force will be as Pp/PR. But the acceleration produced is in the ratio of the absolute force divided by the matter to be moved; and the matter to be moved is the particle Pp. Therefore the acceleration is as $1/PR$ . . ." [Thus TAYLOR's argument, if somewhat obscurely[2]), *calculates the resultant normal force acting on an infinitesimal element of the string*, obtaining

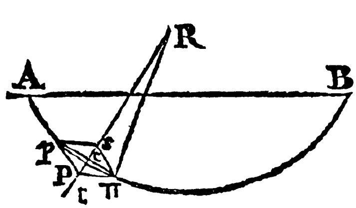

Figure 46.
TAYLOR's figure for analysis of the vibrating string (1713)

1) "*De motu nervi tensi*," Phil. trans. London 28, No. 337 (1713), 26—32 (1714). I have not consulted the translated and abridged ed., "*Of the motion of a tense string*," Phil. trans. abridged 6, 14—17.

An attempt to determine the frequency of a string from certain statical assumptions which are not clear to me is made by SAUVEUR in his last paper, "*Rapport des sons des cordes d'instrumens de musique, aux flèches des cordes; et nouvelle détermination des sons fixes*," Mém. acad. sci. Paris 1713, 4[to] ed., Paris, 324—350 (1716) = [2[nd]] 4[to] ed., Paris, 324—350 (1739). See esp. § 47.

2) Despite the introduction of the chord $p\pi$, the argument seems to be correct.

$$\text{(74)} \qquad \sigma A_n = -F_n \propto \frac{T}{r}, \qquad A_n = \text{normal acceleration.}$$

Indeed, the statical principle, at least to within a constant factor, is that expressed in JAMES BERNOULLI's unpublished formula (40). Not only does TAYLOR obtain the result independently, but also by applying to it the Newtonian principle on "the acceleration produced", he adds something new: *This is the first time the momentum principle is applied to an element of a continuous body.* From (74) the *modern* reader will conclude at once that small motions are governed by the partial differential equation

$$\sigma \frac{\partial^2 y}{\partial t^2} = T \frac{\partial^2 y}{\partial x^2} .$$

While indeed small motions are TAYLOR's objective, his result (74) is an equation valid for *finite motions* of a perfectly flexible string, as indeed his figure suggests. Moreover, he does *not* approximate (74) by the wave equation. Instead, after this brilliant beginning, he wanders into a morass of special assumptions and errors.]

His Problem 1 is "To determine the motion of a taut sinew." The displacement from the axis AB is assumed small, "so that the increment of the tension from the increase of length, as well as the obliquity of the radii of curvature, may safely be neglected." Applying a plectrum at the midpoint, deform the string into a triangular form. When the plectrum is removed, only the apex will move, by Lemma 2. "But then by the bending of the sinew at points nearby . . ., those points too will begin to move, and so on . . ." Each point moves fastest when it first begins, more slowly thereafter, since the curvature decreases. This occurs in such a way "that since the forces are properly tempered among each other, all the motions conspire together so that all points simultaneously reach the axis and recede from it, back and forth ad infinitum.

"But for this to happen, the sinew should always take on the form of a curve such that in any point the curvature is as the distance of that point from the axis." What TAYLOR now attempts to prove is the converse, that if the curvature is as the distance, then all points of the string reach the axis simultaneously; [this is true, under the hypotheses made, but TAYLOR's argument is obscure if not faulty.] A second argument invokes Lemma 1 as well as Lemma 2. It then follows that the acceleration of each point is as its distance from the axis. By a known theorem, the vibrations are isochrone, and the motion of an arbitrary point is that of a simple pendulum. [The argument is now logically correct but trivial, since to apply Lemma 1 to the motion of a string we must *assume* that $y = f(t)\, g(x)$, and most of what was to be proved follows *a fortiori*.] As a corollary, $r = a^2/y$, where $a = \text{const.}$

Problem 2 is, "Given the length and weight of a sinew, along with the stretching weight, to find the time of one vibration." TAYLOR's result, in modern notation, is

$$\nu = \frac{1}{2l}\sqrt{\frac{T}{\sigma}}\,. \tag{75}$$

[Thus, extending GALILEO's proportion (10) and HUYGENS' unpublished approximation (17), he calculates from theory the *fundamental frequency of the vibrating string.*] The argument is divided into two parts. The first sets up a precarious analogy between the motion of the central point on the string and the motion of a pendulum whose length is the amplitude of vibration of the central particle, $\mathfrak{A}$. The result is, in effect, $\nu = \frac{1}{2\pi\mathfrak{A}}\sqrt{\frac{T}{\sigma}}$. The second part, "to find the line," combines the general formula $r = \dot{s}\dot{y}/\ddot{x}$ [when $\dot{s}=$ const.] with $r = a^2/y$, yielding the differential equation $a^2\ddot{x} = \dot{s}y\dot{y}$. "Taking the fluent" [*i. e.* integrating] yields $a^2\dot{x} = \frac{1}{2}\dot{s}y^2 - \frac{1}{2}\dot{s}\mathfrak{A}^2 + \dot{s}a^2$, where the constant of integration is adjusted so that $\dot{x} = \dot{s}$ "at the midpoint" [*i. e.*, when $y = \mathfrak{A}$]. Putting $\dot{s}^2 = \dot{x}^2 + \dot{y}^2$ and solving for x yields a quadrature; TAYLOR supposes that "$\mathfrak{A}$ and y vanish with respect to a," simplifying the quadrature to $\dot{x} = a\dot{y}/\sqrt{\mathfrak{A}^2 - y^2}$. Hence

$$y = \mathfrak{A}\sin\frac{x}{a}\,. \tag{76}$$

Putting $y = \mathfrak{A}$ corresponds to $x = \frac{1}{2}l$; hence $a = l/\pi$. Putting this result into the above derived expression for ν yields (75).

[Possibly TAYLOR's work was criticized for the manifest contradiction between the initially triangular form assumed in Problem 1 and the sinusoidal form (76) given as the result. In the revised form presented in his book[1]), the indication of the finite velocity of propagation of a disturbance has been removed. In its place is a passage concluding "Therefore, in whatever way the sinew is struck [initially], it very quickly takes on the form of curve here described," *i. e.*, one in which the curvature is proportional to the displacement [and hence a sine curve. Thus began an error that was to hang on for half a century. While indeed the effects of friction may be such as to cause the form of a vibrating string to become more nearly sinusoidal as the motion subsides, nothing of the sort is mentioned here.] TAYLOR's argument is purely dynamical [and fallacious.

From this extraordinary performance we see that TAYLOR had within his hands the correct dynamical principles and the partial differential equation we now regard as governing the whole problem, but he turned aside from them to the special and restricting hypothesis that the curvature is as the displacement; thus all that could emerge from his analysis are the sinusoidal forms and the periods of the simple modes; the fundamental

1) Pp. 88—93 of *op. cit. ante*, p. 86, Note 3. Here (74) is not stated so clearly as in the original paper; *e. g.*, only the word "accelerating force", not the word "acceleration" is used. The calculation of the frequency is slightly more direct.

he did in fact obtain, though by a roundabout and scarcely convincing argument. At that, however, he did not apply to his hypothesis even so simple an idea as that the acceleration is $\partial^2 y/\partial t^2$. What is missing is the calculus of partial derivatives. Indeed, even to speak of the acceleration of a particle on the string requires some concept of partial differentiation, but in TAYLOR's work we find no sign that the partial derivative was included among the entities he could manipulate even in the simplest contexts.

From this jumble of brilliance and error in principle, little but confusion could result[1]).]

18. JOHN BERNOULLI's analysis of the loaded vibrating string (1727). Among the *Selected theorems to be proved as illustrations of the conservation of live forces and to be confirmed by experiments*[2]) which JOHN BERNOULLI communicated to his son DANIEL in 1727 are the following. Theorem IV asserts that TAYLOR's formula (75) is correct; [this simple restatement implies that BERNOULLI saw the insufficiency of TAYLOR's derivation.] Theorems V—VII concern the *weightless string loaded by n equally spaced and equal masses M/n*. [While this model had been used by HUYGENS (above, pp. 45, 49), his work remained unpublished, and his methods were inadequate; here we see the first publication of a partially satisfactory theory for small vibration of a system of several degrees of freedom.] If we write $\nu^{(n)}$ for the [fundamental] frequency for n masses and ν for TAYLOR's value (75) for the continuous string, then, putting $\sigma = M/l$, we may express JOHN BERNOULLI's results as follows:

$$\begin{aligned}
&\frac{\nu^{(1)}}{\nu} = \frac{1}{\pi}\cdot 2\,, \quad \frac{\nu^{(2)}}{\nu} = \frac{1}{\pi}\cdot\sqrt{6}\,, \quad \frac{\nu^{(3)}}{\nu} = \frac{1}{\pi}\cdot 2\sqrt{6-3\sqrt{2}}\,,\\
&\frac{\nu^{(4)}}{\nu} = \frac{1}{\pi}\cdot 2\sqrt{\frac{5(5-\sqrt{5})}{5+\sqrt{5}}}\,, \qquad \frac{\nu^{(5)}}{\nu} = \frac{1}{\pi}\cdot\sqrt{60-30\sqrt{3}}\,,\\
&\frac{\nu^{(6)}}{\nu} = \frac{1}{\pi}\cdot\sqrt{\frac{42x^2-126x+168}{2x^2+x+1}}\,,
\end{aligned} \tag{77}$$

where x is "the root" [or "a root"?] of $x^3 - x^2 - 2x + 1 = 0$. "By the same method,

1) Immediately a solution was published by HERMANN, "*De vibrationibus chordarum tensarum disquisitio,*" Acta erud. August 1716, 370—377. HERMANN regards the motion as arising from the linear elasticity of the string. Somehow he concludes that the resultant transverse force for small displacement y is Ty/l, where T is the tension in the undisturbed state. He then supposes the entire mass of the string to move as a mass-point subject to this tension. This is not even a discrete model such as that forming the beginning of HUYGENS' treatment (Figure 11, p. 48) but merely juggling to get (75) for an answer.

2) "*Theoremata selecta, pro conservatione virium vivarum demonstranda et experimentis confirmanda, excerpta ex epistolis datis ad filium DANIELEM, 11. Oct. & 20. Dec. (stil. nov.) 1727,*" Comm. acad. Petrop. 2 (1727), 200—207 (1729) = Opera omnia 3, 124—130.

which I have, I can go on to determine the number of vibrations for a string loaded by more weights, but I turn to other matters."

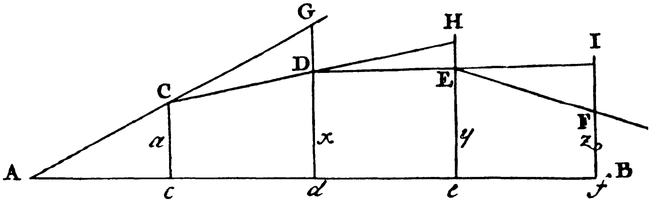

Figure 47
JOHN BERNOULLI's analysis of the vibrations of a loaded string (1727)

The proofs are given in a short paper dense with equations: *Thoughts on vibrating strings, loaded by little equidistant weights, where the number of vibrations . . . is sought from the principle of live forces alone*[1]). JOHN BERNOULLI says that the vibrating string (Figure 47) "must compose itself into such a shape that all the little weights simultaneously reach the line AB, whence it follows that the velocities of the several particles, as well as their accelerating forces, must be proportional to the distances to be travelled Cc, Dd, Ee, *etc.*" To follow BERNOULLI's argument, consider the k^{th} particle at the center of Figure 48. The tension T is taken as constant, and the accelerating force F_k arising from the tension is assumed purely transversal [these assumptions are justifiable only for small motion]. Then by equating the projections of the forces on to the direction normal to the left-hand segment we obtain $F_k \sin\psi_k = T\sin\varphi_k$. "Since the shape is almost a straight line," $\sin\psi_k \approx 1$, $\sin\varphi_k \approx z_{k+1}/a$. From the geometry of the figure, we have exactly $z_{k+1} = 2y_k - y_{k-1} - y_{k+1}$. Hence

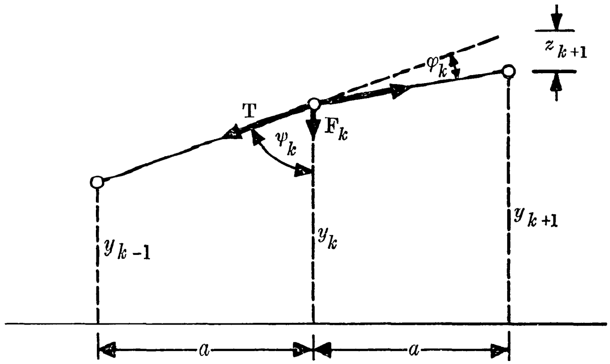

Figure 48. Variables used in JOHN BERNOULLI's analysis of the vibrations of a loaded string

$$F_k \approx \frac{T}{a} z_{k+1} = \frac{T}{a}(2y_k - y_{k-1} - y_{k+1}) \,. \tag{78}$$

[Thus BERNOULLI calculates in full generality the *restoring force* on the k^{th} particle, subject only to the hypothesis of a nearly rectilinear form.] By hypothesis, $F_k \propto y_k$. Hence

$$\frac{2y_k - y_{k-1} - y_{k+1}}{y_k} = \text{const.} \tag{79}$$

1) "*Meditationes de chordis vibrantibus, cum pondusculis aequali intervallo a se invicem dissitis, ubi nimirum ex principio virium vivarum quaeritur numerus vibrationum chordae pro una oscillatione penduli datae longitudinis D,*" Comm. acad. Petrop. **3** (1728), 13—28 (1732) = Opera omnia **3**, 198—210.

[We note how close BERNOULLI comes to establishing the equations of motion, yet he fails to do so, being misled by TAYLOR's hypothesis.] For each of the cases $n = 2, 3, \ldots, 7$, BERNOULLI works out a solution of (79). Each time his solution is such that all displacements are of the same sign. *E. g.*, for $n = 2$ he has $y_1 = y_2$; for $n = 3$, he has $y_1 = y_3$ with y_2 adjusted accordingly, *viz*, $y_2 = y_1\sqrt{2}$; and only for $n = 7$ does he mention any other solution, but then he says, "it does not belong here." [With the whole set of principal modes for the loaded string standing before him, he refuses to notice any but the fundamental.]

Next BERNOULLI turns to calculation of the frequency. The tension is regarded as caused by a weight hung over a pulley at A (Figure 49). The "descent of the weight" is the distance it has to be lifted in order for the string to assume its present configuration; for the case shown in Figure 47, this is $AFGHB - ACEIB$, which BERNOULLI finds to be $2(2-\sqrt{2})\,\mathfrak{A}_1^2/a$, where $\mathfrak{A}_1$ is the displacement CF of the first weight, F. By the principle of *vis viva*, this must equal the kinetic energy of the weights when they cross the line AB. At this point BERNOULLI recognizes the fact that the initial assumption $F_k \propto y_k$ implies simple harmonic motion. In particular, the velocity v_k with which the particle crosses the axis satisfies $v_k \propto \mathfrak{A}_k$. Thus, again for the case when $n = 3$, we have $v_2 = \sqrt{2}\,v_2$, and hence the total kinetic energy is $\frac{1}{3}M\cdot v_1^2 + \frac{1}{3}M\cdot 2v_1^2 + \frac{1}{3}M\cdot v_1^2$. Therefore

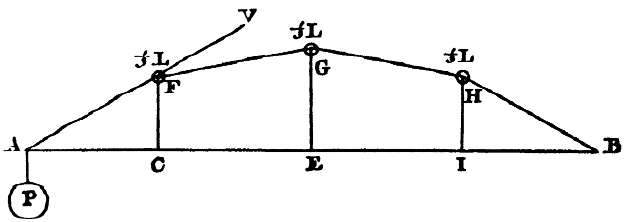

Figure 49
JOHN BERNOULLI's use of the principle of live forces (1727)

$$\frac{4}{3}Mv_1^2 = 2(2-\sqrt{2})\,\frac{\mathfrak{A}_1^2}{a}\cdot T = 8(2-\sqrt{2})\,\mathfrak{A}_1^2\,\frac{T}{l}\,. \tag{80}$$

Since $v_1 = 2\pi\nu^{(3)}\mathfrak{A}_1$, substitution in (80) yields an expression for $\nu^{(3)}$ which is in fact $(77)_3$. The calculations grow more and more complicated, but BERNOULLI carries through the same method as far as $n = 6$.

BERNOULLI then gives "solutions of the same problems from the principle of statics." Here everything rests on Lemma IV, which calculates the frequency of a motion $x(t)$ of a body of mass $\mathfrak{M}$ subject to a force[1] $-Kx$, *viz*, $\nu = \dfrac{1}{2\pi}\sqrt{\dfrac{K}{\mathfrak{M}}}$. As he says in the scholion, by this method we may treat a *general* number of weights, n. For by (78) follows

1) As far as I can learn, this passage contains the first treatment of simple harmonic motion by straightforward integration of the differential equation. *Cf.* above, pp. 56—57.

$$F_1 = \frac{T}{a}(2y_1 - y_2) = \frac{T}{a}(2 - \alpha_n)\, y_1 \,,$$

where $y_2 = \alpha_n y_1$. Thus, since $M = \frac{1}{n}\mathfrak{M}$, in full generality we have

$$\frac{\nu^{(n)}}{\nu} = \frac{1}{\pi}\sqrt{n(n+1)(2-\alpha_n)}\,. \tag{81}$$

All that is needed now is the factor α_n. This, for $n = 1, 2, \ldots, 7$, was determined earlier [but plainly BERNOULLI is unable to determine it for general n; to find it is equivalent to finding the solution of the difference equation (79) such that $y_k > 0$ for $k = 1, 2, \ldots, n$ and $y_0 = y_{n+1} = 0$]. For example, for $n = 7$ we have $\alpha_7 = \sqrt{2+\sqrt{2}}$, hence

$$\frac{\nu^{(7)}}{\nu} = \frac{1}{\pi}\cdot 2\sqrt{14\left(2-\sqrt{2+\sqrt{2}}\right)}\,. \tag{82}$$

Coming finally to the continuous string, BERNOULLI quickly calculates the restoring force on an element as being $T\frac{d\theta}{ds}$, where θ is the slope angle [this is essentially TAYLOR's result $(74)_2$ and in any case was familiar to BERNOULLI from the unpublished researches on the catenary]. "But it must be noted that the curve . . . is an elongated companion of the trochoid" [*i. e.* sine curve]. On this assumption $d\theta/ds$ is easily calculated, and the period (75) follows at once by the second method used for the loaded string. BERNOULLI gives also a proof based on conservation of live force; for this, a more accurate calculation of the curvature is necessary.

The paper ends with a proof that the shape of the string must be sinusoidal. Since the restoring force is proportional to the curvature, at any instant we have $\frac{1}{r} \approx \frac{d^2y}{dx^2}$; by hypothesis, the restoring force is proportional to y. Hence $\frac{d^2y}{dx^2} \propto y$, whence the assertion follows.

[The reader cannot fail to be disappointed. After the brilliant start expressed by (79), BERNOULLI has shut his eyes to the real problem three times over. The calculations of the fundamental frequencies are of course correct, and the general formula (81) is clever. But there is no hint that other frequencies can occur and no indication that the frequency satisfies a polynomial equation of degree n. Here we look for recognition of the *problem of proper frequencies*, but we look in vain[1]). Rather, following TAYLOR, BERNOULLI insists at every turn that the force must vary as the displacement. On the one hand, in demanding that all particles cross the axis simultaneously, BERNOULLI seems to realize that he imposes a restriction. However, he fails to find the other modes sharing this property. His

1) The description of his paper given by BURKHARDT, § 1 of *op. cit. ante*, p. 11, while technically not incorrect, gives a partly false impression by describing what the problem really is rather than the problem as BERNOULLI himself handled it.

treatment of the continuous string is shorter and clearer than TAYLOR's in that he uses TAYLOR's assumption $F \propto y$ directly, without TAYLOR's detours, but in principle both are alike[1]).

19. Summary: EULER's heritage. Our scene is ready for the man destined to take up the theory of deformable bodies and by slow degrees exalt it, as he did most parts of mathematics, to a perfection scarcely thought possible from the material as it first came to his hands. This man is EULER. We cast the sum of his heritage in our subject.

The past ninety years had seen the field of elastic and flexible bodies opened by drives upon five largely isolated special problems. Should the reader contrast our following survey of them with his own immediate impression from a sample of the old writings, he will find little in common. Indeed, a statistical summary of the papers of the late seventeenth century would reflect little more than a mass of "constructions" relating one curve to another. This helps in understanding the researches in the following quarter century, since for the students of that time it must have been even harder to extract the real thoughts from the endless differentiations and integrations in which they were entwined.

PROBLEMS

I. *Equilibrium of flexible lines.* The general differential equations, both in rectangular co-ordinates (39) and intrinsic co-ordinates (40) (42), were derived correctly by JAMES BERNOULLI and finally published by HERMANN. For the most interesting special cases, explicit solutions were found. In this sense, the problem was closed. However, real chains or strings show some measure of stiffness, and a theory taking account of it was lacking. (See Problem V below.)

The basic concept for the theory of flexible curves was the *tension*, evolved from special cases by JAMES BERNOULLI and published by HERMANN (above, pp. 81, 87).

II. *Small vibrations of flexible or elastic bodies.* TAYLOR's formula (75) for the frequency of the continuous vibrating string and JOHN BERNOULLI's formulae (77) for the string loaded by one to seven masses were definite achievements. For the loaded string, it was not shown that JOHN BERNOULLI's method, which seems to rest on guessing a part of the solution of (79) and then calculating the rest by laborious elimination, really would go through for an arbitrary number of weights. The work of both authors was misleading if not erroneous from special assumptions which a modern reader sees at once confine the results first to the simple modes and then to the fundamental. Neither author recognized the true nature of the problem, either mechanical or mathematical. There is no hint of a

1) The earliest researches of EULER on vibrating systems, which began in connection with this paper by his teacher, are described in footnote 1, pp. 142—143.

spectrum of frequencies or, apart from the errors TAYLOR bequeathed to his successors for half a century, of any other form of the string than that to which the exhibited frequencies belong. Indeed, what are lacking are the *equations of motion*. These TAYLOR, and to some extent also JOHN BERNOULLI, had within their hands but cast aside.

Generalizations of the problem, as for example to a heavy chain hung from one end, could be attacked by the same methods, with the same measure of success and failure to be expected.

That elastic vibrations are of a different kind was recognized, but the only definite result concerning them was MERSENNE's empirical formula (9), apparently little known. LEIBNIZ's suggestion that the elastic and acoustic properties of bodies are connected had not been followed except in unsatisfactory work by RICCATI; it is soon to be made definite by EULER. That the vibrations of a given body, whether elastic or flexible, may occur at several different definite frequencies, to each of which corresponds a motion with a definite number of nodes, should have been clear from the experiments organized by SAUVEUR, but the theorists took no heed of it, leaving the field clear for DANIEL BERNOULLI.

III. *Rupture*. The problem of rupture, apparently, is ill adapted to mathematical treatment and remains today unsolved. It gave rise to the LEIBNIZ-VARIGNON formula (61) for the bending moment acting upon a cross-section of a beam when the bending itself is neglected but the tension is assumed to vary linearly over the cross-section.

IV. *Extension*. HOOKE's linear relation (18) was known to all the following students of the subject but esteemed lightly. To the extent they consider deformation at all, the early researches always concern *finite deflection*, for which indeed (18) is rarely appropriate. On the one hand, linearization as a device for cutting the problem down to the size of the man was reserved for a later age to discover. On the other, the early geometers failed to exploit the implications of a fact they all knew, namely, that large forces may accompany scarcely perceptible changes of shape. JAMES BERNOULLI's parabolic law, elongation $\propto$ (force)^m, had been explored but not found appealing.

While JAMES BERNOULLI had seen that *force per unit area* (stress, τ) and *change of length per unit length* (strain, ϵ) are the proper variables for a theory of elasticity, nothing had been done with these basic concepts. The last work of JAMES BERNOULLI implies the existence of a material constant or modulus having the dimension of stress and specifying the degree of elasticity, but he did not introduce such a modulus explicitly because he wished to avoid assuming any particular stress-strain relation. We shall encounter the modulus of extension in EULER's first paper.

V. *Bending*. The principle expressed by JAMES BERNOULLI's equation (56) or its special case (57), defining the *elastic curve*, was a second major discovery. However, the theory was in a primitive state. First, BERNOULLI's derivations of the basic formulae (45) and (46), relating the bending moment to the curvature, were unsatisfactory. It remained to integrate over the cross-section of a beam, in a word, to unite the LEIBNIZ-VARIGNON formula (61) with JAMES BERNOULLI's formula (45). This will be EULER's first achievement.

While BERNOULLI had stated that the moment is to be taken, at each cross-section, with respect to the point where it intersects the neutral fibre, his theory for calculating the position of that fibre was faulty. A correct and essentially general application of statics to the forces and moments acting upon the cross-section had been given by PARENT, but only at the expense of neglecting the bending. That the neutral line is the central line if the tensions vary linearly over the cross-section was known and stated by BERNOULLI and PARENT. This aspect of the theory is to be disregarded by EULER and nearly all other savants in the eighteenth century.

Second, the true shape of the elastic curve was still a mystery. While a literature grew up around it, this literature, in the style of the day, presented "constructions" whereby the curve *could* be drawn in terms of other curves, but no one drew it. So simple an idea as to perform the quadrature (57) numerically and compare the result with experiment is not to be found, nor did anyone calculate the approximate shape for small deflections. The only concrete results were BERNOULLI's series (51) and the bounds (52) for the rectangular elastica. It is to be EULER's achievement to determine all possible forms of the initially straight band subject to terminal load, besides other elastic curves.

But this is not all. JAMES BERNOULLI's first paper on the elastic band (above, p. 89) concludes with a list of further problems chosen with his usual insight and left to "the industry of our readers". These problems remained *untouched*. Indeed, in this period *only one person*, the great BERNOULLI himself, put in print anything at all concerning the fundamental theory of the elastica. The first among these problems was "to investigate the kind of curves engendered when the elastic band is bent by its own weight in addition to the suspended weight"—in effect, *to unify the theories of the catenary and of the elastica*. How to do this was far from clear, since the most general problem of the flexible line had found its formulation in terms of the tension, while the law of the elastic band was a statement concerning the bending moment. The matter lay quiet until 1724, when in the Acta Eruditorum appeared a note, *The famous problem of the catenary proposed again to the geometers, especially to those who are members of the royal societies of London and Paris*[1]). The rather scornful anonymous proposer speaks of "a rope or little chain not infinitely but rather moderately flexible—if you like, not imaginary but real," such that the slope at the points

1) "*Celebre catenariae problema geometris denuo propositum, iis praesertim qui ex Soc. Reg. sunt, quae Parisiis florent & Londini; Parisiis ad collectores actorum erud. transmissum,*" Acta erud. August 1724, 366—367.

of support may be prescribed, "as is seen to occur not by pure hypotheses but in fact . . . It is easily shown that this case is possible, you cannot doubt it . . . Farewell. LBC." While this challenge seemed to pass unnoticed, and LBC, whoever he was, had to wait several years for an answer, solution of this fundamental problem of principle will furnish the subject of DANIEL BERNOULLI's and EULER's first publications on our subject.

METHODS

I. *Models.* Three mathematical models for real cords, chains, wires, bands, planks, and bars were proposed:

a. *The continuous line* was introduced by PARDIES and used in nearly all later researches. This is not to be disguised by the loose language in which the arguments are sometimes put, as when TAYLOR speaks of the vibrating string consisting in "infinitely small rigid particles".

b. *The line loaded by discrete masses or weights*, thus far equidistant and of equal magnitude, may be related to the continuous line by one of two passages to the limit:

1. *In the equations of motion or equilibrium.* For the simple catenary, this seems to have been done by HUYGENS (above, pp. 66—68).

2. *In the final answer.* In the case of the suspension bridge, where it is easy, this was done in unpublished work of BEECKMAN and HUYGENS (above, pp. 24, 45—46). While it was doubtless JOHN BERNOULLI's aim in connection with the vibrating string, his solution for the discrete model is too fragmentary to be used (above, pp. 132—135).

These models, recognized as distinct[1]) by early writers, all will appear in researches of the eighteenth century.

II. *Theory and experiment.* All the first researchers in our subject turned to experiment for guidance, and most of them experimented themselves. However, they showed little comprehension of what experiment can do and what it cannot. While theory made brilliant progress in the seventeenth century, experiment remained crude, serving virtually as a popular diversion, and it is almost surprising that certain definite results were discovered experimentally[2]). On the one hand, preliminary analysis of the factors that govern an

1) However, this distinction may not always be apparent to a reader accustomed to modern precision of statement. *Cf.* § 2 of BURKHARDT, *op. cit. ante*, p. 11. That in much of the early work the limit passage is unrigorous or even incorrect is of course a different matter.

2) These were:

1. The MERSENNE-GALILEO laws (10) for vibrating strings
2. MERSENNE's law (9) for vibrating rods
3. The existence and coexistence of definite overtones accompanied by nodes, at least for strings
4. HOOKE's law (18) for extension

Even here we should be cautious, for only in Nos. 3 and 4 do the original reports declare and reveal the experimental nature of the discovery. That No. 2 was found out by experiment, the circumstances experiment, such as the dimensionless parameters relevant for its interpretation, was lacking, and the need for reporting the specific data from a sequence of tests rather than one or two isolated cases was not felt. On the other, the geometers, sharply aware of the uncertainty of their hypotheses, turned to some simple experiment for direct confirmation or denial of those hypotheses rather than waiting for a more troublesome check of detailed predictions from resulting solutions. The case of HOOKE's linear law of extension is typical: For any noticeable extension it does not hold for most materials, but the more subtle idea that it could be tested indirectly for unmeasurably small extensions by checking JAMES BERNOULLI's derived formulae for large bending was never suggested. At the same time the geometers, triumphing in the power of their new methods, hastened on to try new problems. Thus remained frustrate the high hopes expressed by all the early theorists that their results find important practical application. Thus began the chasm between elastic theory and elastic experiment or engineering (if such it may then be called) that spread ever wider for a hundred years and more and to this day is not closed. In the century that follows we need not study the general course of experiment, for most of it was unrelated to our subject; as we shall see also, many splendid theoretical discoveries of EULER remained long unheeded by the experimenters.

III. *The principles of mechanics.* In *static* problems both the BERNOULLIS in time came to isolate a differential element and balance the forces acting upon it. The deeper work of JAMES BERNOULLI was available, if in unpalatable form, in HERMANN's book. In *dynamic* problems it was a different matter, for in those days each savant treated motion after his own fashion. A variety of mechanical principles, each correct in some range between the very special, such as GALILEO's laws for an inclined plane, to the rather general, such as the LEIBNIZ-BERNOULLI principle of live forces, was known, and recognition of the central importance of the momentum principle was to come only at the hands of EULER some fifty years later. Here the modern reader, accustomed to obtain equations of motion by what is called "NEWTON's second law," will expect the English to have the advantage. But in our subject the English, aside from the unfortunate GREGORY, kept silent until TAYLOR's work began to appear, and indeed TAYLOR's formula (74) results from *the sole application of the momentum principle to a continuous body* so far; however, TAYLOR failed to write or use it as a differential equation. Thus, it seems,

a. None of the continental geometers used the momentum principle, at least in connection with continuous bodies.

b. The English (aside from NEWTON himself, who kept aloof from our subject) lacked

lead us to presume. No. 1, as we have seen, is the cumulative result of experience, guesswork, plausible reasoning, and experiment, definitively tested at last, though only in the more special form (8), by MERSENNE.

the mathematics sufficient to carry through an analysis based on the momentum principle.

IV. *Mathematics.* The last observation is essential. For today it is instantly plain that *the language of our subject is partial differential equations.* Now the English with their fluxions and the Continental geometers with their differentials were, in principle, on a par so long as problems involved but one independent variable. But no problem of the dynamics of continua, apart from those artificially simplified by extra assumptions, is of this kind. The *idea* of a partial derivative was indeed known, known in the same sense that the ideas of a tangent and area of an arbitrary curve were known and used correctly long prior to the differential and integral calculus. Lacking was a formal *calculus of partial derivatives,* and for developing such a calculus the fluxional concepts, while indeed admissible, were not conducive. What was needed was a man who could *express and master the* NEWTONIAN *view of mechanics in* LEIBNIZIAN *partial differentials.* This man was EULER. As we shall see, with some deviations this program occupied much of his effort for much of his life, and for the next twenty-five years our history records a part of his gradual progress toward achieving it.

Part II. The Beginnings of the General Theories, 1727—1748

20. Euler's derivation of James Bernoulli's law of bending from Hooke's law of extension; introduction of the modulus of extension (1727). At the time when John Bernoulli was calculating the [fundamental] frequency of the loaded string (*cf.* § 17, above), his student Euler was studying problems of vibration under his guidance. [It may be comforting to learn that even Euler must have found these problems difficult when a student, for his earliest attempts are faulty[1]). By 1726, however, he had a derivation[2]) of Taylor's formula (75), which he had confirmed by experiment[3]).

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1) Through the kindness of Professor Mikhailov I have seen a copy of a note of 1725—1727 in which Euler reports a value of $\nu_1^{(1)}$ which is greater than the correct value in the ratio $\sqrt{14/11}$ (*i. e.* $2/\sqrt{\pi}$); on this note John Bernoulli wrote the correct value $(77)_1$.

Further information may be obtained from the unpublished notebooks of Euler. These were hastily catalogued by Eneström, Jahresber. Deutsch. Math.-Ver. **22** (Ergänzungsband), 191—205 (1910), who assigned to them the numbers (E) H1—H9. Slight inspection of these notebooks shows that Eneström's dates are not always correct; my conjectured dates are as follows:

E H1	1726—7 (completely from Basel)
E H2	1727
E H3	1736—1740
E H4	1740—1744
E H5	1745—1750
E H6	1750—1757
E H8	1759—1760
E H7	1760 or 1761—1763
E H9	Miscellaneous

These notebooks, despite their length of some 3000 pages, contain little that Euler did not ultimately publish, usually in extended form, and the subjects of many of Euler's printed works are not mentioned in them at all. It is rarely possible to use them to date discoveries; rather, the dates of entries in them usually must be bounded by dates from letters or published papers.

I conjecture that the pages of E H6, E H7, and E H8 are not now in their original order, though most of the contents of E H8 was surely written before E H7. The material on pp. 72—75 of E H8 can be bracketed with certainty between Euler's letter to Lagrange of 23 October 1759 and the presentation date, 13 December 1759, of E 307. Also p. 84 and pp. 86—87 contain the first treatment of material in E303 and E302, respectively, presented in Berlin on 25 September 1760 and 22 January 1761 and listed on p. 184 of E H7 as among the memoirs sent to Petersburg on 26 April 1762.

A partial description of the notebooks, with similar conjectured dates, is given by Г. К. Михайлов «*Записные книжки Леонарда Эйлера в архиве АН СССР*,» Истор.-мат. исслед. Былуск **10**, 67—94 (1957); G. K. Mikhailov, "*On Leonhard Euler's unpublished notes and manuscripts on mechanics*," Proc. 3rd. congr. theor. appl. mech. Bangalore, 19–24 (1957); Г. К. Михайлов & В. И. Смирнов, "Неопубликованные материалы Леонарда Эйлера в архиве Академии Наук СССР," Леонард Эйлер, сборник статей в честь 250—летия со дня рождения, Москва, Издат. акад. Наук СССР, 1958, pp. 47—79. G. K. Mikhailov, "*Notizen über die unveröffentlichten Manuskripte von Leonhard Euler*," Sammelband zu Ehren des 250. Geburtstages Leonhard Eulers, Berlin, 1959, pp. 256—280.

On pp. 133—136 of EH1 is a first attempt on the vibrating string, by an obscure method leading to

Before he left Basel, EULER had written a paper, *On the oscillation of elastic rings*[1]), [which, while failing to solve the difficult problem to which the title refers, nevertheless obtains a major result in the theory of elasticity: *derivation of JAMES BERNOULLI's law of bending from HOOKE's law for the extension of the fibres.* Although this reaches the synthesis toward which JAMES BERNOULLI had struggled in vain, EULER does not appear to recognize what he has done. The paper was not printed until sixty years after EULER's death; the derivation it contains he published first in another paper on the same subject, written more than thirty years later (below, pp. 320, 388). For all these intervening years the two problems, this one and the one stated in the title, are to *remain untouched* by anyone else.]

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no result. A false frequency for the continuous string is obtained on pp. 137—139; for the string loaded by one mass, on pp. 139—140. On p. 146 is another false frequency for the continuous string.

2) This is asserted in § 20 of E2, *Dissertatio physica de sono,* Basel (1727) = Opera omnia III 1, 182—196.

Toward the end of 1726 EULER describes to DANIEL BERNOULLI the contents of *De sono* and mentions that he has calculated the speed of sound. On 24 December 1726 DANIEL BERNOULLI replies, "Not without pleasure have I learned that you intend to write a dissertation on *sound*; thus you will show very well how necessary is the joining of physics to higher mathematics. I doubt that anything definite can be said regarding the speed of sound, since there is as yet no right explanation of the propagation of sound. It is not the same with the number of vibrations executed by a taut musical sinew in a definite time, which I have no doubt at all may be determined from the laws of mechanics. I conjecture that our theories on this acoustic problem do not differ at all, since after the most exact experiments I have found, as you do, that a string which gives out the lowest C or ut executes 139 vibrations per second . . . I do not know whether you have seen the considerations of TAYLOR, a most acute English geometer," who by "a great and abstruse argument" has derived (75). In our special case this formula gives a frequency of 145/sec., "which differs much from ours." This seems to imply that DANIEL BERNOULLI's first attempt to calculate the frequency of a vibrating string led to a result other than (75) and hence was erroneous. He then refers to the second of SAUVEUR's methods, which "demands too much exactness in observation, transcending human strength;" as for the first, "I disapprove of it altogether. I will explain my method to you in person . . . Regarding the drum, I say nothing but that it admits the same solution as the musical sinew."

The sources of the correspondence between EULER and the BERNOULLIS are cited below, p. 165.

3) In § 21 of E8 EULER reports that he determined experimentally that the frequency of the note called $\bar{c}$ is 466/sec.; in §§ 10 and 13 of E33 (cited below, p. 154) he reports a different experiment yielding a = 392/sec. and $\bar{c}$ = 472/sec.; in comparing these results with modern values, recall that from surviving organs it is known that baroque pitches were at least a full tone lower than modern concert pitch. CHLADNI, commenting on the second of EULER's determinations of pitch just cited, writes that in the years between 1731 and 1802 the accepted pitch had already risen by more than a semi-tone (*cf.* § 57 of *op. cit. infra,* footnote 2, p. 329).

1) E831, "*De oscillationibus annulorum elasticorum,*" Opera postuma 2, 129—131 (1862) = Opera omnia II 11, 378—382. The crude treatment of simple harmonic motion in § 9 suggests a very early date; the methods and terms are unconnected to those in EULER's published papers. Dr. MIKHAILOV has kindly examined the original and has written me that the handwriting and paper indubitably confirm my conjecture that this work was written in EULER's Basel period.

1 The purpose is to calculate the period of oscillation of an elastic ring and hence to "lay the foundations for determining the oscillations of bells and other bodies."

2 A circular ring is supposed to oscillate slightly, assuming an oval form (Figure 50), [but in fact the following analysis is local and hence not restricted to the case

3 of a circular ring]. "The ring . . ., like a struck cord, will try to restore itself, and in such a way that every particle is drawn toward its natural position by a force proportional to its distance therefrom . . . If a particle is that much farther from the circle, by that much more will it be drawn back to it." [This last is a false start; as we shall see now, EULER in fact applies HOOKE's law correctly to the *extension* of the filaments.]

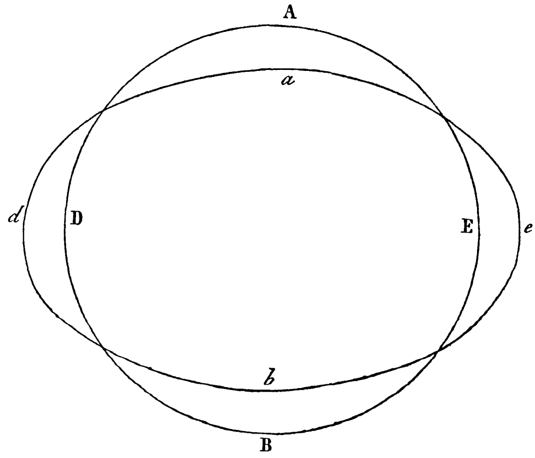

Figure 50.
EULER's diagram for an oscillating ring (1727)
(redrawn for publication in 1862)

4 On the left-hand side of Figure 51 we see the ring before it vibrates; the radius of curvature Ca is R, the thickness Aa is c. On the right, the ring is in vibration, and the element ab of length ds is selected so as to be equal to its counterpart on the left. The new radius of curvature ac is r, and thus the increment $E\epsilon$ in length of the element AB is given by

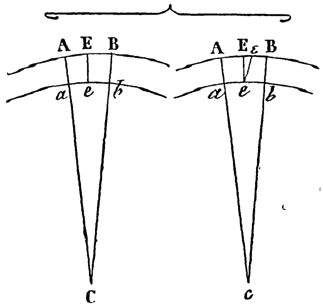

Figure 51. Diagram for EULER's first derivation of BERNOULLI's law from HOOKE's law (1727) (redrawn for publication in 1862)

$$d\tau = E\epsilon = \frac{R-r}{Rr}\,c\,ds = \left(\frac{1}{r} - \frac{1}{R}\right)c\,ds\,. \tag{83}$$

5 " . . . I suppose the joined particles $AaeE$ and $BbeE$ to be elastic filaments such that the more they are stretched, the greater force they have for contracting themselves. Therefore the angle $Ee\epsilon$ is full of such elements transversely disposed; these try to join the sides Ee and ϵe, and from the force of these threads depends the cohesion of the parts of the matter of which the ring is made. Let this cohesion . . . be such that the series of filaments $FG = f$ extended to $FJ = g$ may sustain the weight P (Figure 52)." [That is, $\dfrac{P}{fg}$ is the force per unit length of cross-section and per unit length

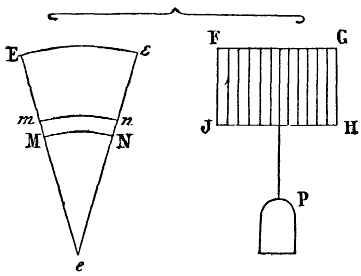

Figure 52. EULER's first definition of the modulus of extension ("YOUNG's modulus") (1727) (redrawn for publication in 1862)

of extension, or what is now called "YOUNG's modulus", *which makes its first appearance here*[1]).] Call $eM = x$, $Mm = dx$. The little space $MmnN$ will be full of filaments of length 6
$\frac{x}{c}d\tau$. The force F_x pulling Mn toward Nn is given by

(84) $$F_x = \frac{P}{fg}\cdot dx \cdot \frac{x}{c}d\tau \ .$$

"Therefore the weight to be applied at E and ϵ so as to constrain the sides Ee and ϵe with the same force" will be

(85) $$F = \frac{x}{c}F_x = \frac{P}{fgc^2}x^2 dx d\tau \ .$$

[Thus we see that EULER is using "pondus", here translated "weight", to mean "force", while he uses "vis", here translated "force", more generally to mean "effect". He has calculated the force F to be applied at E in order to exert *the same moment* about e as does F_x acting at x.] Integration form $x = 0$ to $x = c$ yields the total "weight" to be applied at E:

(86) $$\text{weight} = \frac{Pc}{3fg}d\tau = \tfrac{1}{3}\cdot\frac{Pc^2}{fg}\cdot\left(\frac{1}{r}-\frac{1}{R}\right)ds \ .$$

[This result we recognize as

(87) $$\mathcal{M} = \mathcal{B}\left(\frac{1}{r}-\frac{1}{R}\right), \quad \mathcal{B} = EI,$$

specialized to a rectangular cross-section when the neutral line is taken as the fibre on the concave side, so that $I = \frac{1}{3}AD^2$, and E is "YOUNG's modulus". Thus not only is the LEIBNIZ-VARIGNON formula (61) successfully combined, at last, with JAMES BERNOULLI's formula (45), but also the formula (69) for initially curved rods is included.]

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1) In previous theories we have encountered constants or coefficients of two types:

A. In GALILEO's theory, and in the work of LEIBNIZ, VARIGNON, and PARENT, there is a material constant K which is interpreted as the *rupture stress*; *cf.* (12). These theories in effect consider elastic stress but neglect the elastic strain to which it gives rise.

B. In HOOKE's theory, as refined by later authors, there is an *elastic force* defined as that force which produces a specified strain in a given body.

What is new here is that EULER's $P/(fg)$ is a *material elastic constant*, i. e., *a mean stress* (not a force) *which produces a specified elastic strain* (not rupture).

The existence of such a material constant is clearly implied by the more general considerations of JAMES BERNOULLI (above, p. 106), but it was not introduced explicitly by him. Its explicit appearance in this work of EULER is easily explained: This is the first problem requiring such a modulus for a proper solution.

For the later history of "YOUNG's modulus", see below, pp. 402—404.

7 EULER now wishes to calculate the force accelerating a given element toward the center of the circle. He regards this as the product of (86) by the difference of the lengths $E\epsilon$ at different times during the motion, calculated at the end of the major axis of the ellipse into which the circle is deformed. He then assumes simple harmonic motion and by
8—9 elimination of the distance calculates the period. [The analysis is difficult to follow,] and
11 EULER at first rejects the result. By rearrangement of constants he concludes that for a ring of radius R and thickness D the frequency is given by

$$\nu = \frac{D}{2\pi R^2}\sqrt{\frac{E}{\varrho}}\,. \tag{88}$$

10—11 He is dismayed at the conclusions that the period is independent of the altitude of the ring and that the pitch grows higher the smaller is the radius R. [Doubtless these prevented him from publishing the paper; however, the general nature of the relation (88) is correct, although for an inextensible ring EULER's neglect of longitudinal motion is not justified (*cf.* below, p. 320).]

A marginal note asserts that in order for a bell as a whole to give out the same sound, it is necessary that $R^2/D =$ const. [Thus EULER at this early period thinks of a bell as composed of a pile of circular rings vibrating independently. This incorrect idea he is to exploit later (below, pp. 321).]

21. EULER's unification of the catenary and the elastica (1728). We now find in St. Petersburg the two savants who will dominate our subject, nay, monopolize it, for twenty years and more: DANIEL BERNOULLI and EULER, one being the son and both being the pupils of JOHN BERNOULLI, both junior members of the academy, where in friendly competition they discuss and solve the same problems. DANIEL BERNOULLI, twenty-eight years old, is already a famous scientist, while EULER at twenty-one has published but three papers. In February 1728 each communicates a unified theory of flexible or elastic lines; their papers appear consecutively in the volume containing also JOHN BERNOULLI's proofs on the vibrating string, described in § 18.

DANIEL BERNOULLI's note is called *Universal method for determining the curvature of a thread stretched by powers following any law among themselves, along with a solution of certain related new problems*[1]). The first half of the paper concerns perfectly flexible lines
10 [and contains nothing new[2])]. "To find the curvature of an elastic band curved partly by its

1) "*Methodus universalis determinandae curvaturae fili a potentiis quamcunque legem inter se observantibus extensi, una cum solutione problematum quorundam novorum eo pertinentium,*" Comm. acad. sci. Petrop. **3** (1728), 62—69 (1732).

2) DANIEL BERNOULLI implies that up to this time only loading normal to the curve or parallel to a fixed direction had been considered. That he was ignorant of his uncle's unpublished work is to be

own weight and partly by an attached weight," Daniel Bernoulli assumes the band to be "of the same structure throughout its length, although the problem does not become much more difficult if it is of non-uniform structure." The weight of the band acts at its center of gravity; hence its moment about s is $g\sigma s(x - x_c) = g\sigma s\left(x - \frac{1}{s}\int_0^s x\,ds\right) = g\sigma\int_0^s s\,dx$. The moment of the attached weight P is Px. The total moment is related to the curvature by the formula

$$\mathcal{M} = \frac{\mathcal{B}}{r}\,, \quad \mathcal{B} = \text{"modulus of bending" or "flexural rigidity".} \tag{89}$$

[We have seen that an equivalent result was obtained by James Bernoulli, but, always emphasizing the tension acting on the cross-section, he never stated the law of the elastica in this way. The more general and more explicit result (87), derived in a major special case by Euler, was unpublished. In Daniel Bernoulli's paper is the first explicit recognition of (89) as the basic law of the elastica, although to derive it he does no more than restate it[1]). Here also we find the first explicit appearance of $\mathcal{B}$, which Euler is soon to call "the absolute elasticity". Thus (89), or its generalization (87), may justly be called the *Bernoulli-Euler formula* for the bending of a beam, it being understood that reference is made both to James and to Daniel Bernoulli.] Hence

$$g\sigma\int_0^s s\,dx + Px = \frac{\mathcal{B}}{r}\,. \tag{90}$$

Next Daniel Bernoulli considers the problem from the Acta Eruditorum of 1724 12
(above, p. 138), for which, he says, no solution has been published. We need only consider the

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expected, since James Bernoulli's papers were kept from John Bernoulli and his circle; that Daniel Bernoulli should not know the general solution in the book by his senior colleague, Hermann (above, p. 86), is surprising, especially since Euler refers to it (below, p. 149).

It is typical of Daniel Bernoulli that he stays close to the simplest special cases by resolving a general load into a normal component F_n and another, F_y, parallel to a fixed direction, thus losing the advantages both of intrinsic and of fixed co-ordinates (§ 1). First he laboriously balances such forces acting upon a chain of three links (§§ 2—3), then passes to the limit as the junctions approach one another (§ 4). The result of all this we may derive at once from (40) and (42) if we observe that the normal load is $F_n - F_y\dfrac{dx}{ds}$, the tangential load is $F_y\dfrac{dy}{ds}$, and then eliminate T.

Daniel Bernoulli's examples include a generalized lintearia in which both the weight of the fluid and the weight of the curve are considered (§§ 5—7); neglecting the latter leads to the ordinary lintearia, "first studied by my uncle James Bernoulli" (§ 8); neglecting the former, to the ordinary catenary, "first proposed to the geometers by my father" (§ 9).

1) Recall that for James Bernoulli it was not a postulate but rather a result he attempted to derive. For Daniel Bernoulli, as with most principles he considered true, it seems to be self-evident and scarcely worthy of comment.

weight P as acting at an arbitrary angle to the band; resolving the weight into two com-
13 ponents P_x, P_y, we replace the term Px in (90) by $-P_y x + P_x y$. If the density σ is non-uniform, $\sigma \int_0^s s\,dx$ is to be replaced by $\int_0^s \Sigma\,dx$, where $\Sigma = \int_0^s \sigma\,ds$. The paper closes with the remark that "our most enlightened EULER solved this problem, proposed to him by me, in such a way that it would not seem possible to add anything."

EULER's *Solution of the problem of finding the curve assumed by an arbitrarily elastic band loaded by arbitrary forces at its several points*[1]) begins right off with a "general problem[2])" including all previously
P. 3 studied cases of the equilibrium of a string or rod: *To find the differential equation of an arbitrarily elastic band fixed at one end B and loaded by arbitrary forces along its length and by an arbitrary force at its other end A*
2 (Figure 53). The "Hypothesis" is: "If two rods (Figure 54) aB, BC joined at B by a spring are twisted by the power AD into the con-

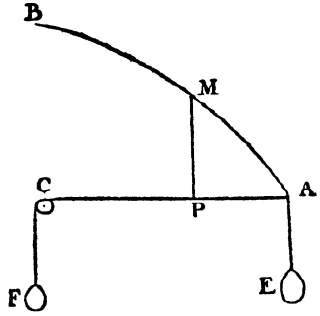

Figure 53.
EULER's diagram for the elastic band subject to arbitrary forces

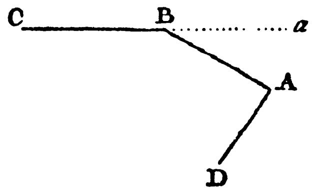

Figure 54.
EULER's diagram for statement of the BERNOULLI hypothesis

1) E8, "*Solutio problematis de invenienda curva quam format lamina utcunque elastica in singulis punctis a potentiis quibuscunque sollicitata,*" Comm. acad. sci. Petrop. 3 (1728), 70—84 (1732) = Opera omnia II 10, 1—16. Presentation dates: February 1728, 22 December 1730.

Through the kindness of Professor SPIESS I have seen a copy of a manuscript by EULER which seems to be a preliminary version of E8 and must surely date from the period of his studies in Basel. This is Mscr. C2 of the Basel University Library, "*De figuris quas corpora flexibilia debent induere a potentiis quibuscunque sollicitata.*" Two marginal notes by JOHN BERNOULLI describe his solution for the catenary subject to forces directed toward a fixed center (above, p. 86). Marginal notes in another hand point out errors, which EULER corrects in an appendix. (While the inscription on the envelope, written by JOHN III BERNOULLI, states that these notes are by DANIEL BERNOULLI, the circumstances make this attribution unlikely.)

This paper, carried through with a kind of mathematics considerably more primitive than that in E831, begins by considering various kinds of perfectly flexible lines. Its most interesting feature is the faulty supposition that a plane curve which is the figure of equilibrium subject to a certain plane load will serve as generator of a surface of revolution forming the figure of equilibrium for analogous spatial loading. *E. g.*, the lintearial surface is a sphere. The error is pointed out in the marginal notes. An attempt at a direct treatment of some spatial problems is given in the appendix, to which there is no counterpart in the printed paper E8.

Elastic problems begin only at Proposition 15, which is the "Hypothesis" put at the beginning of E8. Here EULER treats only the case of terminal load and obtains only the rectangular elastica.

The *unification* that is the dominant feature of E8 is completely lacking in this early study.

2) On p. 15 EULER mentions the special case proposed in the Acta Eruditorum for 1724 (above, p. 138), "the solution of which no one, so far as I know, has obtained up to now, except for the most enlightened DANIEL BERNOULLI, who achieved the solution about the same time as I did."

figuration ABC, so as to subtend an angle ABA, the moment of the power AD at B will be jointly as the elastic force at B and the angle ABa. This hypothesis is commonly assumed; probably its truth can be proved physically when the angle is very small." In
what follows, EULER interprets this hypothesis as implying (89). EULER's mechanical 4
principle, like BERNOULLI's, is the equilibrium of moments about an arbitrary point on the band; thus the solution of the "general problem" [for a band that is either perfectly flexible ($\mathcal{B} = 0$) or naturally straight] is

$$(91)\qquad \begin{gathered} -P_y x + P_x y - \int_0^x Y\,dx + \int_0^y X\,dy = -\frac{\mathcal{B}}{r}\,, \\ Y = \int_0^s F_y\,ds\,,\quad X = \int_0^s F_x\,ds\,, \end{gathered}$$

the origin being taken at the end where the loads P_x, P_y are applied. For the coefficient $\mathcal{B}$ EULER uses the notation Av, where A is a constant of proportionality and v is the "elasticity", not necessarily constant.

The perfectly flexible case is obtained by setting $\mathcal{B} = 0$. By differentiating (91) 4
twice, EULER obtains a differential equation for the curvature; later he obtains the same 10
result expressed in terms of normal and tangential loads[1]: 11

$$(92)\qquad \frac{d(F_n r)}{ds} + F_t = 0\,.$$

As EULER remarks, only HERMANN had published anything so general (above, p. 86).
If $F_x = 0$, EULER's first form of (92) yields 6—7

$$(93)\qquad rF_y\left(\frac{dx}{ds}\right)^2 = \text{const.}\,;$$

when $F_y = \text{const.}$, the ordinary catenary results, and when $F\frac{dx}{ds} = \text{const.}$, the parabola. For purely normal load, as in the cases of the velaria and the lintearia, (92) yields $F_n r = \text{const.}$ This exhausts the familiar types of flexible lines.

While EULER considers some other cases, he cannot effect the integrations. Some space is given to differential manipulations showing that a given special case may be obtained either from the rectangular Cartesian form or from the intrinsic form of the general equation.

[Comparison of these simultaneous works of EULER and DANIEL BERNOULLI reveals a course typical of what will follow. DANIEL BERNOULLI suggests the problem and is perhaps the first to solve it; his paper reproduces what were doubtless the labors of discovery, groping from one special case to the next, and ends just before achieving the

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1) To derive this directly, eliminate T from (40) and (42).

goal. Just at the degree of generality where one must abandon the device of concentrating the distributed force at the center of gravity, DANIEL BERNOULLI abandons the problem. EULER sweeps all this aside and in his finished paper starts at the point where BERNOULLI left off. DANIEL BERNOULLI's mathematics is at least as clumsy as that in the old papers on calculus of forty years previous, and clumsier than his father's at this same date; EULER's is secure and fluent. DANIEL BERNOULLI puts in print the preliminary trials which ought to have been left in the notebook; now, as for the rest of his life, he cannot *revise*. Indeed, the revision and polishing that the savants of the previous century carried to extremes is to be almost wholly abandoned in the prolific eighteenth century. In the present case, however, EULER presents a finished and elegant treatise[1]); not only does he unify the doctrine of elastic or flexible bodies[2]) to the extent it had been cultivated up to that time, but also he is the first to publish an adequate exposition of the known special cases[3]). In this, EULER's first published paper in our subject, shine forth the *clarity, order*, and *scope* which beautify nearly all his writings. It is also typical of EULER's work in mechanics, in contrast not only to DANIEL BERNOULLI's but also to most of that we have discussed up to now, that he does not bury or glide over the basic principles but brings out (89) explicitly as a *postulate*.

On (91) EULER is to found all his researches in this field for the next twenty years[4]). While at the time the choice of the *equilibrium of moments* rather than of the equilibrium of forces must have seemed the only way to include problems of bending, we see now that it was an unfortunate one, for this method is little suited to further generalization. In particular, a proper theory of *motion* of elastic bodies does not follow naturally from the consideration of moments.]

22. MUSSCHENBROEK's experimental discovery of the law of buckling in compression (1729). The second quarter of the eighteenth century produced not only the first attempts at a fairly general elastic theory but also the *first systematic and successful program of experiment* on the strength of materials, which is reported in MUSSCHENBROEK's *Intro-*

1) The second presentation date nearly three years after the first suggests that EULER may have withdrawn his first attempt and replaced it by a maturer work.

2) At this time EULER uses "lamina", "filum", and "corpus" as virtually equivalent.

3) *Cf.* our remarks above on the treatments of TAYLOR, HERMANN (pp. 86—87), and DANIEL BERNOULLI (p. 147).

4) This paper completed the general theory of plane flexible lines, though publications concerning them continued to appear for another century. Here we mention only the exposition in §§ 561—570, 889—890 of MACLAURIN's *A Treatise of Fluxions*, Edinburgh, Ruddimans, 1742; the elegant and concise treatment of J.-B. CLAIRAUT, "*Methodus generalis inveniendi catenarias*," Miscell. Berol. 7, 270—272 (1743); and the merely derivative work of KRAFFT, "*De curvis funiculariis et catenariis, vel illis, quae corporibus flexibilibus inducuntur, cum a potentiis quibusvis solicitantur*," Novi comm. acad. sci. Petrop. 5 (1754/5), 145—163 (1760).

duction to the coherence of solid bodies[1]). This work deserves the high esteem given to it by the writers of the eighteenth century, who refer to it as the standard collection of experimental data. To MUSSCHENBROEK is due the invention of special *testing machines*[2]) permitting systematic variations of experimental parameters in an easy succession of measurements. Unfortunately, all his conclusive experiments refer only to *breaking;* elastic deformation is described, but no definite laws are found for it. MUSSCHENBROEK is a scholarly reader of all earlier work, including mathematical theories he does not understand fully; in contrast to his lucid descriptions of experiments, he makes his treatise harder to read by inserting stretches of tedious geometrical proofs in the style of GALILEO's school[3]).

MUSSCHENBROEK begins by experiments on extension. He infers GALILEO's formula (12) by reasoning and thus considers it sufficient, in cases where weight is neglected, to measure the rupture force P_t. In a sequence of 47 experiments on variously shaped prisms of various woods, he measures not only P_t but also the elongation and the transverse contraction prior to rupture; [unfortunately he does not infer any elastic law]. In each case he describes and illustrates the surface of rupture. He notices that fracture sometimes occurs gradually, as if one fibre after another were breaking. The occasional inconsistency of the results he attributes to the irregularity of wood structure. pp. 466—474 481—494

Coming to work with metal wires, he begins to doubt (12) and decides to test it by 494—506

1) "*Introductio ad cohaerentiam corporum firmorum,*" pp. 421—472 of *Physicae experimentalis, et geometricae, . . . dissertationes*, Lugduni Batavorum, Luchtmans, 1729, [x] + 685 pp.

There is an earlier work, *Epitome elementorum physico-mathematicorum conscripta in usus academicos*. Lugduni Batavorum, Lugtmans, 1726. This seems to be derivative, giving no experimental results of interest, but in §§ 380—395 are clear physical definitions of the terms "hard", "perfectly hard" (*i. e.*, rigid), "soft", "perfectly soft", "flexible", "elastic", and "perfectly elastic".

There is also a later work which includes summaries of some parts of the great treatise we describe above. This is *Elementa physica conscripta in usus academicos*, Lugduni Batavorum, Luchtmans, 1734; see §§ 396—400. In §§ 322—329 we find the definitions mentioned above, and also the statement that heating a body always renders it less elastic. MUSSCHENBROEK writes that experiments of BOYLE, HAUKSBEE, DERHAM, and others show that a body has the same elasticity in a vacuum as in open air.

2) For tensile test of glass rods, Figs. 8 and 9 of Tab. XVII; for the tensile test of wooden beams, Fig. 6 of Tab. XIX; for the tensile test of metal wires, Figs. 2, 3 of Tab. XX; for the bending test of wooden beams supported or clamped at the ends and loaded in the middle, Fig. 36 of Tab. XXIII; for the compression test of wooden struts, Fig. 16 of Tab. XXVII; for a test of hardness, Fig. 3 of Tab. XXVIII.

Earlier authors had performed much the same tests but with little or no precaution or plan. *E. g.*, on p. 480 MUSSCHENBROEK writes of MARIOTTE's tensile test, "In this method I noticed the inconvenience that the feet of him who performs the experiment are always exposed to danger of injury when the weight falls."

A special machine had been designed and built by 's GRAVESANDE for the faulty test mentioned in footnote 1, p. 117.

3) Especially pp. 467—479, 552—610, 625—639.

experiment, although this "will seem superfluous to the geometers." He prepares the wires by drawing to a certain diameter, then softening them by heating. A series of 34 tests on wires of copper, brass, gold, lead, tin, silver, or iron generally *fails* to yield $P_t/A =$ const., whether A is taken as the area of the wire before it is tested, after it is broken, or at the surface of rupture. His results do not always agree with MERSENNE's (above, p. 32). That P_t does not depend on the length of the specimen MUSSCHENBROEK regards as incontrovertible, though he does not report experiments testing it.

506 "I observed in all these experiments that the wires . . . took on considerable heat when they were elongated . . . and broken; this heat arises from the rubbing of the parts moved upon one another and strongly pressed while the metal is thinned . . ." [Thus MUSSCHENBROEK is the first[1]) to write that doing work upon a deformable body heats it, and he attributes this heating to *internal friction*.]

525—534 MUSSCHENBROEK attempts to infer from theory that the numerical factor in (11) may have any value not exceeding $\frac{1}{2}$, depending upon the law of tension; thus *the factor must be determined by experiment*. [While the former conclusion is true, MUSSCHENBROEK's reason-
535—537 ing is faulty[2]).] Experiments on 50 circular or rectangular prisms of various woods yield numerical factors between $1/2\frac{1}{2}$ and $1/18$. "These experiments bring out more clearly into the daylight the fact that neither the rule of GALILEO, nor MARIOTTE's, nor any other, is universal . . ." MUSSCHENBROEK observes also that the numerical factor is almost always less for a circle than for a square [*cf.* the work of PARENT, above, pp. 111—112)].

535 MUSSCHENBROEK makes some attempt to test JAMES BERNOULLI's theory of the elastic curve by experiment, but he finds that wooden beams continue to deform under load. "If it is permissible to present so crude an observation . . . I say . . . that there are as many different curves formed by attaching a weight as there are different woods that I have investigated." Also, the variety of woods obtainable from the same trunk make it uncertain whether the strength determined by breaking one specimen was applicable in interpreting the defor-
540—541 mation of the next. [Strangely, however, instead of carrying out the measurement of deflection for metal bars,] MUSSCHENBROEK complains that he cannot study the breaking of metal bars in bending because they are too flexible.

541—548, 610—625, 639—650 There follows a long series of tests of GALILEO's proportion $(13)_2$, not only for beams subject to terminal load but also for beams supported or clamped at both ends and loaded at their middles. The lever arm of the weight at the instant of rupture is recorded, but the

1) Long before RUMFORD.

2) Pp. 532—534. MUSSCHENBROEK here employs a linear law, varying only the slope and the position of the neutral fibre. Perhaps he is trying to follow PARENT (above, p. 113), but he does not seem to understand the problem, as he does not consider the contribution of the compressed parts at all, nor does he apply PARENT's condition that the area under the curve of tensions equal that under the curve of pressures.

vertical deflection of the beam is not. There are hundreds of measurements, terminating in a systematic program on beams of oak or pine 10″ or 11″ broad, 10″ to 15″ deep in steps of 1″, 6′ to 40′ long in steps of 2′. In general, the agreement is not satisfactory; MUSSCHENBROEK finds a dependence on depth as D^a, where the exponent a is less than 2 and varies from one wood to another. Only for glass is GALILEO's proportion $(13)_2$ verified. MUSSCHENBROEK finds that clamped beams are many times stronger than supported beams, but he is unable to infer a specific law.

The climax of MUSSCHENBROEK's work is the series of experiments numbered 222—248, 654—662
"the first in this doctrine of the firmness of compressed bodies," which are summarized in the criterion for *failure in compression* stated in Proposition 119: "Parallelepipeds of the same wood ..., compressed along their lengths, exert forces of resistance which vary inversely as the square of the length, directly as the thickness of the side that is not bent, and directly as the square of the side that is bent." *I. e.*,

$$P_c \propto \frac{D^2 B}{l^2} . \tag{94}$$

[Thus the law of failure in compression is *entirely different* from that for failure in tension.] MUSSCHENBROEK's conceptual explanation regards the strut as compressed elastically, 652—653
though of course in ratio less than that of the compressing weight[1]), until the internal pressures transmitted through the irregular, porous structure of the wood result in *bending;* when the compression is increased, the strut "breaks in the middle, where it is bent the most," and this latter assertion is verified in the experiments. [Thus MUSSCHENBROEK is the first to distinguish *buckling* from breaking; *cf.* the remarks of HERON and of LEONARDO DA VINCI (above, pp. 18, 20). Furthermore, he is the first to discover by experiment any non-trivial relation in the strength of materials[2]). It seems unlikely, however, that the dependence on B and D given by (94) can be correct[3]); the striking dependence on l is now classical.

To derive from theory a formula of this type will be a major achievement of EULER in 1742 (below, p. 211)[4]).

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1) The reason given is that of JAMES BERNOULLI, above, p. 106.

2) GALILEO's proportions seem to have been inferred from conjecture rather than experiment; HOOKE's law is merely linear and follows as a first approximation from most theories. The proportion (94) represents a different order of achievement.

3) If (94) is true, there is a material constant of the dimensions [force]/[length]. On the other hand, if we assume the existence of an elastic modulus E, by dimensional analysis follows

$$P_c = \frac{E D^3 B}{l^3} f\left(\frac{D}{l}, \frac{B}{l}\right).$$

EULER's theory (below, p. 404) gives $f = \text{const.}$ (provided $\mathcal{B} = EI$).

4) MUSSCHENBROEK's treatise ends with a discussion of the bursting strength of pipes (pp.

23. Daniel Bernoulli's discovery of the simple modes and proper frequencies of vibrating systems (1733), and related work of Euler. Before describing the beautiful discoveries Daniel Bernoulli is soon to make concerning vibration, we mention some earlier work of Euler. The introductory material on acoustics given in his great essay on music[1]) seems to derive in large part from his reading and from his researches in Basel before 1727. Euler is the first author since Mersenne to assert rules for furnishing a collection of strings such as to give out equable sounds. First, the ratios $\frac{T}{A}$ of all strings should be the same. For a material of density ϱ we have $\varrho g A l = W =$ weight of the string $= g\sigma l$; hence if all strings are of the same material, we should adjust them so
18 that $\frac{T}{\sigma}$ has the same value. Since the loudness depends on the amplitude, and the amplitude depends on the place and the amount the string is struck initially, we should strike all strings at the same place. Then the loudness will depend only on the amount of striking force. The loudness of the sound transmitted in air depends upon the speed acquired by the particles of the air, and this is to be estimated from the maximum speed of the string, in its turn proportional to $\sqrt{T/l}$ [but this I do not follow, since for given amplitude the
19 maximum speed is proportional to ν and hence to $\frac{1}{l}\sqrt{\frac{T}{\sigma}}$]. Combined with the above, this yields $\frac{l}{\sigma} =$ const., or $\frac{l}{A} =$ const. That is, for equable sound we should have $A \propto l$ and $T \propto l$. By (10), the sounds will then be reciprocally proportional to the lengths. "This rule will have great use in the construction of musical instruments." [It is justly criticized by Daniel Bernoulli[2]).]

663—668) and an attempt to measure the hardness of wood by the amount of energy, supplied by impacts of a ball of given mass and speed striking the handle of a chisel, necessary in order to cut through a specimen of given size.

On pp. 508—524 he continues the experiments of de Réaumur (above, p. 58) and shows that twisting always notably weakens the total strength, but he cannot form a definite law. He finds to his surprise that the thinnest animal fibres have the greatest breaking stress; *e. g.*, the finest fibres of cocoon silk are stronger than spider silk, which in turn is stronger than human hair.

1) E 33, *Tentamen novae theorie musicae ex certissimis harmoniae principiis dilucide expositae*, Petropoli, 1739 = Opera omnia III 1, 197—427. The work was complete, or nearly so, in 1731.

The rules for equable sound are given on p. 158 of Notebook EH1 (cited above, p. 142), apparently written before the letter of 1726 to Daniel Bernoulli which we have quoted above, p. 143. It appears that Euler inferred these rules from experiment.

2) On 28 January 1741, just after he had first seen the work, he wrote to Euler "I have conjectured from some passages that you have not read Mersenne . . ., who has very curious experiments . . . I have wondered if for hearing it is not required that the tympanum be tuned to the sound perceived, which office the muscles can do with extraordinary speed and from which many phenomena may be deduced. On p. 10 it is said that the sound is most pleasant in strings as taut as possible. This question Mersenne treats [*cf.* above, p. 31] and gives only half this degree of tension for the sweetest

"We have said that the sound will be less pleasing if the string is not tense enough, 20
for then the travel in vibrating is too great and thence the air is moved rather as a wind than induced to execute oscillations . . . Also, as is known, the great vibrations are not isochronous with the lesser, so that the sound at first is lower and does not remain the same. Thence it easily happens that the whole string does not produce its oscillations all at once, but one part reaches its maximum speed and its point of rest faster, another slower, whence the sound is inequable and rough . . ."

For similar bells of like material, Euler repeats [Mersenne's] rule $\nu \propto 1/\sqrt[3]{W}$, 22
[equivalent to (9)].

For prismatic rods or bars, "the sounds seem to depend upon the length in this way, 23
that each fibre stretched along its length should be regarded as vibrating by itself." Thus $\nu \propto 1/l^2$, [but how Euler infers this law he does not disclose, nor does he make clear to which kinds of vibrations he regards it as applying[1]). No dependence on cross-sectional area or form is mentioned.] "Finally, the frequencies of prisms of different material depend not only on the specific gravity, but also he who would determine the sounds themselves from theory must know the rule of the cohesion and stretching of the material."

Euler says that "both from theory and from experience" we know that a string can 41
vibrate in halves, thirds, fourths, *etc.*, thus giving out its harmonics. [The experiments of Sauveur were well known, but no theory of any kind for the overtones was in print when this book was written. Perhaps Euler refers to still unpublished work of Daniel Bernoulli, which we now describe.]

Before leaving Petersburg in 1733, Daniel Bernoulli had communicated[2]) his *Theorems on the oscillations of bodies connected by a flexible thread and of a vertically suspended chain*[3]). The remarkable results in this paper establish him as the discoverer of the *simple modes and proper frequencies of an oscillating system.* He has observed the "very 1

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sound . . . From what he says it is clear that the greatest tension is the least pleasant, and I think too that the sound will be not at all constant in strings as taut as possible, since the elongations are not proportional to the stretching forces, while not far from rupture everything must be very irregular. That the breaking forces are proportional to the thicknesses of the strings is not confirmed by experience . . . Experience shows also that nearby a high sound is louder, while far away a low sound is louder."

1) For *longitudinal* vibrations, such as the foregoing text suggests, it is false, the correct law being $\nu \propto \frac{1}{l}\sqrt{\frac{E}{\varrho}}$. To prove that for transverse elastic oscillations of a bar the correct scaling law is $\nu \propto \frac{D}{l^2}\sqrt{\frac{E}{\varrho}}$ will be a great later achievement of Euler in several steps, beginning in 1735 (below, p. 169). Both these laws are consistent with Mersenne's law (9).

2) This is confirmed by Euler in § 3 of E49, cited below, p. 162.

3) "*Theoremata de oscillationibus corporum filo flexili connexorum et catenae verticaliter suspensae,*" Comm. acad. sci. Petrop. 6 (1732—1733), 108—122 (1740).

irregular" motions of a hanging chain; [since establishing the equations of motion for such a system seemed out of reach of methods then known,] he decides "to determine into what curve the chain should be bent so that all its particles, beginning to move at once, would simultaneously reach the vertical passing through the point of suspension : for I understood that in this way the oscillations would be equable and such as to have a definite period of oscillation . . . In the solution I have used new principles, and besides that I wished to confirm the theorems with experiments . . . We shall consider only very small isochronous oscillations, but for the experiments it is allowable to use somewhat greater ones without noticeable error."

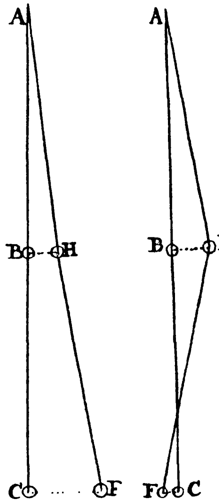

Figure 55.
DANIEL BERNOULLI's drawing of the two simple modes of vibration for a string loaded by two weights

2—4 Theorems 1 and 2 concern the weightless cord loaded by two equally spaced weights of equal mass and assert the existence of *two possible modes* of vibration, shown in Figure 55, with amplitudes and frequencies satisfying

$$(95) \qquad \frac{CF}{BH} = 1 \pm \sqrt{2}\,, \qquad \nu = \frac{1}{2\pi}\sqrt{\frac{g}{l}}\sqrt{4 \pm \sqrt{8}}\,,$$

5 where $l = AC$, the whole length. In the "collaborating" mode, corresponding to the upper signs, the oscillations are but slightly faster than for a simple pendulum of the same length, while
6 for the "contrary" mode they are very much faster. These
7 results are confirmed by experiments. Theorem 3 asserts for two weights at distances $AH = \alpha l$, $HF = \beta l$, $\alpha + \beta = 1$, and with masses γM and δM, where $\gamma + \delta = 1$, (95) is generalized by

$$(96) \qquad \begin{aligned} \frac{CF}{BH} &= \frac{\gamma(\alpha - \beta) + \delta \pm \sqrt{4\beta^2\gamma\delta + [\alpha + \beta(\gamma - \delta)]^2}}{2}\,, \\ \nu &= \frac{1}{2\pi}\sqrt{\frac{g}{l}}\sqrt{\frac{1 \mp \sqrt{4\beta^2\gamma\delta + [\alpha + \beta(\gamma - \delta)]^2}}{2\alpha\beta\gamma}}\,. \end{aligned}$$

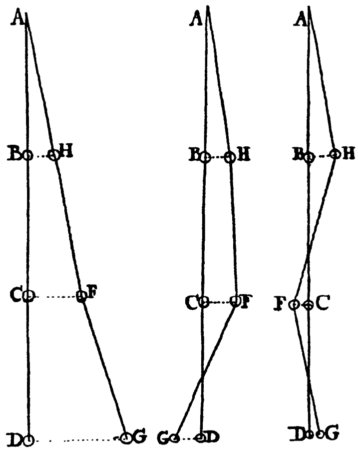

Figure 56.
DANIEL BERNOULLI's drawing of the three simple modes of vibration for a string loaded by three weights

8—9 Theorems 4 and 5 concern the case of three weights (Figure 56). Then if $x = \frac{CF}{BH}$, x may be taken as any one of the three roots of

$$(97) \qquad 4x^3 - 12x^2 + 3x + 8 = 0\,,$$

while $DG = 3x^2 - 2x - 2$. The frequencies are then

$$(98) \qquad \nu = \frac{1}{2\pi}\sqrt{\frac{g}{l}} \cdot \sqrt{3(5 - 2x)}\,.$$

The roots of (97) are calculated numerically. Theorems 6 and 7 generalize the results to 11, 13
the case of three weights of unequal magnitude at unequal distances. Among the corollaries 12, 14
we note only the particularly elegant one that follows by taking $CF = 0$ in the second
mode, thus yielding [HUYGENS' and] JOHN BERNOULLI's result $(77)_1$.

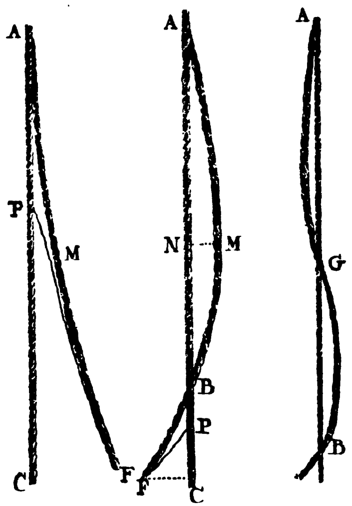

Figure 57.
DANIEL BERNOULLI's drawings of the first three simple modes of vibration for the continuous heavy cord

There follows a "general scholion": "I can give 15
similar equations for four, five, or as many bodies as desired: Always the equation rises to as many dimensions as there are bodies . . . this law appears from the method I have used."

Theorem 8 asserts that for a uniformly heavy 16
hanging cord of length l, in "uniform oscillations" (Figure 57) the displacement y at a distance x from the bottom is given by the series we should now denote by

$$(99) \qquad y = \mathfrak{A} J_0\left(2\sqrt{\frac{x}{\alpha}}\right),$$

where $\mathfrak{A} = CF$ and where α is so chosen that

$$(100) \qquad J_0\left(2\sqrt{\frac{l}{\alpha}}\right) = 0\,.$$

[This is the first appearance of "BESSEL functions".] By 17
a method for solving transcendental equations DANIEL BERNOULLI gave in the preceding
volume he calculates that the [largest] value of α is given by

$$(101) \qquad \frac{\alpha}{l} = 0{,}691 \quad \left[= \frac{1}{1{,}45}\right].$$

According to Theorem 9, the period is that of a simple pendulum of length α; *i. e.*, 18

$$(102) \qquad \nu = \frac{1}{2\pi}\sqrt{\frac{g}{\alpha}}\,.$$

Alternatively,

(103) $\qquad \alpha =$ the subtangent CP at the bottom,

as is immediate from the series for (99). Thus the chain oscillates more slowly than a 19
pendulum of the same length. An experiment performed on thread loaded by many 20
small equidistant leaden weights confirms (102) and (103).

Moreover, the equation (100) "has infinitely many real roots, and also the chain can 21
be bent in infinitely many ways so as to execute uniform vibrations; the value of α takes
on smaller and smaller values until it virtually vanishes. In all cases the length of the

isochronous pendulum is α, or the subtangent CP; thus the corresponding oscillations are almost infinitely rapid." The modes[1]) may be distinguished by the number of intersections with the vertical [*i. e.*, nodes]; in the first, there are none besides the point of support. In the second, there is one, in the third, two, *etc.* For the second mode BERNOULLI gives the following approximations:

$$\frac{\alpha}{l} = 0{,}13 \quad \left[= \frac{1}{6{,}1}\right], \quad \frac{CB}{l} = 0{,}19\,, \quad \frac{CN}{l} = 0{,}47\,, \quad \frac{MN}{FC} = \frac{2}{5}\,. \tag{104}$$

"The arcs cut off between two neighboring points of intersection will be greater, the higher up the chain they are. However, in a chain of virtually infinite length the highest arc will not differ sensibly from the figure of a taut musical string, since the weight of this arc is as nothing in respect to the weight of the whole chain[2]). Nor would it be difficult to derive from this theory a theory of musical strings agreeing with those given by TAYLOR and by my father . . . (above, §§ 9—10). Experiment shows that in musical strings there are intersections [*i. e.*, nodes] similar to those for vibrating chains . . ."

[This passage makes it plain that DANIEL BERNOULLI has in his hands a direct theory of the simple modes and proper frequencies of the vibrating string, as yet given by no one, but he has not worked out the details. Later he will have heavy grounds to regret that he let his ideas lie undeveloped.]

Theorem 10 concerns a heavy chain of length l suspended from a weightless cord of length λ. If β is the amplitude at the junction $x = l$, then[3])

$$y = \beta \frac{J_0\left(2\sqrt{\frac{x}{\alpha}}\right)}{J_0\left(2\sqrt{\frac{l}{\alpha}}\right)}\,, \qquad J_0\left(2\sqrt{\frac{l+\lambda}{\alpha}}\right) = 0\,. \tag{105}$$

Theorem 11 gives an equation for the proper frequencies when a concentrated mass is fixed at the point where the chain is joined to the cord[4]).

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1) While DANIEL BERNOULLI does not use this term here, he has used it above in connection with discrete systems.

2) In the terms of BESSEL functions, the above passage asserts that the positive roots of $J_0(2\sqrt{z})$ are infinite in number, that the interval between them increases, and that for large z we have $J_0(2\sqrt{z}) \approx f(z)\sin g(z)$, where f and g are virtually constant functions.

3) BERNOULLI does not define λ, but the above seems the obvious explanation; the series written by BERNOULLI is that we denote by $(105)_1$, but linearized with respect to λ, though he does not say that λ/l is small, and in fact for his following example he takes $\lambda = l$. He gives also a rational fraction which he says is a first approximation to the largest value of α satisfying $(105)_2$, but there is no reason why this problem of proper frequencies should differ from the preceding.

4) I am unable to verify the result. In § IX of the paper cited below, p. 159, DANIEL BERNOULLI says that the proof follows by adding a suitable constant in the previous result. That this is so appears from (148).

Theorem 12 concerns the chain of non-uniform thickness. The weight is now $g\,\Sigma(x)$; 25
then the equations determining the shape and frequency are

$$\int y\,d\Sigma = -\alpha\Sigma\frac{dy}{dx}\,, \qquad \nu = \frac{1}{2\pi}\sqrt{\frac{g}{\alpha}}\,. \tag{106}$$

The case $g\,\Sigma = x/l$ gives (99); the case $g\,\Sigma = x^2/l^2$ gives the series we should now 26
write as [1])

$$y = 2\mathfrak{A}\left(\frac{2x}{\alpha}\right)^{-\frac{1}{2}} J_1\left(2\sqrt{\frac{2x}{\alpha}}\right), \quad J_1\left(2\sqrt{\frac{2l}{\alpha}}\right) = 0\,. \tag{107}$$

[In differential form, (106) and its special case $g\,\Sigma = x/l$ are

$$\alpha\frac{d}{dx}\left(\Sigma\frac{dy}{dx}\right) + y\frac{d\Sigma}{dx} = 0\,, \qquad \alpha\frac{d}{dx}\left(x\frac{dy}{dx}\right) + y = 0\,.] \tag{108}$$

The paper ends with a "general scholion" warning that the oscillations must be small 27
if this theory is to apply. This means, for example, that in the middle drawing of Figure 57 we must have $FC << CB$. Finally, "if these pendulums are set in rotary oscillation they will take on the same forms as here determined, and they will complete their rotations in double the time as if they oscillated in a single plane [2])."

[The results in this remarkable paper show that Daniel Bernoulli has mastered the phenomenon of simple modes and proper frequencies for vibrating systems of considerable generality. He is the first to explain by theory of any kind the *sequence of overtones* a single vibrating body may emit. He clearly and explicitly states that for the systems he treats the k^{th} mode has $k-1$ nodes. His reference to the vibrating string in § 21 suffices indeed to explain the existence of its harmonic sounds. It is curious that he does not make any use of Sauveur's terms (above, p. 121). What is missing from Daniel Bernoulli's theory is all reference to *the displacement as a function of time* and any suggestion that the simple modes, which he explicitly recognizes as *special* motions, may be superposed to form more complicated ones.]

In the next volume appear Daniel Bernoulli's *Proofs of his theorems concerning the oscillation of bodies connected by a flexible thread and of a vertically suspended chain* [3]).
These rest upon a *new principle of mechanics*, giving a method for calculating the accelera- I

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1) From our description of this paper and of the work of Euler to be described below, p. 164 *et seqq.*, it is plain that the history of Bessel functions given in Ch. I and other passages of Watson's *A treatise on the theory of Bessel functions*, Cambridge, 1922, is not complete, especially as regards the earliest researches.

2) This proposition, due in principle to Huygens, we have proved above, footnote 3, p. 48.

3) "*Demonstrationes theorematum suorum de oscillationibus corporum filo flexili connexorum et catenae verticaliter suspensae,*" Comm. acad. Petrop. 7 (1734/1735), 162—173 (1740).

tions from the accelerating forces in a *constrained system*. "Think that at a given instant the several bodies of the system are freed from one another, and pay no attention to the motion already acquired, since here we speak only of the acceleration or the elementary change of motion. Thus when any body changes its position, the system takes on a configuration different from that it would assume if not freed. Therefore imagine some mechanical cause to restore the system to its proper configuration, and again I seek the change of position arising from this restitution in any body. From both changes you will learn the change of position in the system when not freed, and thence you will obtain the true acceleration or retardation of each body belonging to the system." [This obscure statement contains the famous *Principle* D'ALEMBERT is to lay down, in scarcely clearer form, as the general law of mechanics in 1743. As we shall see in our analysis of D'ALEMBERT'S work in § 26, the Principle itself is a general statement of an idea created by JAMES BERNOULLI for his solution of the problem of the center of oscillation in 1703.]

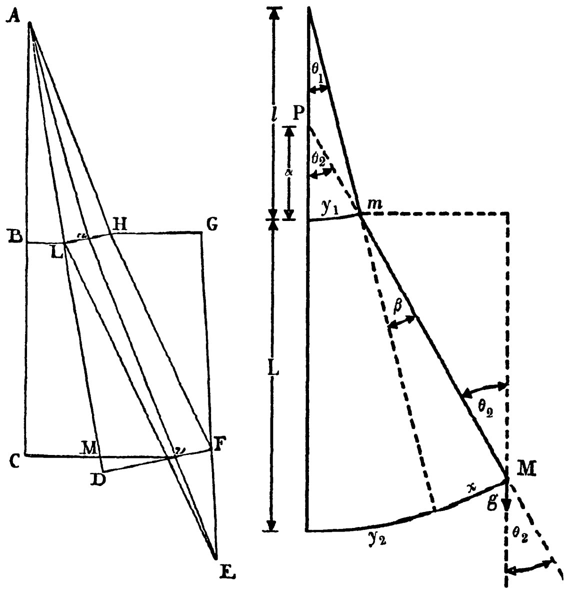

Figure 58.
Diagram for DANIEL BERNOULLI's analysis of the simple modes of a string loaded by two weights

When we try to follow the proof in the special case for two masses, we discover that it rests upon the balance of forces. [Besides BERNOULLI's figure we put a drawing from which the argument seems clearer (Figure 58). In what follows, we replace BERNOULLI's infinitesimal distances by accelerations and omit his awkward geometrical calculations.] (a) Suppose the lower link be freed; then the tangential acceleration of m is $g \sin\theta_1 \approx g\theta_1$, while M moves straight downward with acceleration g. (b) Now restore the lower link; then to make M move on the tangent to the arc y_2 an acceleration along the link must be supplied. To the lowest order in θ_2, this acceleration is g, and hence the resulting acceleration is $g\theta_2$ along the tangent. But

$$\theta_2 \approx \sin\theta_2 \approx \frac{y_2 - y_1}{L} = \frac{1}{L}\left[\frac{l+L}{l}y_1 + x - y_1\right] = \left(\frac{x}{L} + \frac{y_1}{l}\right).$$

Hence

(109)
$$\text{accel. of } M = \left(\frac{x}{L} + \frac{y_1}{l}\right) g\,.$$

(c) The acceleration g along the lower link imparts to m an accelerating force of magnitude Mg and hence an acceleration $\frac{M}{m}g$ along the link. Thus results an acceleration tangent to the arc y_1 in the amount $\frac{M}{m}g\sin\beta \approx \frac{M}{m}g\frac{x}{L}$. Combined with the result (a), this yields

$$\text{(110)} \qquad \text{accel. of } m = \left(\frac{y_1}{l} - \frac{M}{m}\cdot\frac{x}{L}\right)g\,.$$

[Thus far, the analysis is general though clumsy from obscurity of principle along with an unfortunate selection of variables. Now comes, as always in this early work, TAYLOR's assumption:] the accelerations are as the displacements. This yields

$$\text{(111)} \qquad \frac{\frac{y_1}{l} + \frac{x}{L}}{\frac{y_1}{l} - \frac{M}{m}\cdot\frac{x}{L}} = \frac{y_1}{y_2} = \frac{y_1}{\left(1+\frac{L}{l}\right)y_1 + x}\,.$$

This is a quadratic equation for $\frac{x}{y_1}$. Since $y_2 = \left(1+\frac{L}{l}\right)y_1 + x$, two values for the ampli- III
tude ratio $\frac{y_2}{y_1}$ result. From (110) we obtain the frequency $\nu = \frac{1}{2\pi}\sqrt{g}\sqrt{\frac{1}{l} - \frac{M}{m}\cdot\frac{1}{L}\cdot\frac{x}{y_1}}$.
These results prove Theorems 1, 2, and 3.

For three bodies, a similar argument is applied: First the bottom mass may be freed, IV—V
then that next to the bottom.

For the general case, BERNOULLI has perceived a general rule, [apparently by in- VI
duction from the cases of six and seven weights]. Number the masses from the bottom, and let θ_k be the angle between the link connecting M_k with M_{k+1} and that just above it. Then

$$\begin{aligned} &\text{accel. force on } M_1 \propto \sum_{k=1}^{n}\theta_k\,,\\ \text{(112)} \qquad &\text{accel. force on } M_2 \propto \sum_{k=2}^{n}\theta_k - \frac{M_1}{M_2}\theta_1\,,\\ &\text{accel. force on } M_3 \propto \sum_{k=3}^{n}\theta_k - \frac{M_1+M_2}{M_3}\theta_2\,,\ldots \end{aligned}$$

Assuming the accelerations are as the distances then yields as many linear equations as there are unknowns [and thus an equation of degree n] satisfied by the proper frequencies.

From (112) we read off the result for the continuous case, since the angle of contact VII
is $\frac{d^2y}{dx^2}$, where x is the distance measured from the bottom. That is,

$$\text{(113)} \qquad \text{accel. force at } x \propto \int_x^l \frac{d^2y}{dx^2}dx - x\frac{d^2y}{dx^2}\,.$$

If this is set proportional to y, $(108)_2$ results, whence follows Theorem 8. To calculate the period, all we need is the accelerating force at the bottom; this, by analogy to $(112)_1$, is $\int_0^l \frac{d^2y}{dx^2}\,dx = -\left.\frac{dy}{dx}\right|_{x=0}$. This proves Theorem 9.

IX When the density is not uniform, analogy to (112) yields

$$\text{(114)} \qquad \text{accel. force at } x \propto \int_x^l \frac{d^2y}{dx^2}\,dx - \frac{\Sigma}{\frac{d\Sigma}{dx}}\,\frac{d^2y}{dx^2}\,,$$

whence $(108)_1$ follows.

[These two papers are as fine as any DANIEL BERNOULLI ever wrote, and they bring a magnificent contribution to the theory of vibrations, one indeed scarcely equalled by any other work. It is instructive to follow the difficult and clumsy steps by which BERNOULLI demonstrated his results. In addition to being as great an expert on mechanics as any then living, he was an especially thorough student and admirer of NEWTON's *Principia*. Those who parrot the conventional view that NEWTON's principles suffice to solve all problems of mechanics should read these papers, from which it is most plain that if such be the case, DANIEL BERNOULLI, at least, did not know it in 1733. In fact, the method of "NEWTON's equations" is due to EULER and will first appear in his work of 1744—1750 (see § 35, below).]

EULER, indeed, is immediately able to obtain DANIEL BERNOULLI's results more elegantly, but his method is only slightly different. In his paper, *On the oscillations of a*
1 *flexible thread loaded by arbitrarily many weights*[1]), he writes, "Several years ago, when the most enlightened [DANIEL] BERNOULLI was residing here, there was raised between us the question of the curvature of a chain oscillating about one fixed end. But experience taught us that those curves may be most irregular and various, whence we considered the problem not only most difficult but even exceeding human strength unless some restriction is imposed. Therefore we turned our attention to infinitely small oscillations . . ., not seeking . . . all [such] oscillations but only those in which the several parts of the chain occupy a vertical line or natural state simultaneously. We observed indeed that more often the oscillating chain is never entirely displaced to one side only . . ., but we foresaw that the oscillations could be adjusted initially in such a way that the several parts would reach the vertical simultaneously. From this we set up the following problem: *To find the curvature of a chain oscillating in such a way that its several parts reach the vertical simultaneously, and to find the length of the simple pendulum that executes its oscillations in the same time.*

2 "The chain . . . is to be regarded as a thread, perfectly flexible, devoid of gravity, and loaded by infinitely many little weights. And the chain is wont to be regarded in this way

1) E49, "*De oscillationibus fili flexilis quotcunque ponduscudis onusti,*" Comm. acad. sci. Petrop. 8 (1736), 30—47 (1741) = Opera omnia II 10, 35—49. Presentation date: 31 January 1735.

when the shape of a chain hung up by its ends, or the catenary curve, is sought. Therefore . . . the thread is to be regarded . . . as loaded first by one weight, then by two, then by three, . . ., whence . . . the conclusion can be extended to the case of infinitely many little weights . . . Before his departure the most enlightened BERNOULLI gave his solutions 3
without proofs, and just now he has sent us the proofs. Indeed, since at that time he, his father, and I discussed these questions, I too obtained solutions agreeing excellently with his, but since I see now that his method is quite different . . ., I will explain mine here . . ."

Beginning with a simple pendulum, EULER observes that in any configuration 4

$$\frac{\text{accelerating force}}{\text{weight}} = \frac{\text{displacement}}{\text{length of pendulum}}, \tag{115}$$

and this is applied in all the cases which follow. In Figure 58, EULER sets up a straightfor- 5
ward balance of forces. The tangential force on M is $M\theta_2$, that on m is $m\theta_1 - M\beta$. EULER observes that for a simple oscillation the length l as calculated from (115) must have the same value for each body in the system, *viz*

$$\frac{m\theta_1 - M\beta}{my_1} = \frac{M\theta_2}{My_2} = \frac{1}{L+\alpha}. \tag{116}$$

Forming $\tan\theta_2$ shows that $\theta_2 \approx \dfrac{y_2}{L+\alpha} = \dfrac{y_2 - y_1}{L}$; forming $\tan\theta_1$ shows that 6
$\theta_1 \approx \dfrac{y_1}{l} = \dfrac{y_2 - x}{l+L}$, and hence $\beta = \dfrac{x}{L} = \dfrac{y_2}{L} - \dfrac{y_1}{l}\cdot\dfrac{l+L}{L}$. Substitution in (116) yields the quadratic

$$WX^2 + X(1 - \lambda - W - \lambda W) - 1 = 0, \tag{117}$$

where $W = M/m$, $X = y_2/y_1$, $\lambda = L/l$. Therefore just two values of the ratio X are possible. If the displacements conform initially to one of these values of X, they will continue to do so, the point P will therefore remain stationary, and $L + \alpha$ will be the length of the equivalent simple pendulum. With X a root of (117), $L + \alpha$ may be calculated from (116); in fact, $L + \alpha = XL/(X-1)$.

EULER's results on the weighted string are much the same as BERNOULLI's but go 7—14
beyond them in that EULER obtains the explicit solution for the case of n equally spaced and equal weights. His result, which we here express in the notation of "LAGUERRE polynomials", is[1])

1) The introduction of these functions, too, is due to DANIEL BERNOULLI and EULER. Note that comparison of EULER's solution for the loaded string with DANIEL BERNOULLI's for the continuous string suggests at once the famous limit formula

$$\lim_{k\to\infty} L_k\left(\frac{x}{k}\right) = J_0(2\sqrt{x}),$$

along with a corresponding relation for the zeros.

$$y_{k+1} = \mathfrak{A} L_k\left(\frac{a}{\alpha}\right), \quad k = 0, 1, \ldots, n-1, \tag{118}$$

where $\mathfrak{A}$ is the displacement of the lowest weight $(k = 0)$, y_k is the simultaneous displacement of the k^{th} weight, a is the distance between weights, and the frequency is given by $\nu = \frac{1}{2\pi}\sqrt{\frac{g}{\alpha}}$. [EULER gives this result very briefly; it is plain from his analysis that the circular frequencies ω of all the modes are determined from the roots of

$$L_n\left(\frac{a}{\omega}\right) = 0, \qquad \omega^2 = \frac{g}{\alpha}\,.\,] \tag{119}$$

15—20 For the continuous case, EULER easily derives $(108)_1$ and the results (99)—(102). For $(108)_1$ he obtains a general first integral. He notes that for $x < 0$ the curve given by (99) is not suitable because it becomes infinite [*i. e.*, it violates the hypothesis of small displacement, no matter how small is $\mathfrak{A}$]. The entire curve for $x \geqq 0$ is appropriate for representing the semi-infinite continuous chain, [but the figure he gives is crude and does not show the diminishing amplitude and nodal distance].

21—22 The remainder of the paper treats the case when $\sigma = \Sigma' \propto x^n$. From $(108)_1$ follows then

$$\frac{x}{n+1}\,\frac{d^2y}{dx^2} + \frac{dy}{dx} + \frac{y}{\alpha} = 0\,, \tag{120}$$

and the first integral reduces to a Riccati equation. If $n = -\frac{1}{2}$, we get the solution

$$y = \mathfrak{A} \cos\sqrt{\frac{2x}{\alpha}}\,. \tag{121}$$

23 For general n, EULER derives the series solution of (120) we should now write in the notation of BESSEL functions as

$$\frac{y}{\mathfrak{A}} = q^{-\frac{n}{2}} I_n(2\sqrt{q})\,, \qquad q = -\frac{(n+1)x}{\alpha}\,; \tag{122}$$

24 [this is the first appearance of "BESSEL functions" of arbitrary real index[1]). EULER does not discuss the mechanical interpretation but rather derives the integral form

$$\frac{y}{\mathfrak{A}} = \frac{\int_0^1 (1-t^2)^{\frac{2n-1}{2}} \cosh\left(2t\sqrt{\frac{(n+1)x}{\alpha}}\right) dt}{\int_0^1 (1-\tau^2)^{\frac{2n-1}{2}}\, d\tau}\,. \tag{123}$$

[This is perhaps the earliest example of solution of a second-order differential equation by

1) The result (121) is equivalent to

$$J_{-\frac{1}{2}}(z) = \left(\frac{2}{\pi z}\right)^{\frac{1}{2}} \cos z\,.$$

a definite integral. The result itself, slightly transformed, is usually called "POISSON's integral representation", *viz*

$$(124)\qquad J_n(z) = \frac{2}{\sqrt{\pi}}\,\frac{(\frac{1}{2}z)^n}{\Gamma(n+\frac{1}{2})}\int_0^{\frac{1}{2}\pi}\cos(z\sin\varphi)\cos^{2n}\varphi\,d\varphi.$$

The order of discovery in the foregoing works we cannot ascertain. Comparing them, we find DANIEL BERNOULLI's clearer in respect to the *phenomena* being explained, EULER's clearer in the *analysis*. Although EULER mentions the sequence of proper frequencies and modes [1]), he fails to give them the emphasis they deserve; BERNOULLI sees that they are representative of *a phenomenon occurring in all vibrating systems*. While EULER's derivations are clearer, BERNOULLI's contain deeper undeveloped possibilities. As far as *principle* is concerned, both researches are incomplete, and to about the same extent: Both investigators failed to establish the equations of motion and failed to connect the simple modes with more general motions. The permanence here, then, lies in the phenomenon rather than the analysis used to derive it; thus it is the achievement of DANIEL BERNOULLI, and it is a very great one.]

24. DANIEL BERNOULLI's and EULER's first calculations of simple modes and proper frequencies for the transverse vibrations of bars (1734—1735). [Just before the papers described above were written, there began between the two authors the most interesting of all correspondences [2]) concerning mechanics, for after leaving Russia DANIEL BERNOULLI

1) Contrary to the assertion of BURKHARDT, § 3 of *op. cit. ante*, p. 11.

2) Five sources for the correspondence of EULER with JOHN and DANIEL and JOHN III BERNOULLI have been available to me:

I. P.-H. FUSS, *Correspondence mathématique et physique de quelques célèbres géomètres du XVIIIème siècle* 2, St. Pétersbourg, 1843.

II. G. ENESTRÖM, "*Der Briefwechsel zwischen LEONHARD EULER und JOHANN I BERNOULLI,*" Bibliotheca Math. (3) 4, 344—388 (1903); 5, 248—291 (1904); 6, 16—87 (1905).

III. G. ENESTRÖM, "*Der Briefwechsel zwischen LEONHARD EULER und DANIEL BERNOULLI,*" Bibliotheca Math. (3) 7, 126—156 (1906—1907).

IV. In the BERNOULLI Archive at Basel are photostats of a number of letters, some unpublished, from all parties.

V. In the BERNOULLI Archive at Basel are transcripts of the passages omitted by FUSS (No. I).

For assistance in using Nos. IV and V I am deeply indebted to Professor SPIESS.

I have been informed that a great deal of relevant manuscript material is preserved in the archives of the Academy of Sciences, Leningrad.

In seeing DANIEL BERNOULLI's remarks only in translation, the reader loses the pleasure of savoring the private dialect he employs. *E. g.* on 6 June 1729 he writes to EULER, "P. S. Weil ich aus dero ersterem gesehen, daß Sie sonderlich rein toütsch zu schreiben sich beflissen, als zweifle ich nicht, ich werde Dero keüsche ohren sehr mit meinem undermengten frantzösichen & lateinischen wörtern verletzt haben, weswegen sehr umb verzeihung bitte. ade noch einmahl."

exchanged problems and solutions with the friend he left behind. Apparently these letters put in writing the kind of communication that passed orally between the two great geometers when they were colleagues in Petersburg. While not going so far as to conceal their methods from one another, they chose on the whole to disclose results and discuss phenomena. Thus it often turned out that their ideas were different though the results were more or less the same, and thus, at first, double publication resulted. While both were annoyed by the delay, sometimes as much as eleven years, between the writing of a paper and its appearance in the Petersburg Memoirs, in fact it ensured their monopoly of the field of oscillating systems for a decade : No one learned even their results through publication until they themselves were far ahead on more difficult problems. During this period EULER continued also the correspondence he had begun with JOHN BERNOULLI, his teacher and the father of DANIEL. The two correspondences interlace to some extent, especially since old JOHN BERNOULLI conceals much of his doings from his son, who sometimes writes to St. Petersburg to find out what his father and colleague in the University of Basel is up to.

Nothing of interest[1]) concerning our subject appears] until 18 December 1734, when DANIEL BERNOULLI says he has studied the small vibrations of a horizontal uniformly elastic band with one end fixed in a wall, "but I am not very pleased with my solution." On 4 May 1735 BERNOULLI thanks EULER for having read to the Academy his paper, described above, on the hanging chain. BERNOULLI now has shown, he says, that the transverse displacement y of an elastic band fixed at one end in a wall satisfies the differential equation

$$K^4 \frac{d^4 y}{dx^4} = y \,, \tag{125}$$

where K^4 is a constant. Has EULER thought about this subject? "But this matter is very slippery, and I should like to hear your opinion on it." BERNOULLI says the "logarithm" satisfies (125) as well as $K^2 \dfrac{d^2 y}{dx^2} = y$, "but no such [logarithm] is general enough for the present business."

1) The correspondence begins tamely. From Paris on 22 September 1733 DANIEL BERNOULLI writes that he has determined the form of equal resistance for a horizontal beam loaded by gravity and an attached weight. He promises to send a memoir on this subject to the Academy. On 18 February 1734 EULER replies that "The . . . problem concerning the form of beam . . . requires a theory of breaking such as that given by your honorable uncle, and as it seems to me he treated this very problem" [this last is a lapse of memory on EULER's part]. "However, the complete working out and application is surely the most beautiful and the most difficult in this subject, and thus I await with pleasure the dissertation your worship has promised on this subject." From EULER's letter of November 1734 and BERNOULLI's of 18 December 1734 we learn that a memoir of BERNOULLI's, which might well be the promised one concerning solids of equal resistance, was lost in the post.

In his reply[1]), written before June, 1735, EULER says he has already derived (125) and the more general equation

$$(126)\qquad K^4 \frac{d^2y}{dx^2} = \int_0^x dx \int_0^x y\,dx + \alpha \int_0^y x\,dy\,,$$

valid for a heavy bar [and including $(108)_2$ as the special case when $K = 0$].

EULER, too, is unable to integrate (125) except in series. [These are two great mathematicians who have just shown themselves not fully familiar with the exponential function; we must recall that this is 1735!] He applies the end conditions

$$(127)\qquad \int y\,dx = 0, \ \int dx \int y\,dx = 0\,;$$

we should now write his series solution in the form

$$(128)\qquad y = \mathfrak{A}\left[\left(\cos\frac{x}{K} + \cosh\frac{x}{K}\right) - \frac{1}{b}\left(\sin\frac{x}{K} + \sinh\frac{x}{K}\right)\right],$$

where b is determined by the condition $y = 0$ when $x = l$, *viz*

$$(129)\qquad b = \frac{\sin\frac{l}{K} + \sinh\frac{l}{K}}{\cos\frac{l}{K} + \cosh\frac{l}{K}}\,,$$

[but not until four years later is EULER to recognize his series as representing these simple expressions]. "By this method it would not be very difficult to solve the same problem when the band is not taken as everywhere equally thick and equally elastic."

Shortly thereafter EULER completed *A new and easy method for the very small oscillations of rigid or flexible bodies*[2]), presenting a new approach "of the greatest generality

1) Undated, unpublished, included in Source IV cited on p. 165. In this letter EULER gives a derivation of the equation $(108)_2$ for the heavy hanging cord; this derivation, using the method of E8 (above, p. 148) and hence different from that in E49 (dated 31 January 1735, cited above, p. 162), is that published in E40 (dated 27 October 1735, cited in the next footnote). This new method, essentially, rests upon a special case of what is now called "D'ALEMBERT's principle", but it is applied to the balance of moments rather than of forces. Thus it is close to the ideas of JAMES BERNOULLI's great paper of 1703.

In his answer, dated 4 June 1735, DANIEL BERNOULLI writes "I have still other mechanical principles beyond [that] of the change of the system from the force of gravity and its subsequent restitution, from which principles I have solved the problem of the vibrations of a flexible chain; thereafter, [that] of the change of the system from continued motion and subsequent restitution, which I have not as yet published anything, *etc.*" This passage, again, suggests D'ALEMBERT's principle.

2) E40, "*De minimis oscillationibus corporum tam rigidorum quam flexibilium methodus nova et facilis*," Comm. acad. sci. Petrop. 7 (1734/5), 99—122 (1740) = Opera omnia II 10, 17—34. Presentation date: 27 October 1735.

and so well founded in the principles of statics that by its aid not only did I solve with remarkable ease questions of the oscillations of an elastic band and of a hanging cord, but
7 also I can very quickly set in order all things connected with oscillations." EULER considers only very small oscillations "because these, not only for a simple pendulum but also for any body, are isochronous; only very rarely may larger oscillations be compared with those of a pendulum." The new method consists in combining the ideas of EULER's two previous papers. First, by (115) the accelerating force is expressed in terms of the length α of the isochronous pendulum. This is then put into the equation expressing balance of moments; for continuous lines, this is (91). [Thus, again the equations of motion are
8—31 avoided.] The first part works out the radius of gyration for a rigid body by this method. EULER is able to show that his principle, if applied with sufficient caution and ingenuity, yields all previously known results concerning the centers of oscillation of various bodies.

32—36 For an elastic band fixed at one end in a vertical wall and oscillating in a horizontal plane, by (115) the accelerating force on the element dx is given by $F_y dx = \frac{\sigma g y}{\alpha} dx$, where y is the displacement. Substitution in (91) yields

(130) $$\frac{\mathcal{B}}{r} = \mathcal{B}\frac{d^2y}{dx^2} = \frac{g}{\alpha}\int_0^x dx \int_0^x \sigma y\, dx\,.$$

Differentiation yields (125), with

(131) $$K^4 = \frac{\alpha\mathcal{B}}{\sigma g}\,; \quad \text{[equivalently, } \nu = \frac{1}{2\pi K^2}\sqrt{\frac{\mathcal{B}}{\sigma}}\,.$$

The rudimentary state of the theory of differential equations is shown by EULER's statement concerning the simple equation (125):] "But from this differential equation of fourth order it is very difficult to derive anything toward understanding the oscillation of
37 elastic bands." [Without giving any reasons,] EULER proposes for the free end, $x = 0$, the conditions (127) along with $y = \mathfrak{A}$. [In what follows the conditions he actually uses are

(132) $$y = \mathfrak{A},\ \frac{d^2y}{dx^2} = 0,\ \frac{d^3y}{dx^3} = 0\,;$$

the second and third follow from (130).] For the end at the wall, where $x = l$ and $y = 0$, he proposes $dy/dx = 0$, "as required by the nature of the spring, which cannot be bent
38 through a finite angle except by an infinite power." EULER is still unable to integrate (125) except in series; in addition to (129), by applying the condition $dy/dx = 0$ at $x = l$ he obtains a result we should now write as

(133) $$b = \frac{\cos\frac{l}{K} + \cosh\frac{l}{K}}{-\sin\frac{l}{K} + \sinh\frac{l}{K}}\,.$$

Between (129) and (133), b is to be eliminated; EULER simply indicates the resulting series of powers of $(l/K)^4$ which must vanish; [in the notations used in (129) and (133), this equation is 39

$$1 + \cos\zeta \cosh\zeta = 0\,, \qquad \zeta \equiv \frac{l}{K}\,]\,. \tag{134}$$

EULER gives the approximate root

$$\zeta \approx \sqrt[4]{\frac{25}{2}}\,. \tag{135}$$

From (131) follows

$$\nu = \frac{\zeta^2}{2\pi l^2}\sqrt{\frac{\mathcal{B}}{\sigma}} = \frac{\zeta^2}{2\pi l^2}\sqrt{\frac{\mathcal{B}}{\varrho A}}\,, \tag{136}$$

whence EULER concludes that "*for various elastic bands of the same uniform thickness . . . the periods will be directly as the squares of the lengths of the bands and inversely as the square roots of the absolute elasticities.*" [Thus appears a proof of the law EULER had asserted, though without sufficient qualification, in his *Music* (above, p. 154). EULER fails to state that there are infinitely many possible frequencies, although for the similar problem of the heavy hanging cord he has mentioned this fact.] He suggests obtaining the ratios of 40 elasticities of two substances by comparing the periods of oscillation of bands of the same dimensions. [Thus at last we encounter an explicit case to substantiate LEIBNIZ's perception that the elastic and acoustic properties of a body are connected (above, p. 63). EULER's proposal, while neglected in his own day, is widely applied in ours.]

When the weight of the band is taken into account, it being supposed that the band 41 points vertically downward when in equilibrium, we have $F_x = -\sigma g$, and (91) now yields (126), with K again given by $(131)_1$.

EULER shows also that application of (91) and (115) to the vibrating string yields 42—43 (76) and (75), which "the most enlightened TAYLOR . . . and JOHN BERNOULLI . . . obtained from far different principles[1])." The paper concludes with a derivation of the equation 47 for transverse vibrations of a rod with both ends pinned. [The result, of course, should again be (125), but EULER makes an error in sign.

Thus by a single method all known oscillation problems were united, and new solutions were obtained. To include in a common scheme not only flexible and elastic oscillations but also *rigid* ones, the method of moments was surely the only possibility. It is easy to see today that this method is ill adapted for further progress toward the general principles of motion of continuous bodies and that EULER's early work, while elegant and efficient for the immediate aim, is a false start.

1) The *dynamical* principles are indeed different, but all three authors are alike in assuming, in one way or another, that the restoring force is as the displacement.

In the foregoing exchange of letters and subsequent paper by Euler we have seen the first example of what is now to be the typical phenomenon : Bernoulli and Euler discuss a problem, and each achieves a measure of success. Within a few months, Euler has worked his form of the solution into a clear, finished paper including a sequence of generalizations[1]). Daniel Bernoulli publishes nothing but goes on to new problems, often accompanied by experiments. One essential part of mathematical research he fails to appreciate, namely, that a work is not finished until it is published. Ideas that seem clear and final to the thinker often show on paper as but the beginning of the research. Years later he will come to regret his indolence. He will see his ideas rediscovered or developed not only by Euler but also by the inimical d'Alembert and Lagrange, whereupon, in a vain attempt to reestablish the brilliant work he had done in younger years, he will quickly write it up and publish it, stubbornly refusing to see in the later researches of others anything of value beyond his own old and now primitive ideas, ideas which if shaped and presented when fresh would have earned him a greater name in the history of mechanics than in fact he has.]

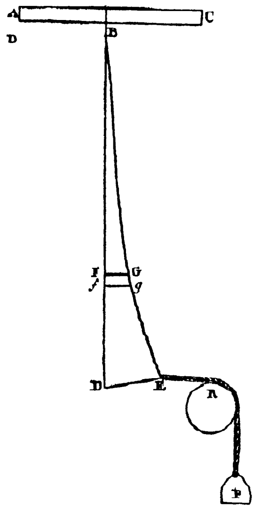

Figure 59.
Daniel Bernoulli's definition of the stiffness of an elastic band

25. Further researches on the elastica; Euler's general solution for linear vibration problems (1735—1739). On 26 October 1735 Daniel Bernoulli writes to Euler, "Your remarks on the vibrations of an elastic band agree with mine. The most important thing to calculate is this (Figure 59) : Given the length of the elastic band BD or BE, given its weight [*i. e.* attached weight] P, given the distance DE, which is the measure of its elasticity[2]), to find the absolute number of vibrations in a given time."

Euler considered this letter so important that he wrote a summary of it, amplified by his own analysis solving the problem[3]). [It seems strange that neither Euler nor Daniel Bernoulli mentions that this special case is included in James Bernoulli's formulation (above, p. 101). Instead,] Euler sets up the problem afresh and thus reaches (57) with $\pm\, ab = -\, l^2$

1) As Daniel Bernoulli wrote him on 4 June 1735, "Be assured that I esteem your judgment above all, especially since from what you say you also have applied yourself to mechanics at the same time, and everything you understand you deepen at once."

2) In this sense Euler interpreted Bernoulli's "cuius ope elasticitas habetur"; another possible meaning is that from the displacement produced by a given weight the elasticity (*i. e.* elastic modulus) may be calculated.

3) E830, "*Recensio litterarum a Cl. D. Bernoullio Basilea die 26. Oct. 1735 ad me datarum una cum annotationibus meis,*" Opera postuma **2**, 125—128 = Opera omnia II **11**, 374—377. In the latter publication part of Bernoulli's letter is adjoined.

and $a^2 = 2\mathcal{B}/P$. [Just as had JAMES BERNOULLI for the rectangular elastica (above, p. 95),] EULER integrates (57) in series, obtaining the first terms of a power series for the end displacement $\delta = y(l)$ as a function of $\frac{1}{2}l^3P/\mathcal{B}$. Approximate solution for $\mathcal{B}$ yields

$$\mathcal{B} = \frac{l^3P}{3\delta - \frac{81}{35}\frac{\delta^3}{l^2}} = \frac{Pl^3}{3\delta}\cdot\frac{1}{1-\frac{27}{35}\frac{\delta^2}{l^2}}\,. \tag{137}$$

Combination with EULER's formula (136), after readjustment of units, yields the solution to the problem posed by BERNOULLI. [This is the earliest solution for small deflection of an elastic beam.

Here we see DANIEL BERNOULLI, as usual, preferring to express all results directly in terms of measurable quantities, while EULER prefers quantities leading to the maximum formal conciseness :] EULER adds the remark, "In place of an experiment of this kind, it seems apter to me to determine the value of the letter $\mathcal{B}$ directly from the number of oscillations, easily discerned by observation." [The tradition of elasticity, both theoretical and experimental, has followed EULER. His absolute elasticity $\mathcal{B}$ is now adopted universally as the measure of susceptibility of a beam to bending, though the concept is usually refined by splitting $\mathcal{B}$ into a modulus of the material and a geometrical property of the cross-section : $\mathcal{B} = EI$, as in (86).]

DANIEL BERNOULLI's younger brother, JOHN II BERNOULLI, won the Paris prize of 1736 with his *Physical and geometrical researches on the question: How does the propagation of light take place*[1]? Much of this work concerns mechanical vibration problems. There are XXXVII
two kinds of equilibrium, illustrated by a body connected to two springs : In "*forced equilibrium* . . ., a body is held in equilibrium by two tense springs, which make equal efforts to dilate themselves in opposite directions . . .," while in "*idle equilibrium* . . . the body is located between two loose or released springs, so that it remains in equilibrium, or rather at rest, simply because it is pressed neither on the one side nor on the other. The "General XXXVIII
proposition" asserts that any body slightly displaced from a position of forced equilibrium will execute a periodic and isochronous motion. JOHN II BERNOULLI's graphical argument shows that he considers only the case when both springs obey the same law, so that the total force on the body is $F = f(x) - f(-x)$. In effect, he replaces F by the first two terms of its power series expansion, so that

$$\begin{aligned} F &= f(0) + xf'(0) + \cdots - [f(0) - xf'(0) + \cdots]\,, \\ &= 2xf'(0) + \cdots \end{aligned} \tag{138}$$

1) *Recherches physiques et géometriques sur la question; Comment se fait la propagation de la lumiere*, Pièce qui a remporté le prix de l'acad. . . ., Paris, 1736 = Recueil des pièces qui ont remporté les prix de l'acad. . . . **3** (1752). In the correspondence between JOHN I BERNOULLI and EULER, this work is discussed in the letters of 2 April, 27 August, and 6 November 1737.

From the "known property" of forces proportional to the distance, the motion is iso-
XXXIX chronous. "Hence . . . the flutterings of an elastic body, when it is in a state of compres-
XL sion . . ., are isochronous when the body has just been struck or violently disturbed . . . It
must be remarked that forced equilibrium is absolutely necessary in order that the flut-
XLI terings, large or small, be isochronous." The argument seems to rest on assuming that the total force is an even function of the displacement; in criticizing this passage, DANIEL BERNOULLI soon thereafter[1]) asks, "is it not plain" that the force is an odd function? [In respect to the theoretical and experimental laws of elasticity, all this shows poorly in contrast to the searchings of the previous generation, but JOHN II BERNOULLI's attempt is apparently the earliest to describe the nature of small oscillations subject to a general non-linear spring.]

LIX—LXVIII JOHN II BERNOULLI's treatment of small longitudinal oscillations[2]) follows closely his father's analysis of transverse oscillations, leading to (78). For a conical string, he asserts [but does not demonstrate] the differential equation

$$C\frac{d^2y}{dx^2} = -x^2y\,. \tag{139}$$

While he is unable to integrate this, he says that "methods of approximation show very certainly that conical strings . . . vibrate more rapidly than those of uniform thickness, other circumstances being equal." [This is false, as will appear from later results of EULER (below, p. 302).]

Three years later arose a new problem which would seem unconnected but in fact gave rise to important researches in our subject. On 24 May 1738 DANIEL BERNOULLI writes to EULER that certain mechanical problems lead him to wish to find among all isoperimetric curves that for which $\int r^m ds =$ minimum or maximum, where r is the radius of curvature. On 30 July EULER communicates his solution of this problem to JOHN BERNOULLI[3]). On

1) In § XXVIII of "*Recherches mécaniques et astronomiques sur la question proposée par l'Académie Royale des Sciences pour l'année 1745. La meilleure manière de trouver l'heure en mer, par observation, soit dans le jour, soit dans les crepuscules, & sur-tout la nuit, quand on ne voit pas l'horison,*" Recueil des pieces qui ont remporté les prix . . . **6** (1752).

2) *Cf.* Part II E of my introduction to EULER's Opera omnia II **13**. In § LVIII JOHN II BERNOULLI repeats the old assertion of TAYLOR (above, p. 131) that a string deformed into a triangle will assume the form of a sine curve after a few vibrations.

3) The problem is mentioned also in JOHN BERNOULLI's letter of 11 October 1738 to EULER, in DANIEL BERNOULLI's letter of 9 August to EULER, and in EULER's of 13 September to DANIEL BERNOULLI. The results are stated on pp. 358—359 of Notebook EH3 (cited above, p. 142). With his customary dispatch, EULER prepared his results for publication in E99, "*Solutio problematis cuiusdam a celeb. DAN. BERNOULLIO propositi,*" Comm. acad. sci. Petrop. **10** (1738), 164—180 (1747) = Opera omnia I **25**, 84—97; presentation date: 9 September 1738. This paper begins: "In the last letter that the famous DANIEL BERNOULLI sent me from Basel, dated the 24th of May of this year," *etc.* In § 10

8 November DANIEL BERNOULLI writes, "This problem of mine is very real, and I was led to it by some phenomena of nature, and it includes the most general equation of the elastica when ds is regarded as constant. But if $d\xi$ is taken as constant, then the elastica, as I can show, is of such a nature that . . .

$$\int \frac{ds^3}{r^2 d\xi^2} = \text{max. or min.} \tag{140}$$

I will send my reflections on this subject another time. Thus I wonder why my father in his inclosed letter[1]) says *that which the proposer of the problem himself did not know*, etc."

On 20 December 1738 EULER writes to JOHN BERNOULLI, "I have recently noticed a singular property of the rectangular elastica," *viz* (in the notation of (50))[2])

$$\frac{s(c)}{c} \cdot \frac{y(c)}{c} = \int_0^1 \frac{dx}{\sqrt{1-x^4}} \cdot \int_0^1 \frac{x^2 dx}{\sqrt{1-x^4}} = \tfrac{1}{4}\pi\,, \tag{141}$$

"which observation seems to me most noteworthy"[3]). Three days later he writes to DANIEL BERNOULLI, "Meanwhile I am very curious to learn from your Worship what use this problem may have in discovery of the elastic curves. For superficially I see well that these curves have a maximum or minimum in the course of bending, of the kind the catenary has, as the one whose center of gravity falls lowest among all isoperimetric curves.

"What does your Worship think of the property of the rectangular elastica I communicated to your father . . .," *viz* (141). "I have come across this most obliquely and against all expectation . . ." EULER mentions that the result came up in the course of his investigations on sequences, and he gives a long list of such multiplication formulae.

The nature of the minimum principle DANIEL BERNOULLI explains on 7 March 1739. "I have today a quantity of thoughts on elastic bands, . . ., *etc*. . . . On the first occasion I will show how these [variational] problems include the curvature of the elastica." For the

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EULER considers the special case when $m = -2$ but without the isoperimetric restriction; the solution then leads to JAMES BERNOULLI's quadrature (49), "whence it is learned that the curve satisfying it is an elastica normal to the axis . . .".

1) In the correspondence we often see old JOHN BERNOULLI gloating over his son's inferiority to EULER.

2) This "new property of the elastica" is derived as a corollary of a more general result and illustrated by a diagram on p. 398 of Notebook EH3.

3) On 7 March 1739 JOHN BERNOULLI replies, characteristically, that he himself in earlier days has shown that the sum of the two quadratures in (141) could be expressed in terms of the length of an ellipse. With his usual frankness, EULER writes on 5 May 1739 that such properties as (141) "seem to me more noteworthy in proportion to the indirectness of the route by which they are proved or discovered." JOHN BERNOULLI's property, on the other hand, is one of those things that "comes of itself as soon as one looks for it."

general equation for the uniform band, naturally straight and elastic, it is necessary that (140) holds. "For I can show that any band forced into a state of given curvature must be endowed with a potential live force equal to $\int \frac{ds^3}{r^2 d\xi^2}$, and I think that an elastic band which takes on of itself a certain curvature will bend in such a way that the live force will be a minimum, since otherwise the band would move of itself. I plan to develop this idea further in a paper; but meanwhile I should like to know your opinion on this hypothesis." On 5 May EULER replies, "That the elastic curve must have a maximum or minimum property I do not doubt at all . . . but what sort of expression should be a maximum was obscure to me at first; but now I see well that this must be the quantity of potential forces which lie in the bendings : but how this quantity must be determined I am eager to learn from the piece which your Worship has promised[1]) . . ."

On the same day EULER explains to JOHN BERNOULLI the method by which he discovered (141) : multiplication of series [so as to obtain an infinite product,] followed by a "special manner" of integrating[2]).

By the accidental observation that a watch when hung up sets itself in vibration as a pendulum, KRAFFT[3]) reopened the problem of forced oscillations. The subject as it was conceived concerns only a single oscillator and thus does not properly belong in this history. However, recalling the crude state of the theory of a single free

1) DANIEL BERNOULLI is largely absorbed in his own problems and frankly gives up the attempt to follow his friend's diverse researches. Also he seems to forget rather quickly the contents of previous letters. In this letter EULER finds it necessary to remind him that "on an occasion your Worship provided" he long ago read a piece on the vibrations of elastic bands (doubtless E40, above, p. 167) and had told DANIEL BERNOULLI of the content.

2) This is the method by which EULER calculated $\sum_{n=1}^{\infty} \frac{1}{n^2}$. It is given in E122, "*De productis ex infinitis factoribus ortis,*" Comm. acad. sci. Petrop. 11 (1739), 3—31 (1750) = Opera omnia I 14, 260—290. There (141) appears as a special case of a more general formula for the product of two quadratures. A proof using gamma functions was given by TODHUNTER, § 59 of *op. cit. ante*, p. 11.

DANIEL BERNOULLI took no note of EULER's relation (141) until 12 December 1742, when he indicated it to be of little interest to him because obtained as "a corollary and as if *a posteriori*," and on 20 March 1745 only "Your last proof . . . is indeed easier than the first one." These remarks are symptoms of DANIEL BERNOULLI's growing dislike for pure mathematics. While he had been a leading mathematician in his youth, by the end of this history we shall find him a confirmed enemy of all that is not "useful" and, as a corollary, left behind in the development of physical principles expressed by partial differential equations.

On the other hand, with his usual feeling for important clues, EULER seems particularly proud of (141); he communicates it to CLAIRAUT in an undated letter of 1742—1743, and he comes back to it again and again, until finally it reveals itself to him as only a special case of the addition theorem for elliptic integrals he is to discover later (below, p. 357).

3) "*De novo oscillationum genere,*" Comm. acad. sci. Petrop. 10 (1738), 200—206 (1747). The theory in this paper is confined to calculation of moments; no motions are determined.

harmonic oscillator scarcely two decades previous[1]), we notice in passing the development of this second necessary preliminary to a full theory of elastic vibrations. Since the time of BEECKMAN and GALILEO (above, pp. 26—27, 34—35)[2]), the phenomenon of resonant

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1) This is confirmed by EULER's labored discussion of it in the first part (§§ 9—21) of the paper we are just about to consider.

2) A related but more elaborate problem, which may be idealized as that of free oscillation of two elastically coupled pendulums, or as that of the motion of two pendulums attached to the arms of a balance, or as that of oscillation of one pendulum driven by a harmonic generator acting at the end of a spring, had been raised by HUYGENS in the earliest days of his pendulum clocks. In a letter to R. F. DE SLUSE of 24 February 1665 he mentions "the remarkable sympathy of my clocks, just discovered." A part of his letter of 26 February 1665 to his father found its way into print: "*Extrait d'une lettre de la Haye le 26. Fevrier 1665*," J. des Sçavans 1 (1665—1666), No. 11 (16 March 1665), Amsterdam ed. 148—150 = Œuvres 5, 244. Two clocks having pendulums of nearly equal length are found, when hung up one or two feet apart, to come into perfect consonance within a half hour; if this consonance is forcibly broken, it reestablishes itself; while if the two clocks are separated by a distance of fifteen feet, one gains 5 secs. per day upon the other. The agreement does not imply that the pendulums swing parallel to one another; rather, they swing in opposite senses. HUYGENS attributes the phenomenon to "a kind of sympathy, . . . an imperceptible agitation of the air, produced by the motion of the pendulums." However, when he placed a large baffle between the clocks, the effect was not at all diminished.

(On 27 February 1665 HUYGENS wrote to the same effect to Sir ROBERT MORAY; according to BIRCH, *History of the Royal Society* . . . 2, 19 (1756), this letter was read to the Royal Society on 1 March 1664/5, whereupon the Society directed that experiments be instituted to see if "this pretended sympathy" were "magnetical" and also "whether three or four watches do the same, that two do." On pp. 14—15 of Phil. Trans. 1, No. 1, 6 March 1664/5, is printed part of the letter but nothing concerning sympathy. HUYGENS' letter to MORAY of 27 March 1665 shows that the publication in the Journal des Sçavans was without his knowledge and contrary to his wish.)

In his letter of 6 March 1665 to MORAY, HUYGENS describes experiments showing that "the sympathy . . . does not come from the motion of the air but from the said small disturbance" imparted by the mechanism to the case and can be nullified by sufficiently firm mounting. According to BIRCH, *ibid.*, 21, this letter was read to the Royal Society on 8 March; the only recorded response is the utterance of doubts that pendulum clocks are accurate. Pp. 162—163 of the Journal des Sçavans 1, No. 12, 23 March 1665, carry an elaborate retraction of HUYGENS' first conjecture.

On 5 October 1665 R. PAGET in a letter to HUYGENS speaks of "the sympathetic or homotonic oscillation of your clocks" as being "not unlike the harmonic motion of musical strings."

On p. 509 of *op. cit. ante*, p. 126, DE LA HIRE in 1692 recalls HUYGENS' observations and attributes to him the explanation that the beam connecting the clocks "fell into a motion midway between the two, which it communicated back to the pendulums," and DE LA HIRE adds a muddy experiment of his own.

Just before KRAFFT's observation, J. ELLICOTT rediscovered HUYGENS' phenomenon. In his paper, "*An account of the influence which two pendulum clocks were observed to have on each other*," Phil. Trans. London 41, No. 453 (1739), 126—128 (1742), ELLICOTT finds a baffling variety of influences; *e. g.*, one clock may stop dead. In his "*Further observations and experiments concerning the two clocks afore-mentioned*," *ibid.* 128—135, ELLICOTT has come to realize that the vibration is communicated to the cases and thence to the common flooring, *etc.* This Fellow of the Royal Society seems to

oscillation had been recognized, but for the motion of a naturally oscillatory system subjected
22 to a periodic force, no theory of any kind existed. Euler at once reduces the problem to its essentials and in the paper, *On a new kind of oscillations*[1]), considers the sinusoidally driven harmonic oscillator :

$$M\ddot{x} + Kx = F \sin \omega_d t \tag{142}$$

27, 28 First he obtains the solution by quadratures. Then he remarks that in one special case this solution must be replaced by another which shows that "after an infinite time these oscil-
29—34 lations grow out to infinity and run over an infinite space." Somewhat taken aback, he approaches the whole problem anew by integration in series and obtains the same results ; this time he introduces formally the dimensionless ratio of driving frequency ω_d to natural frequency $\omega = \sqrt{K/M}$,

$$n \equiv \frac{\omega_d}{\omega}\ , \tag{143}$$

40—41, 35 whose significance he recognizes only at the very end. For $n = 1$ he verifies his earlier
39 solution. After further experimentation with special cases, he finally realizes that "among all these cases the one when $[n = 1]$ deserves the greatest notice ; in it, the space in which each oscillation is contained increases continually and finally grows out to infinity. This effect is all the more to be wondered at, since it occurs in this special case alone and arises from finite forces. Therefore, if it can conveniently be reduced to practice, it seems to allow the invention of perpetual motion." One has only to apply to a cycloidal pendulum an "automaton" having the same period and then to overcome resistance and friction sufficiently that the oscillations, though not increasing, at least perpetually conserve the same amplitude.

Euler is unable to classify the results for values of n other than 1 but infers that the oscillations will be "the more irregular" the more the ratio n "fails of commensurability."

[Thus Euler obtains the first theory of *resonance*. The paper reads like an excerpt from a notebook. The brilliant discovery it contains might have been better understood had Euler withheld the long calculations in favor of a clear explanation of the results. His

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have some vague notions of the principles of mechanics, but he proceeds in the pragmatic way favored in England at this time and hence finds nothing but a mass of bewildering details. His floundering explanations show that he has no idea how one might study precisely, either by theory or by rationally designed experiment, the simplest vibration problems.

1) E126. "*De novo genere oscillationum,*" Comm. acad. sci. Petrop. **11** (1739), 128—149 (1750) = Opera omnia II **10**, 78—97. Presentation date: 30 March 1739. In this work, as Euler wrote on 5 May 1739 to John Bernoulli, he found "such various and wonderful motions as would surely fail to be suspected until the calculation was completed."

recommendation for an "automaton" passed unnoticed by the "practical" men of his time but furnishes a key to the resonant circuits of today.]

We have seen above that neither EULER nor DANIEL BERNOULLI could integrate except in series the simple differential equation (125) governing the form of a vibrating rod. On 15 September 1739 EULER writes to JOHN BERNOULLI, "I have recently discovered an extraordinary way of integrating differential equations of higher order all at once, so that right away a finite equation results." [What EULER has found is the method of obtaining the general solution for a linear differential equation of n^{th} order with constant coefficients by *superposition* of particular solutions of the form e^{px}.] Let the equation be

$$\sum_{k=0}^{n} A_k \frac{d^k y}{dx^k} = 0\,, \qquad A_k = \text{const.}, \tag{144}$$

and let p stand for a real root or a pair of complex roots $p = \alpha \pm i\beta$ of the polynomial equation

$$\sum_{k=0}^{n} A_k p^k = 0\,. \tag{145}$$

Then the parts of the solution corresponding to p are, respectively,

$$y = Ce^{px}, \quad e^{\alpha x}(C\cos\beta x + D\sin\beta x)\,, \tag{146}$$

[but EULER does not consider the case of repeated roots. While he does not mention any general connection with vibration problems,] he uses (125) as the first example, obtaining [1])

$$y = Ce^{\frac{x}{K}} + De^{-\frac{x}{K}} + E\sin\frac{x}{K} + F\cos\frac{x}{K}\,. \tag{147}$$

1) On 9 December 1739 JOHN BERNOULLI reminds EULER that long ago he had introduced a similar notation for the exponential function and that he had solved certain equations of this kind. He will not accept EULER's method when there are complex roots; for example, for $k^4 \dfrac{d^4y}{dx^4} + y = 0$, k will be "impossible or not real." On 19 January 1740 EULER replies, "I fell upon my solution unexpectedly, nor before that had I any suspicion that the solution of algebraic equations could be so useful in this business." What is important is that EULER's method is *general*; he is not interested in solving "here one equation, there another," but of course he easily writes down the general solution of JOHN BERNOULLI's example. In reply to a further objection regarding imaginaries from BERNOULLI on 16 April, EULER on 20 June tries to pass off the whole matter by saying his method and BERNOULLI's are essentially the same. On 31 August BERNOULLI refuses to drop the subject: "I ask that you answer categorically, as is right among friends," whether the solution of the example is not wrong because the roots of $p^4 + k^4 = 0$ are "purely imaginary or impossible." On 18 October EULER says "I do not remember ever to have said that your method is not general enough . . ., but rather that it is inconvenient because the integral often involves imaginaries." As for EULER's solution of the special case, "I answer categorically . . . that it is right," and he goes on to explain the use of the formula $2\cos x = e^{+x\sqrt{-1}} + e^{-x\sqrt{-1}}$ for obtaining such solutions and reconciling them with others. The matter is mentioned again in JOHN BERNOULLI's letters of 18 February and 28 October 1741.

Apparently EULER informed DANIEL BERNOULLI of some of his results on the forced oscillator and on the solution of linear differential equations with constant coefficients. The relevant letters are lost or at least not presently available; DANIEL BERNOULLI sent EULER some comments[1]) which from their style appear to have been written for publication. On forced oscillations BERNOULLI gives his own method of obtaining the solution [but does not discuss the results or mention the phenomenon of resonance, though his solution is obviously invalid when $n = 1$]. As for vibrating bands, he says that the solution (147) in terms of circular and hyperbolic functions is due to EULER; [his qualification of EULER's method as "indirect" is just, since the "direct" methods, *i. e.* transformation and successive integration, when applicable, imply a proof of completeness, while EULER's method, since a uniqueness theorem had not been established, though indeed exhibiting a solution with a sufficient number of constants, did not show that no other solutions were possible]. After deriving a solution of $d^n s/dv^n = f^n s$, DANIEL BERNOULLI says "... this equation is plainly the same as yours, which you wrote out for me, without calculation or method. Although I had not thought about this problem before reading your letter, I cannot now say that I should have achieved the details therefrom, nor do I ask that you believe me. Meanwhile you will see from the following example [*i. e.*, the general case,] not the least vestige of which you supplied, that I am not straying from the right path. Nor was brought to my attention anything of what you write you have communicated to my father on this subject." For the general differential equation with constant coefficients, DANIEL BERNOULLI then obtains the general solution[2]), including the modification for the case when there are repeated roots.

[In evaluating older studies of vibration problems and also those that appeared in the next few years we must constantly remember that this simple method[3]) of solving the typical differential equations of the subject, the method that is now second nature, was not known.]

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1) "*Excerpta ex litteris a* DANIELE BERNOULLI *ad* LEONHARDUM EULER," Comm. acad. sci. Petrop. **13** (1741/1743), 1—15 (1751). In this undated paper DANIEL BERNOULLI says that he has not yet communicated his work on vibrating bands to the academy, though he had by this time confirmed his solutions by experiments. His two papers on this subject appear further on in the same volume and are described below.

2) Indeed EULER's solution is obvious; when he communicates it to CLAIRAUT on 31 October 1741, CLAIRAUT in his response of 4 January 1742 is easily able to provide a derivation. Often the obvious is not noticed.

3) EULER's finished exposition is given in E62, "*De integratione aequationum differentialium altiorum graduum,*" Misc. Berol. **7**, 193—242 (1743) = Opera omnia I **22**, 108—149. Presentation date: 6 September 1742.

26. The first differential equations of motion: JOHN BERNOULLI's and D'ALEMBERT's treatments of the hanging cord (1742—1743). DANIEL BERNOULLI's views on the subject of
small oscillations were meanwhile maturing. His *Remarks on composite oscillations, especially*
those that take place in bodies hung from a flexible thread[1]) begin by distinguishing *simple*
from compound oscillations. For the former, all parts of the system have the same period, 1
while for the latter, for example in the case of the bodies hung from a weightless cord, the
different parts have different [*i. e.* incommensurable] periods. But even for bodies whose 2
oscillations are compound in general, it is possible to assign a constant proportion to the
displacements in such a way that a simple oscillation results. "Moreover not only reason
but also very many experiments lead me to assert that composite oscillations always tend
more and more toward this state of uniformity and fall into it automatically, in some cases
more quickly and in others more slowly, in some indeed very quickly. Thus for example a
musical string cannot make unequal [*i. e.* non-periodic] vibrations unless it does so from
the start, such being the more non-regular the swifter they are, while the string once set
into vibration soon composes itself to the curvature necessary for isochronous motion."
The number of possible modes equals the number of bodies in the system; each mode yields 3
an isochronous motion, but for each of these the frequency is different. "But the state of
uniformity to which the oscillations of the body are most prone is that in which the oscil-
lations are the slowest possible."

[To understand this passage[2]), we must realize that BERNOULLI thinks that a general

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1) "*Commentationes de oscillationibus compositis praesertim iis quae fiunt in corporibus ex filo flexili suspensis*," Comm. acad. sci. Petrop. **12** (1740), 97—108 (1750). In his letter of 5 November 1740 to EULER, DANIEL BERNOULLI writes that this paper had been finished three months earlier.

2) In the previous year DANIEL BERNOULLI had expressed the same ideas less clearly. See § 14 of his "*De motibus oscillatoriis corporum humido insidentium*," Comm. acad. Petrop. **11** (1739), 100—115 (1750), where, after mentioning that "uniform and equable motions can occur in infinitely many ways," he goes on to say that "unless, however, the several bodies are brought out from the vertical line in the proper proportion, when they begin to move the oscillations will be irregular, inconstant, disturbed, but nevertheless they tend more and more to a state of uniformity. These remarks serve also for understanding the trembling motion of sounding strings: For the sound of one and the same string may be made up out of many tones."

EULER expresses this same view in §§ 29—30 of E159, cited below, p. 181. "Since a [flexible] body can move not only about the fixed axis O, from which it hangs, but also about any junction, . . . it can be disturbed from its state of equilibrium in innumerable ways; since all these . . . are equally possible initially, the resulting oscillatory motions will be very diverse . . . But the greater peculiarity . . . is that the several parts . . . do not simultaneously return to the position of equilibrium . . . In such motions, even though reciprocating, nevertheless the oscillations cannot be perceived distinctly, and therefore it will not be possible to employ the previous method, which assumes the existence of an isochronous simple pendulum. Nor indeed are the principles of mechanics yet sufficiently developed as to allow us to reduce to calculation . . . such irregular motions . . . But however much these oscilmotion of a string is not periodic. Moreover, he has not simply fallen into TAYLOR's old error (above, p. 131); whatever his "reason" may be, he says that *experiment* teaches him that a vibrating system quickly settles into its fundamental mode. In modern terms, this is an assertion that the higher modes are damped more severely than is the lowest mode. This may be true. If so, it furnishes some physical justification for confining attention, in a theory which neglects friction, to the fundamental mode or at least to the simple modes. In regard to the principles of mechanics, however, it is a retreat.]

4 "Similarly, a taut musical string can produce its isochronous tremblings in many ways and even according to theory infinitely many, though these are difficult to obtain, and moreover in each mode it emits a higher or lower note. The first and most natural mode is that when the string between oscillations produces a single arch; then it makes the slowest oscillations and gives out the deepest of all its possible tones, fundamental to all the rest. The next mode demands that the string between two oscillations produces two arches on the opposite sides, and then the oscillations are twice as fast, and now it gives out the octave of the fundamental sound." Higher modes are similarly described. [DANIEL BERNOULLI does not present a calculation of these results from theory, but it is plain that he has performed it (*cf.* above, p. 158). His earlier remarks (above, p. 158), combined with the foregoing digression from the subject of the present paper, make the only published basis for his later claim of priority for calculation of the higher modes of the vibrating string (below, p. 255)[1].]

DANIEL BERNOULLI here considers a heavy rigid rod of arbitrary line density suspended by a weightless rod linked to an arbitrary junction upon it. In Figure 58 m is now the point of junction, and the lower segment, which may extend above m, is the heavy rod. [BERNOULLI's analysis is now clear; instead of basing it on his own mechanical principle
8 of 1734 (above, p. 160),] he first balances horizontal forces acting on the rod and then balances moments about m. [Thus the method is essentially that of EULER's paper E 40
7 (above, p. 167).] The accelerating force, as usual, is taken as proportional to the displace-
9 ment. Thus result two equations, from which the constant of proportionality may be elimi-
9—10 nated, yielding a single quadratic equation for the length α in Figure 58. Since the point P
11 remains fixed, the values of α yield the proper frequencies for the two modes. BERNOULLI explains these modes clearly and explicitly, besides deriving the limit cases when $l/L = \infty$, $L/l = \infty$, or the point of suspension is the center of gravity of the rod.

lations are confused and irregular at first, experience shows that soon they subside into uniformity, so that all parts reach the configuration of equilibrium simultaneously and the oscillations, provided they be very small, may be compared with those of a simple pendulum."

1) MACLAURIN, writing some two years later, seems to be unaware of the existence of the higher modes; see § 929 of *op. cit. ante*, p. 150.

It is easy to extend these results to the case when the suspended mass is any rigid body: All one has to do is consider a rod of such a line weight that the center of gravity and the center of oscillation coincide with those of the given body. 14

Before he had seen this paper, EULER had already completed his own treatment of this problem and a class of others of its kind. His paper *On the oscillatory motion of flexible bodies*[1]) contains among other things a more straightforward analysis of the problem with which DANIEL BERNOULLI's paper ends. Taking moments about the junction, EULER 33—38
equates the moment of the weight to the moment of the restoring forces, thought of as acting at the center of inertia and calculated from (115). A second equation results from the balance of moments about the point of suspension. Hence follows a quadratic equation, whose solution gives the two proper frequencies.

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1) E159, "*De motu oscillatorio corporum flexibilium*," Comm. acad. Petrop. **13** (1741/3), 124—166 (1751) = Opera omnia II **10**, 132—164. Presentation date: 20 August 1742. In § 6 EULER observes that for a general restoring force $f(x)$ we have $f(x) = f(0) + xf'(0) + \cdots$, and hence for small oscillations about equilibrium $(f(0) = 0)$ a linear law of force results. *Cf.* the partly erroneous treatment of JOHN II BERNOULLI, p. 171 above.

Some of the subjects of the two papers described in the text above are discussed in the correspondence. On 12 April 1740 EULER writes "The problem which your Worship proposes [in a lost letter] regarding the oscillations of a body hung on ring is included among the oscillations of a heavy rope of non-uniform thickness, to the end of which a rigid body is attached, and thus it can be solved by the same method." On 30 April DANIEL BERNOULLI, perhaps without having received the foregoing, inquires "Have you also looked into the problem of the oscillations of a body hung on a flexible thread? In which case I should like to know if your solution agrees with mine; I have recently . . . sent it to you . . ." Again there is a gap in the correspondence. On 15 September EULER writes, "Your Worship's problem of the oscillation of a body hung on a weightless thread I have not originally taken into account with sufficient attention, but now the more I consider it the more important and useful I find it, since without it I should never have been able to determine correctly the oscillations of a sphere hung on a thread . . . I had to think a long time before I could apply my general method to that kind of oscillatory motion . . ." He goes on to explain the solution that we describe above from § 33 of his paper E159, leading to the same result as DANIEL BERNOULLI's. "Regarding these things please note that I have just now for the first time put them on paper, from which I see easily that I could have explained them much more distinctly, systematically, and briefly, wherefore please pardon this disorderly explanation." On 5 November BERNOULLI states that EULER's method and his own are virtually the same. On 7 March 1742 BERNOULLI implies that EULER has described a new work on the oscillations of bodies suspended by flexible threads. Presumably this is E159.

On pp. 121—123 of Notebook EH3 EULER writes an attempt, of course abortive, to treat the forces acting on linked bodies directly, without use of the tension. The idea seems to be that the actual forces are the same as those that would be sufficient to produce an accelerated motion in which the figure remains unchanged. This note, written probably in 1736—1738, helps us to perceive how great is the advance presented in E159.

A first attempt at the first problem solved in E159 is given in pp. 46—49 of Notebook EH4. Two methods are used, the second being that given in the finished paper.

41—43 To treat in the same way the oscillations of a heavy body suspended from a heavy flexible cord, EULER takes moments both about the junction and about an arbitrary point on the cord and cleverly derives the following equation for the form of the cord, generalizing $(108)_1$:

$$(W+\Sigma)\frac{d^2y}{dx}+\frac{d\Sigma}{dx}\left(\frac{dy}{dx}+\frac{y}{\alpha}\right)=0\,, \tag{148}$$

44—45 W being the weight of the suspended body. EULER gives the beginning of a series solution for the case of uniform line weight and discusses some approximations.

46—49 The same method applies to the case of two arbitrary rigid bodies linked together. There are two possible modes, in one of which the compound pendulum moves as a rigid
50—54 body. For three linked bodies, a cubic equation results. While EULER sees that an equation of degree n will result for the case of n links, the formal complications are too great, and he abandons the problem here.

The papers we have described were not published until 1750 and 1751. Priority in publication for the solution to the problem of a body swinging from a weightless rigid link belongs to old JOHN BERNOULLI, who included it in a miscellaneous collection of mechanical problems[1] he hastened into print[2] in his collected works in 1743. [The method is essentially that used in the above works by his son and by EULER. The solution is correct,] the two modes are obtained, [but there is no discussion of the mechanical significance of the results]. As for problems with a greater number of bodies, the same method will work, but the details he leaves "to those calculators who have plenty of time."

By this time DANIEL BERNOULLI's and EULER's analyses of the weighted hanging cord, written nearly a decade ago (above, pp. 155—164), were in print. It might seem super-

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1) "*De pendulo luxato, et de ejus reductione ad pendulum simplex isochronum*," Art. LVI of "*Propositiones variae mechanico-dynamicae*." Opera omnia 4, 253–386 (dated 1742, published 1743).

2) On 20 October 1742 DANIEL BERNOULLI writes to EULER, "The collected works of my father are being printed, and I have just learned that he has inserted, without any mention of me, the dynamical problems I first discovered and solved (such as *e. g.* the descent of a sphere on a moving triangle, the linked pendulum, the center of spontaneous rotation, *etc.*) . . . If it seemed necessary that I keep off the suspicion that I had plagiarized my father, I should have to justify myself. However, if your Worship thinks that my silence in the Academy at Petersburg would do no harm, it would not be distasteful to me. Before this Mr. BÜLFFINGER reproached me that I had gotten all from my father and done nothing by myself, but in fact I borrowed not a word from him." On 12 December 1742: "The problem of the motion of linked pendulums is so easy that neither the discovery nor the solution of it should bring much fame." On 4 September 1743, when he had finally seen his father's works in print: "The new mechanical problems are mostly mine, and my father saw my solutions before he solved the problems in his way . . ." It is of course entirely plain that priority of discovery for the problem of the linked pendulum belongs to DANIEL BERNOULLI and EULER independently.

fluous for John Bernoulli to issue his own solution[1]), the more so since in his typical style the old man does not mention that anyone else had ever treated the problem. When we examine his paper, however, just after the usual restriction to small isochrone vibrations we read, "but first we shall consider the matter generally." In Figure 60 let M_1 and M_2 be the masses at L and K, 314
let $HG = x_1$, $MI = x_2$, $GL = s_1$, $MK = s_2$. The condition that the length LK equals the length lk is $pl = ok = dx$, say. Then if $\varphi_1 = \angle NLF$ and $\varphi_2 = \angle oKk$, we have $dx = ds_1 \sin\varphi_1 = ds_2 \sin\varphi_2$. The force of the weight $M_1 g$ along lL is $M_1 g \dfrac{dx_1}{ds_1}$; the opposing force arising from the tension T in the link LK is $T \sin\varphi_1 = T\dfrac{dx}{ds_1}$. Hence the resultant accelerating force[2]) along lL is $M_1 g \dfrac{dx_1}{ds_1} - T\dfrac{dx}{ds_1}$. "Hence, therefore, by a dynamical principle"[3])

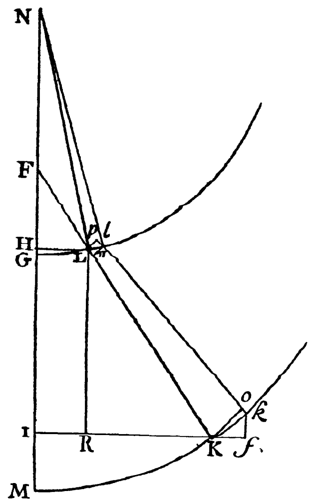


Figure 60.
John Bernoulli's notations for analysis of the motion of a string loaded by two weights

$$\frac{1}{M_1}\left[M_1 g \frac{dx_1}{ds_1} - T\frac{dx}{ds_1}\right] = v_1 \frac{dv_1}{ds_1}, \tag{149}$$

where v_1 is the velocity of M_1. Integration yields

$$\begin{aligned} g x_1 - \frac{1}{M_1}\int T\,dx &= \tfrac{1}{2}v_1^2, \\ g x_2 + \frac{1}{M_2}\int T\,dx &= \tfrac{1}{2}v_2^2, \end{aligned} \tag{150}$$

where the second equation follows in the same way. Solving for T yields

$$T = M_1 M_2 g \frac{d}{dx}\,\frac{x_1 ds_2^2 - x_2 ds_1^2}{M_1 ds_1^2 + M_2 ds_2^2}. \tag{151}$$

315

Balance of normal forces acting on M_2 yields

$$T\cos\varphi_2 = T\sqrt{1-\left(\frac{dx}{ds_2}\right)^2} = M_2 g\sqrt{1-\left(\frac{dx_2}{ds_2}\right)^2}. \tag{152}$$

Eliminating T between this result and (151), we have

$$M_2 \frac{x_1 ds_2^2 - x_2 ds_1^2}{M_1 ds_1^2 + M_2 ds_2^2} = \int\sqrt{\frac{ds_2^2 - dx_2^2}{ds_2^2 - dx^2}}\,dx. \tag{153}$$

1) "*De pendulis multifilibus*," Opera omnia 4, 313–331 (dated 1742, published 1743).

2) Perhaps by a slip, Bernoulli writes "vis acceleratrix" for what his formulae show to be "acceleratio".

3) "Ex Principio Dynamico" may mean "by the principle of dynamics."

While the remaining analysis concerns small isochronous motions [and hence leads to nothing beyond what Euler and Daniel Bernoulli had done long ago, in the foregoing we see *the first use of "Newton's second law"* to obtain complete differential equations of motion for a flexible body. Moreover, the equations are correct for finite motion, and this is *the first complete set of differential equations of motion for a deformable system.* It is a great advance in principle[1]).]

318—324 There follows a noteworthy attempt to treat systematically the small motion of a weightless hanging cord loaded by n equidistant and equal weights, but in the end only the case $n = 3$ is
320 worked out. This time John Bernoulli observes that it is easier to get equations such as (150) directly from the principle of live forces rather than from statics.

325—327 A second treatment rests on [Huygens'] observation, said to follow "from the nature of small oscillations", that the compound pendulum assumes the same form in conical oscillation as in lateral (*cf.* above, pp. 48—49). A simple direct solution then results by considering the conical case and *balancing the centrifugal forces against the tensions.* John Bernoulli works out in detail only the case $n = 3$, but he describes the general case, and here we shall put his words into equations. Figure 61 shows the forces to which the k^{th} weight is subject.

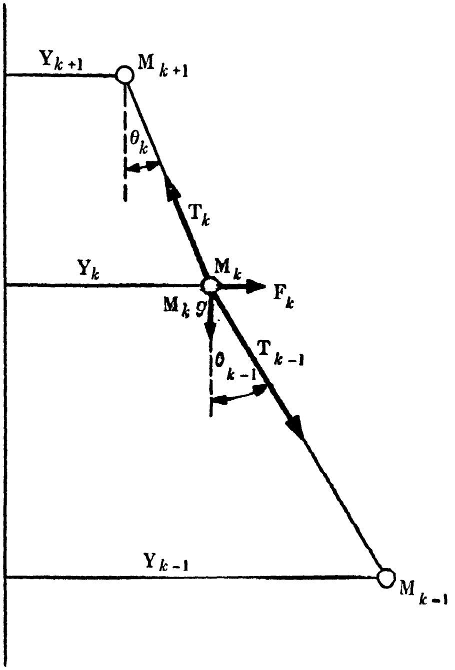


Figure 61.
Sketch realizing John Bernoulli's description of the equations for circular vibration of a string loaded by n weights

F_k is the centrifugal force, T_k is the tension in the k^{th} link from the bottom. Then, with the understanding that $T_0 \equiv 0$, the balance of vertical and horizontal forces gives

$$(154) \qquad \begin{aligned} F_k &= T_k \sin\varphi_k - T_{k-1}\sin\varphi_{k-1} \\ T_k\cos\varphi_k &= M_k g + T_{k-1}\cos\varphi_{k-1} \, . \end{aligned}$$

For a gyration of permanent form, the centrifugal forces F_k must satisfy

$$(155) \qquad F_k = M_k y_k \omega^2, \;\; \omega^2 = \text{const.}$$

John Bernoulli describes the foregoing balance of forces clearly but writes down equa-

1) For the parallel but more far-reaching improvement John Bernoulli achieved in hydrodynamics just afterward, see my Introduction to L. Euleri Opera omnia II 12, p. XXXVI.

tions only for the case of small motions, so that $\cos\varphi_k \approx 1$ and $(154)_2$ yields

$$T_k = g \sum_{q=1}^{k} M_q; \tag{156}$$

since $\sin\varphi_k = \frac{y_k - y_{k+1}}{a_k}$, where a_k is the length of the k^{th} link, $(154)_1$ becomes

$$\frac{M_k}{g} y_k \omega^2 = M_k \frac{y_k - y_{k+1}}{a_k} + \sum_{q=1}^{k-1} M_q \left[\frac{y_q - y_{q+1}}{a_q} + \frac{y_{q-1} - y_q}{a_{q-1}}\right]. \tag{157}$$

JOHN BERNOULLI carries out essentially the above calculation but always in terms of ratios, so that ω^2/g is eliminated all along. Thus there result $n-1$ quadratic equations for the unknowns y_k. The length of the equivalent simple pendulum is then the distance 328 from the bottom weight to the point where the extension of the last link meets the vertical: $\alpha = \frac{a_1 y_1}{y_1 - y_2}$.

[Thus in this second method appears *every element but one* necessary for an exact and general treatment: To find the equations of motion, the centrifugal force F_k should be replaced by the inertial force with components $-M_k \ddot{x}_k$, $-M_k \ddot{y}_k$.]

To obtain the equation for the continuous heavy cord, JOHN BERNOULLI balances the 329 centrifugal force against the weight on a finite section of the chain and so derives $(106)_1$.

Finally, JOHN BERNOULLI gives a third method, "the most natural of all". This con- 330—331 sists in calculating the accelerating forces as before but then equating them to a constant factor times the displacement. The result is the same as (157); the difference is only that here the lateral motions are treated directly.

[This is the last we shall hear of JOHN BERNOULLI, who died six years later at the age of eighty-one. The work just described, while in essence a revision[1]) is a remarkable achievement. The *first* method obtains the differential equations of finite motion for the compound pendulum, this being the *earliest example* of such equations for a non-rigid system; the work goes as far as the energy integrals for finite motion; but the generalization to n bodies is not really clear. The *second and third* methods, which are essentially the same as far as principle is concerned, introduce a *fixed rectangular Cartesian co-ordinate system* for the first time in problems concerning systems of any generality[2]). While they recall JOHN BERNOULLI's treatment of the loaded vibrating string, that analysis, like the later ones of DANIEL BERNOULLI and EULER on the present problem, used normal and tangential components and was not carried out sufficiently to obtain the full set of equations except for small motion. Here the principles are expressed

1) We recall that in younger days JOHN BERNOULLI was quick to issue elegant new proofs of results discovered at length by his brother JAMES.

2) For the importance of this step, see the discussion of the principles of mechanics in § 35.

so generally as to yield at once the correct general equations for the accelerating forces in the case of n bodies, but no differential equations are obtained since the special hypotheses replacing the general reaction of inertia are introduced from the start. A nearer miss would scarcely be possible. With some astonishment we see also that the old man is more skillful in marshalling and directing the different forms of the principles of mechanics than is his son or even EULER. On the other hand, he shows little interest in the nature of the solution and does not discuss the proper frequencies at all.

At the age of twenty-four, there enters our scene now a talented but sinister personality who is to make in six years[1]) a sequence of brilliant discoveries but thereafter will write endlessly in what seems today no more than a dogged attempt to confine the capacities of mathematics and to belittle the solid work of others.] This is D'ALEMBERT. The year before, he had communicated his famous *Principle* of dynamics to the French Academy. In 1743 appeared his *Treatise on Dynamics*[2]), in which the principle is applied to some problems not previously solved by other means. The first of these is that of the compound pendulum, or the cord loaded by discrete weights, [so that D'ALEMBERT shares with JOHN BERNOULLI the achievement of being *the first to obtain differential equations of motion for a constrained but non-rigid system*].

The "General principle for finding the motion of several bodies which react upon each other in any way" reads as follows[3]): "Let A, B, C, etc. be the bodies which constitute the system; suppose that the motions a, b, c, &c. be impressed upon them, but that they are forced because of their mutual reactions to change into the motions a, b, c, &c.... *Decompose each of the motions a, b, c, &c. impressed upon each body into two others:* a, α; b, β; c, $\varkappa$; *&c., which are such that if only the motions* a, b, c, *&c. had been impressed upon the bodies, they would have been able to retain these motions without interfering with one another; and if only the motions α, β, $\varkappa$, &c. had been impressed upon them, the system would have remained at rest.* It is clear that a, b, c will be the motions which these bodies will take on in virtue of their reaction."

[Generations of readers have been baffled by this statement, but it can be deciphered. D'ALEMBERT is a notorious schizograph: the elegant directness of his belles-lettres, often seen also in the prefaces to his scientific works, never enlightens the thick penumbra of

1) After his essay on fluid motion, finished in 1749, D'ALEMBERT's positive contributions to mechanics cease, except for one or two interesting details here and there in the voluminous polemic literature to which he devoted the rest of his scientific thought.

2) *Traité de Dynamique, dans lequel les loix de l'équilibre & du mouvement des corps sont réduites au plus petit nombre possible, & démontrées d'une manière nouvelle, & où l'on donne un principe général pour trouver le mouvement de plusieurs corps qui agissent les uns sur les autres, d'une manière quelconque,* Paris, David l'aîné, 1743. 2nd ed., 1758.

3) § 50 of *op. cit. ante*. In the second edition, the statement is entirely recast.

his mathematical exposition. But the obscurity is not of style only: D'ALEMBERT's language, both in analysis and in natural philosophy, is extraordinarly loose for his day. "Body" he uses as vaguely as had NEWTON; usually, but not always, it means the kind of body now called a mass-point, which had been defined precisely by EULER in 1734[1]). "Motion" he defines as velocity but mainly uses as differential increment of velocity; in most cases, to follow his argument we need to translate "motion" as "acceleration." "Force" he wishes to banish from mechanics as a concept *a priori;* he regards it as a phenomenon necessarily arising from change of motion experienced by mass,] and he *defines* it as the ratio of acceleration to mass[2]). However[3]), "I must give warning that to avoid circumlocutions, I have often made use of the obscure term *force*...;" [*i. e.*, he admits the usefulness of force in heuristic arguments, and it seems that in connection with the special problems solved for the first time in his book, he employs it freely, as we shall see. With this much instruction in his language, we can state his principle in modern terms[4]). For the acceleration $\boldsymbol{a}$ of any body of the system, we have

$$\boldsymbol{a} = \boldsymbol{a}_{\mathrm{f}} + \boldsymbol{a}_{\mathrm{c}} \,, \tag{157A}$$

where $\boldsymbol{a}_{\mathrm{f}}$ is the assigned acceleration, such as the acceleration of gravity, and $\boldsymbol{a}_{\mathrm{c}}$ is the acceleration arising from the mutual actions and constraints. D'ALEMBERT's principle asserts that *the forces corresponding to the accelerations* $\boldsymbol{a}_{\mathrm{c}}$ *form a system in static equilibrium*, i.e.,

$$\Sigma M \boldsymbol{a}_{\mathrm{c}} = 0 \,, \qquad \Sigma \boldsymbol{r} \times M \boldsymbol{a}_{\mathrm{c}} = 0 \,, \tag{157B}$$

where the sums are taken over the bodies constituting the system. Equivalently,

$$\Sigma M (\boldsymbol{a} - \boldsymbol{a}_{\mathrm{f}}) = 0 \,, \tag{157C}$$

$$\Sigma \boldsymbol{r} \times M (\boldsymbol{a} - \boldsymbol{a}_{\mathrm{f}}) = 0 \,. \tag{157D}$$

Be it expressly marked, however, that *no general equations* appear in the work of D'ALEMBERT, and that the above correspond to what he *does* rather than to what he

1) § 98 of E 15, *Mechanica sive motus scientiae analytice exposita* 1, Petropoli, 1736 = Opera Omnia II 1: "These laws of motion... belong properly to infinitely small bodies, which may be considered as points."

2) § 19 of *op. cit. ante*, footnote 2, p. 186.

3) *Ibid.*, preface, p. xxv.

4) A search of the literature has revealed but one author who states as "D'ALEMBERT's principle" the principle D'ALEMBERT himself published: this author is G. HAMEL, § 193 of *Elementare Mechanik*, Leipzig and Berlin, Teubner, 1912, and § 95 of *Theoretische Mechanik*, Berlin-Göttingen-Heidelberg, Springer, 1949. A form more or less equivalent to the original one is stated by LEVI-CIVITA & AMALDI, ¶ ¶ 18–19 of Ch. V of *Lezioni di Meccanica Razionale* 2_1, Bologna, 1926. A variant is called "the third form of D'ALEMBERT's Principle" by BOLTZMANN, § 72 of *Vorlesungen über die Principe der Mechanik* 1, Leipzig, Barth, 1897. All other works I have seen attach the name of D'ALEMBERT to one or both of the two principles described below, footnote 1, p. 191.

says. The modern reader, accustomed to NEWTONian concepts and conversant with the later formulations of the mechanics of systems, will interpret (157C) and (157D) as statements that *no total force or total torque is exerted by the constraints and mutual forces*[1]. This is far from D'ALEMBERT's own approach, but it suffices to show that while the principle is correct for most of the systems treated in analytical dynamics, it is insufficient, in general, to determine the motion of non-rigid systems without additional hypotheses or specializing features. Since, however, interpretation of "D'ALEMBERT's Principle" is a notorious pitfall for historians[2], I give mine only as conjectural. However obscure his statements and procedures, one thing is certain: D'ALEMBERT obtained new results of the greatest value.

The intricacy of his method[3] is best seen] in the example that makes it of interest here, that of the compound pendulum. "Let mu be the arc traversed in the first instant by the body m, and $M\,V$ [recte $M v$] the arc described in the same time by the body M. The body M may be regarded as having simultaneously two motions, one of which, $M\,V$, is equal and parallel to the motion mu of the body m, and the

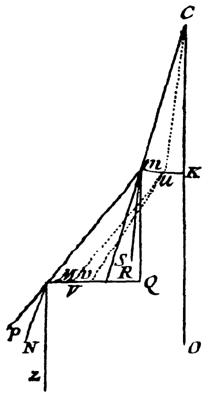


Figure 62. D'ALEMBERT's method of deriving differential equations of motion for the compound pendulum

1) Note the result derived by D'ALEMBERT in § 64: "The state of motion or of rest of the center of gravity of several bodies does not change at all by the mutual action of these bodies among themselves, provided that the system be entirely free; that is to say, that it not be subjected to motion about any fixed point." In § 63 he remarks that this generalizes a proposition of NEWTON (see below, p. 253, footnote 1). In § 66 he shows that under the action of arbitrary constant forces parallel to a fixed direction, the center of mass will describe the same curve as it would if the bodies had no mutual action. Thus D'ALEMBERT himself did not use his principle in so great a generality as in fact it enjoys.

2) Both in print and in conversation I have encountered many enthusiastic partisans of D'ALEMBERT, some of whom find themselves upset by my criticism of him. Not one, however, has named correctly any specific discovery by D'ALEMBERT, let alone following the argument by which he arrived at it. The two historians who studied some of his works in detail, TODHUNTER and BURKHARDT, found much to blame there and did not point out most of the specific achievements I have remarked and explained. His is one of those reputations, more numerous in our day than in his, that seem to need no fuel to keep on burning. So far as I know, the present essay and the Introductions to L. EULERI Opera Omnia, Vols. II 12 and 13, contain the only concrete study of D'ALEMBERT's works published in the last fifty years—the only attempt, that is, to give him the historical justice of fastening what he did rather than merely transmitting cozy generalities.

3) LAGRANGE, *op. cit. infra*, p. 190, remarked: "Thus in combining this principle with the ordinary principles of the equilibrium of the lever, or of the composition of forces, the equations of each problem may always be found by help of more or less complicated constructions ... but the difficulty of determining the forces which must be destroyed, as well as the laws of equilibrium among these forces, often renders the application clumsy and troublesome, and the solutions resulting from it are almost always longer than had they been derived from principles less simple and less direct."

other, Vv, is a circular motion around the center m or u. First we shall decompose the absolute effort of the weight of the body m along mQ into two others, one of which would be capable of making the body m traverse the line mu in the first instant, and the other is directed along the line mR, whose position is unknown. This last effort must be destroyed, since, by hypothesis, the body m can move only along mu. Likewise, we shall decompose the absolute effort of the weight of the body M along ML into two others, one of which would cause the body M to traverse the line MV, and the other, MN, may be decomposed again into two others, one of which would cause the body M to traverse the line Vv, but the other would be entirely destroyed, or, what amounts to the same thing, would be equilibrated by the effort along mR, which also must be annihilated. But for that it is necessary, 1°, that the effort of the body M which must be destroyed be directed along MP in the direction of mM extended, and, 2°, this effort be to the effort along mR as the infinitely small angle SmR ... is to the angle MmS, since for equilibrium it is necessary that the force resulting from the combination of these two efforts be directed along mS ...

"Let $Cm = l$, p the weight of the body m, P that of the body M, $Mm = L$, $mK = x$, $MQ = y$, φ the accelerating force along mu. Then, 1°, the force φ is to the weight p as the angle RmQ is to the sine of the right angle Rmu; ... thus $RmQ = = \frac{\varphi}{p}$. 2°, likewise the angle $NML = \frac{\varphi}{P}$. Thus the angle $PMN = \frac{y}{L} - \frac{\varphi}{P}$, and the accelerating force along $uv = P\left(\frac{y}{L} - \frac{\varphi}{P}\right)$. But the effort of the body M along MP, which differs but infinitely little from its effort along ML and thus may be expressed by $M \times P$, must be to the effort of the body m along $mR (m \times p)$ as the angle RmS or $\frac{x}{l} - \frac{\varphi}{p}$ is to the angle MmS or $\frac{y}{L} - \frac{x}{l}$. Hence $m \cdot \varphi = \frac{mpx}{l} - M \cdot P \times \left(\frac{y}{L} - \frac{x}{l}\right)$, and consequently $\varphi = \frac{px}{l} - \frac{M.P}{m}\left(\frac{y}{L} - \frac{x}{l}\right)$, and the effort along Vv will be $\frac{Py}{L} - \frac{px}{l} + \frac{M.P}{m}\left(\frac{y}{L} - \frac{x}{l}\right)$.

[Thus] ... one has these two equations:

$$
\text{(157E)} \qquad
\begin{aligned}
-\frac{d^2x}{dt^2} &= \frac{px}{l} - \frac{M.P}{m}\left(\frac{y}{L} - \frac{x}{l}\right),\\
-\frac{d^2y}{dt^2} &= \frac{yP}{L} \cdot \frac{M+m}{m} - \frac{x}{l} \cdot \left(p + \frac{M.P}{m}\right).\text{"}
\end{aligned}
$$

[After following this derivation, we are in a position to evaluate the principle itself, the originality of which has been the subject of controversy. D'ALEMBERT himself gave only references which seem designed to mislead[1]). LAGRANGE wrote a history of the subject which gives the right references but confuses the issue by a vagueness[2]) which has been transmitted faithfully or misinterpreted by later historians. In fact, what makes D'ALEMBERT's principle operative are two independent ideas: 1°, the product of the mass by the acceleration of a body, if reversed in sign, may be regarded as a force on a par with the applied forces; 2°, the forces exerted by the constraints need not be considered except insofar as they restrict the actual accelerations. This is not his language, but it explains his procedure. His emphasis is always put on the second. Both of these ideas, in the context of the center of oscillation and expressed in a still different terminology, derive from a great paper of JAMES BERNOULLI[3]) (1703). To solve the problem of the center of oscillation, JAMES BERNOULLI applied these ideas and the principle of equilibrium of moments; D'ALEMBERT's solution[4]) is, at bottom, identical. To solve the problem of the compound pendulum DANIEL BERNOULLI (above, p. 160) applied these ideas

1) His references concern only the applications, not the principle itself. In § 75, in regard to the center of oscillation, he gives an alternate form, in fact only trivially different from the former, but D'ALEMBERT writes, "The principle of this latter solution reduces to the same as that of Mr. [JAMES] BERNOULLI ... It is in this latter way that I had first thought of solving this problem, and this is also what Mr. EULER has done (in E 40, above, p. 168)... But Mr. EULER has not proved this principle at all, which, presented in this way, in fact, perhaps is not so easy to prove. In addition, the author has applied it in this same memoir to the solution of some problems concerning the oscillations of flexible or inflexible bodies." In the second edition, D'ALEMBERT characteristically altered this passage to read: "Mr. EULER has not proved this principle at all, nor can it be proved, it seems to me, except by means of ours. Moreover, the author applied this principle only to the solution of a small number of problems..., and the solution he has given of one of these problems is incorrect. This shows how much our principle is preferable for solving not only problems of this kind, but also all the questions of dynamics."

In § 100, in reference to the compound pendulum, and in §§ 105–106 to the problem of the cord carrying several weights, he mentions the *results* of DANIEL BERNOULLI and EULER (above, § 23) but says nothing about their *methods*.

2) LAGRANGE's history is in the Seconde Partie, Sect. 1ère of the *Méchanique Analitique*, 1788. The important sentence is: "We have already remarked that the principle employed by JAMES BERNOULLI in the study of the center of oscillation had the advantage of making this study depend upon the conditions of equilibrium of the lever, but it was reserved to Mr. D'ALEMBERT to envisage this principle in a general way and to give it all the simplicity and fecundity of which it could be susceptible." The unimportant references are to EULER's paper E 40, where JAMES BERNOULLI's idea is rather concealed than developed, and to HERMANN's *Phoronomia*, where, as LAGRANGE remarked in the second edition, nothing but a rearrangement of JAMES BERNOULLI's solution is to be found. The important omission is any mention of the work of DANIEL BERNOULLI.

3) "*Démonstration générale du centre de balancement ou d'oscillation, tirée de la nature du levier,*" letter of 13 March 1703, Mém. acad. sci. Paris, 1703, 4to ed., 78–84 (1705) = 12mo ed. (Amsterdam), 96–104 (1701) = Opera 2, 930—936. The importance of the method is emphasized in the Histoire for the same year, where the explanations and comments are longer than the paper itself.

4) *Traité de Dynamique*, § 73.

and the principle of equilibrium of forces; D'ALEMBERT's solution is, at bottom, identical in the calculation of the resultant accelerating forces but goes beyond BERNOULLI's in that it does not adopt the specializing hypothesis of TAYLOR, instead proceding to the general differential equations.

In summary, D'ALEMBERT's principle contains no ideas not to be found in the earlier work of JAMES and DANIEL BERNOULLI; rather, its merit is the perception that *those ideas are general and may be used to obtain differential equations of motion for a large class of dynamical systems.*

Finally, as should be clear from the above presentation, D'ALEMBERT's own principle is only tenuously connected with either of the statements traditionally called "D'ALEMBERT's Principle" in the literature of mechanics[1]).

After this long digression on D'ALEMBERT's method,] we return to consideration of his analysis of the weighted hanging cord. He remarks that DANIEL BERNOULLI's results

---

1) These are:

A. Equations of motion follow by adding to the applied forces per unit mass "inertial forces" defined as the negatives of the accelerations of the bodies on which the applied forces act. This principle does not eliminate the forces of constraint. Deriving, like D'ALEMBERT's own principle, from the great paper of JAMES BERNOULLI, it was put in general form by EULER (below, p. 253) and in his later life became his favorite tool for deriving equations of motion. The confusion of this principle with D'ALEMBERT's own was already current in 1811, for in the second edition of the *Mécanique Analytique* LAGRANGE added a paragraph, numbered ¶ 11, after the passage quoted above, p. 188: "If one wished to avoid the decompositions of motions which this principle demands, one would have only to assert immediately equilibrium between the forces and the motions engendered, but taken in the contrary directions. For, if one imagines that one impresses on each body, in the contrary sense, the motion it has to take, it is clear that the system will be reduced to rest. Hence it will be necessary that these motions destroy those which the bodies have received and which they would have followed except for their mutual actions. Thus there must be quilibrium among all these motions, or among the forces which can produce them. In truth, this manner of reducing the laws of dynamics to those of statics is less direct than that which results from the principle of D'ALEMBERT, but it presents more simplicity in applications. It goes back to that of HERMANN and EULER; the latter employed it in the solution of many problems of mechanics, and in some treatises of mechanics it is given the name of 'D'ALEMBERT's Principle'."

B. The principle of virtual work, if the applied forces are supplemented by "inertial forces," becomes the general law of dynamics. This principle, which has in common with D'ALEMBERT's own the elimination of forces of constraint, is due to LAGRANGE, *Méchanique Analitique*, Seconde Partie, Seconde Section, ¶ 7.

The term "inertial force" was used by D'ALEMBERT only in its old meaning, equivalent to the modern "inertia." On pp. ix–x of the *Traité de Dynamique* he writes, "The *force of inertia*, that is, the property that bodies have of persevering in their state of rest or motion, once established . . ." In 1734, in § 74 of E15, cited above, p. 187, EULER had written, "The force of inertia is that ability, innate in all bodies, of remaining at rest or of continuing motion uniformly along a straight line," and in § 76 he had attributed the word to KEPLER, who defined it more broadly and more vaguely.

(above, p. 156) follow from (157E) in the special case when the masses pass through the vertical simultaneously. For the general case, he gives a "construction" for the solution of (157E) [but no interpretation of the results, which are not recognized as combinations of trigonometric functions[1]). Hence there is no question of his establishing the position of DANIEL BERNOULLI's modes as the generators of all solutions by linear combination. Indeed, D'ALEMBERT, like JOHN BERNOULLI, shows no interest whatever in the physical aspects of the problem.] The same method yields differential equations for the small motion of a cord loaded by any number of weights. D'ALEMBERT then claims to prove, in effect, that there is always at least one real solution, [but the proof is elaborately false[2])].

Finally, D'ALEMBERT takes up the case of "a curve loaded by infinitely small weights, placed at infinitely short distances from each other." Remarking that DANIEL BERNOULLI has found the expression (113) for "the accelerating force of each little weight," D'ALEMBERT perceives that when the curve is not such that all its points cross the vertical simultaneously, "it will change in its equation from one instant to the next, and the general value of an ordinate y can be expressed only by a function of the arc s ... and of the time t ... In general, then, let $y = \varphi(t, s)$ ... Then

$$\frac{\partial^2 y}{\partial t^2} = \frac{\partial y}{\partial s} - (l - s)\frac{\partial^2 y}{\partial s^2}\text{."} \tag{157F}$$

Some manipulations of this equation lead to no definite result.

[Here, read off easily by combining a proper dynamical principle with a previously known statical theorem, we see a turning point in the whole history of mechanics: *the first general statement of the law of motion of a continuous medium,* namely, the partial differential equation for the heavy hanging cord.]

27. DANIEL BERNOULLI's definitive work on the transverse vibrations of bars (1740—1742). [We have seen that DANIEL BERNOULLI and EULER, independently, had obtained the differential equation (125) for the transverse oscillations of straight rods, and that EULER had solved this equation, first in series and later in the explicit form (147), and had calculated the general formula (136) for the frequencies as well as the numerical value (135) for the fundamental frequency when one end is clamped and the other end free.] By 1740, the work of EULER was in print, and in this year the subject of vibrations of elastic bands again enters the correspondence between him and DANIEL BERNOULLI. On 5 November

1) Some formal properties of the equations which the modern reader sees to be connected with trigonometric functions are given in §§ 102–103.

2) In § 109 the general problem is reduced satisfactorily to showing that the equation for the frequencies, *i.e.* (119), has one real root. In § 107 D'ALEMBERT has asserted that all its roots are real, but his argument amounts to asserting that "il est visible".

Daniel Bernoulli writes "I have also made experiments on the sounds of prismatic chimes such as are generally used in small carillons, and I think I have achieved this theory too." On 28 January 1741, "It is extraordinary that elastic bands give out different tones depending on how they are supported; that they have their nodes, upon which they must be supported in order to emit a clear tone, *etc.* Otherwise, these tones are indeed as the inverse squares of the lengths in bands of different length and similarly supported. But not only the ratio of sounds but also the absolute sound may be derived for a band of given length, weight, and elasticity . . .

"My thoughts on the shapes of elastic bands, which I wrote on paper only higgledy-piggledy and long ago at that, I have not yet been able to set in order. My first problem is on this subject: for a naturally straight elastic band bent into a given curve, to find the potential live force, or all the motion it can produce in its restitution. Then comes the question: to find the curve such that the elastic band when bent into it has the least potential live force. If you care to make any reflections on this subject, please communicate to me your opinion . . ."

Again on 20 December 1741 Daniel Bernoulli writes, "For some time I have used most of my time in working out the various sounds and other properties of elastic bands, which subject has given me the occasion of many beautiful, entirely new experiments (which agree most perfectly with my theory), and this theory can be extended to all sounding bodies, and especially to bells, as I conjecture. But only I haven't yet had the time to put any of my thoughts on paper." On the 20 October 1742, "A few months ago I sent an extensive and laborious piece [to Petersburg] on the sounds of free bands, where I have explained and worked out many remarkable physical phenomena. But for this a new physical theory was needed before I could apply mathematics." This description fits the second[1]) of the two pieces we now examine. At this same time, or shortly thereafter, Euler solved these same problems and several more. Euler's work, which was published sooner, will be described in § 29.

Daniel Bernoulli's *Physico-mathematical remarks on the vibrations and the sound of* 1
elastic bands[2]) begin with some rather sour remarks to the effect that "some years ago" he solved the problem "with the greatest success that could be hoped and indicated a sum-

1) According to a letter from Krafft to Euler on 12 January 1742, the first piece of Bernoulli, which could not be called "extensive and laborious", was received in Petersburg in September, 1741. Euler had left by this time; thus he learned of Daniel Bernoulli's work only through letters. Krafft remarks in detail on Bernoulli's repetition of material from Euler's paper E40; "but I must admit that I do not clearly grasp the principle that he [Bernoulli] assumes, while the method by which your Worship treats this problem I understand very well, up to the integration in series . . ."

2) "*De vibrationibus et sono laminarum elasticarum commentationes physico-mathematicae,*" Comm. acad. sci. Petrop. **13** (1741/1743), 105—120 (1751).

2 mary of it in two words to the famous Mr. EULER . . . Too great a generality of subject
(which many so love) detracts not a little from the elegance of the argument, nor does it
add any weight to the matter, but more often introduces I know not what ridiculous
3—4 element[1])." A uniform band built in at one end and free at the other is considered; under
the usual hypothesis that the restoring force is as the displacement, a [rather awkwardly
5 calculated] balance of moments leads to (125). To calculate the period, BERNOULLI com-
pares a typical element with a pendulum subject to the same restoring force; this yields
(131).

6 For the integration, there are two methods: "one by series, which I prefer for con-
venience of calculation, the other purely geometrical, which consists in absolute integration.
This latter I should not have attempted at all had I not learned first from the most per-
7, 10 spicacious Mr. EULER that he had it in his power." The results, by both methods, are
equivalent to (128), (129), and (134), [with the explicit form not very conveniently re-
duced]. The value BERNOULLI obtains for the solution of (134) is $\zeta \approx \sqrt{\frac{7}{2}}$, in substantial
agreement with (135).

9 To relate the absolute elasticity to measured quantities, BERNOULLI solves the prob-
lem he had proposed to EULER on 26 October 1735 (above, p. 170). His method, [easier
and less rigorous than EULER's,] is to linearize the *differential equation* of the elastica sub-
ject to terminal load P, obtaining

$$\mathcal{B}\frac{d^2y}{dx^2} = Px\,. \tag{158}$$

The solution is

$$y = (\tfrac{1}{3}l^3 - \tfrac{1}{2}l^2x + \tfrac{1}{6}x^3)\frac{P}{\mathcal{B}}\,, \tag{159}$$

where x is measured from the loaded end. Thus

$$\delta = y(0) = \tfrac{1}{3}l^3\frac{P}{\mathcal{B}}\,, \tag{160}$$

[a first approximation to EULER's solution (137). In (159) we recognize the first appearance
of what is now called the "engineering theory" of beams, *i. e.* the linearized theory of the
11 elastica.] From (160) and (136) follows $\nu = \frac{1}{7\pi}\sqrt{\frac{P}{30l\delta}}$. Experiments on "very long
rounded planks of uniform structure and thickness" confirm this result very well.

13 For taut strings, long before there was a theory giving the definite formula (75), the
proportion (10) was amply verified by experiment. For vibrating bands, no one has pub-

1) This remark can be directed at no one else than EULER; it is the first of a long sequence of such comments that DANIEL BERNOULLI is to make as he falls steadily behind in the general course of researches on mechanics.

lished a description of any kind[1]). BERNOULLI then describes his own experiments on 14—16
bands.

If we "look more closely" at (134), "we shall learn easily that infinitely many other 17
values for the quantity ζ may be determined" so as to satisfy all the equations of the problem exactly. To calculate these roots by series is "a most tedious labor", but by noticing that they rapidly increase in value we may put $\cosh \zeta = \infty$ and so obtain from (134) the simple equation[2]) $\cos \zeta = 0$; hence, approximately, the roots ζ_r are given by

$$(161) \qquad \zeta_r \approx \begin{cases} \sqrt{\frac{7}{2}} & \text{for } r = 1 \\ (2r-1)\cdot\frac{1}{2}\pi & \text{for } r = 2, 3, 4, \cdots; \end{cases}$$

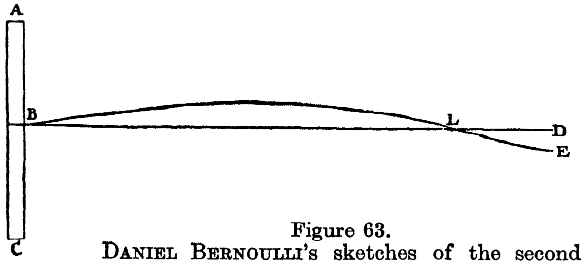
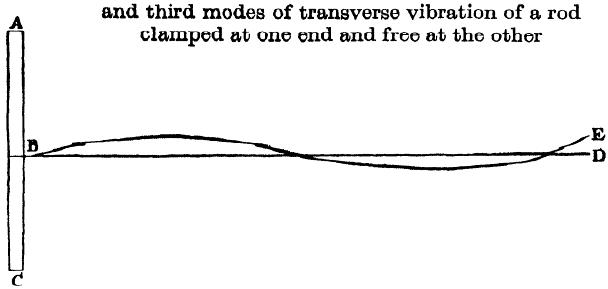


Figure 63.
DANIEL BERNOULLI's sketches of the second and third modes of transverse vibration of a rod clamped at one end and free at the other



the number of nodes is $r-1$. The 18
values (161) substituted into (136) give the frequencies for the various modes of isochronous vibration. The shapes corresponding to the second and third modes are illustrated by carefully drawn figures (Figure 63). Such modes occur also in the vibrations of musical strings and hanging chains, "as I have shown elsewhere and as takes place almost everywhere." [As a matter of fact, though BERNOULLI certainly knows the modes for the string, he has forgotten to write anything about them beyond passing remarks (above, pp. 158, 180). Thus, thirty years after the first mathematical analysis of the vibrating string, its proper frequencies and simple modes, so easy to calculate exactly, remain unpublished, while great effort has been put out to obtain approximate corresponding results for more complex vibrating systems.

Though BERNOULLI does not mention it here, the fact that, (161) being only approximate, the ratios ζ_r^2/ζ_1^2 and hence the ratios of frequencies ν_r/ν_1 as given by (136) are irrational, has an immediate bearing on musical theory: *The overtones of a sounding body*

1) Amazing as it may seem, this was true. Such meager work, both theoretical and experimental, as had been done before the first researches of DANIEL BERNOULLI and EULER in 1734 is described in §§ 6 and 16 above.

2) This is the sense of BERNOULLI's more awkward procedure.

need not be harmonious. On the basis of (161) and similar results for other vibrating bodies, this is to be remarked many times in later writings[1]), often specifically so as to controvert the view attributed to RAMEAU (above, p. 123).]

Shortly afterward DANIEL BERNOULLI wrote the more thorough *Mechanico-mathematical treatise on the manifold sounds that elastic bands give out in various ways, illustrated and confirmed by acoustic experiments*[2]). "The physicist must first think out the
I mechanical way in which the phenomenon . . . can occur, then from the most close connection between geometry and mechanics deduce the numerical values of the several effects and finally compare the calculated values with experimentally measured values. If these are in agreement, there results the highest degree of certainty possible in physics, and that not only in the effects confirmed by experiments but also in all other matters that follow from the theory by mathematical reasoning, even if these often are of such
II a nature as not to admit experimental test." I divide vibrations into two kinds, those slow enough that their frequencies can be measured, and those so swift as to be distinguished only by hearing the sounds they generate. The theory applies to both, but the experiments here reported concern audible sounds only.

III An elastic band can give out sounds of numerous kinds[3]). The "principal" ones are as follows :

Kind	End conditions
I	clamped-free
II	free-free
III	clamped-clamped
IV	pinned-pinned,

1) *E.g.*

a) DANIEL BERNOULLI (1753), the passage quoted on p. 256 below.

b) *idem* (1758), p. 159 of *op. cit. infra*, p. 262, with specific reference to RAMEAU: "... but the harmony of these tones [of the string] ... is only a sort of accident, since the tones of struck bars are not only dissonant but even incommensurable ..."

c) EULER (1759), Summarium and § 10 of E302, cited below, p. 330 (in reference to membranes and bells).

d) *idem* (1759), §§ 1—2 and § 17 of E303, cited below, p. 320 (in reference to bells and circular rods).

e) *idem* (1760), Summarium and §§ 1—2 of E 287, cited below, p. 302 (in reference to non-uniform strings).

f) DANIEL BERNOULLI (1771), § 11 *op. cit. infra*, p. 312 (in reference to two uniform strings joined together).

2) "*De sonis multifariis quos laminae elasticae diversimode edunt disquisitiones mechanico-geometricae experimentis acusticis illustratae et confirmatae,*" Comm. acad. sci. Petrop. **13** (1741/1743), 167—196 (1751).

3) Here I translate "modus" as "kind", reserving "mode" for uses where it agrees with the modern term. Later in this paper BERNOULLI uses "genus" in the sense he here uses "modus".

"and then there are numerous *mixed kinds.*" [BERNOULLI describes the above classifi-
cation in terms of the experimental circumstances rather than the mathematical end con-
ditions. We have expressed it in the terms we shall use henceforth in this history rather
than in translation from the original. Indeed, we have to look further on in the paper to X
see that $(132)_{2,3}$ are the end conditions applied for the second kind; BERNOULLI evidently XIX
grows tired and never treats the third and fourth kinds at all. From this paper, largely
repetitious of earlier work, we describe only the parts where something new appears.]

Regarding the first kind, studied in detail in the previous paper, "I took a chiming VII
needle almost one line thick and five inches long, firmly clamped in a wall; then I observed
that if the whole needle is drawn aside from its natural position, a very blunt sound results,
but if its free end is slightly pushed inward, there is generated a high sound at about the
fifth in the double octave of the first, as according to the theory. Moreover, both sounds
exist at once and are very distinctly perceived. I have said that often in this experiment VIII
both sounds exist together and are perceived, nor is it any wonder, since neither oscil-
lation helps or hinders the other . . ." [The earlier statements that overtones are some-
times heard *simultaneously with the fundamental*, mentioned above (pp. 32, 121), refer to
experience alone. Here we have just read the first vague statement of the principle of
coexistence of small harmonic oscillations according to theory; since the equations of motion
for vibrating systems are not yet known, an assertion of this kind must be *set down as a
principle*, not demonstrated as a theorem. In fact, BERNOULLI gives only the following intui-
tive reason.] "Indeed, when the band is curved by reason of one oscillation, it may always
be considered as straight in respect to another oscillation, since the oscillations are virtually
infinitely small. Therefore oscillations of any kind may occur, whether the band be destitute
of all other oscillation or executing others at the same time. In free bands, whose oscillations we shall now examine, I have often perceived three or four sounds at the same time."

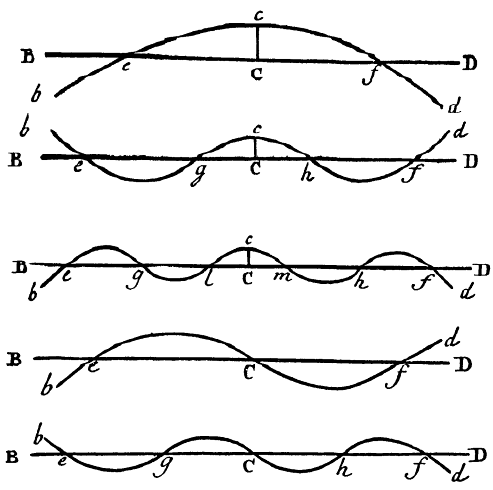


Figure 64. DANIEL BERNOULLI's sketches of the first five modes of transverse vibration of a free rod

For the second kind of oscillations, where X
both ends are free, it is plain that there are
modes in which the form is symmetrical about
the midpoint, where, consequently, the slope
is zero, as is shown in the first three drawings
in Figure 64. For these modes, either half of
the band may be regarded executing an
oscillation of the first kind. For these, then, XI
there are an even number of nodes, and all
the forms and sounds follow from the results

XII obtained for the first kind. But also there are modes with an odd number of nodes, as shown in the bottom two drawings in Figure 64.

XIII—XIV For the theory of free-free oscillations, take the origin $x = 0$ at the midpoint of the band. The solutions are then of the type

$$y = \left\{ \begin{array}{ll} A \cosh \dfrac{x}{K} + B \cos \dfrac{x}{K} & \text{even modes} \\ A \sinh \dfrac{x}{K} + B \sin \dfrac{x}{K} & \text{odd modes} \end{array} \right\}, \tag{162}$$

The corresponding equations for $\zeta = l/K$ are then

$$\begin{aligned} \tan \tfrac{1}{2}\zeta &= \tanh \tfrac{1}{2}\zeta, \\ \tan \tfrac{1}{2}\zeta &= -\tanh \tfrac{1}{2}\zeta, \end{aligned} \quad \text{respectively.} \tag{163}$$

When ζ is large, (163) may be approximated by $\tan \frac{1}{2}\zeta = \pm 1$, and hence for the even modes $\zeta \approx (4n - 1)\cdot\frac{1}{2}\pi$, while for the odd modes $\zeta \approx (4n - 3)\cdot\frac{1}{2}\pi$, where $n = 1$, 2, 3, . . . Only for the fundamental mode is this method inadequate; here a special calculation is needed. The final results for the frequencies of free-free vibrations are

$$\zeta_r \approx \begin{cases} 4{,}7213 & \text{for } r = 1 \\ (2r + 1)\cdot\frac{1}{2}\pi & \text{for } r = 2, 3, 4, \cdots; \end{cases} \tag{164}$$

the number of nodes is $r + 1$.

XV—XVIII There follows an approximate method for calculating the *nodal distances*, with a list of numerical results for the cases shown in Figure 64:

Mode	Frequency	Nodal ratios
1	6,345 (1)	0,220
2	17,627 (2,78)	0,131, 0,500
3	34,545 (5,44)	0,093, 0,356
4	57,105 (9,00)	0,073, 0,277, 0,500
5	86,308 (11,36)	0,060, 0,226, 0,409

The frequencies are given in multiples of the fundamental frequency of clamped-free vibration (*cf.* (135) and $(161)_1$); [in parentheses I have put the ratios of these same numbers to the fundamental frequency in the present case.] The nodal ratio is the fractional distance from the node to the nearest end.

The paper concludes with experiments verifying the calculated nodal distances and tones. [It is typical of researches for the next half century that after listing calculated values to the presumed accuracy shown in the table (where in fact there are errors in the last place of most entries),] for his experiments BERNOULLI considers it sufficient to hold the rod "with the tips of two fingers" and to check the frequency "by observing, as best I was able, the consonant sound on my harpsichord." [Here, as usual, DANIEL BERNOULLI's experiments are well conceived for demonstrating the *phenomenon* but carried out with little regard for the accuracy of measurement.] In a letter to EULER of 12 December 1742 BERNOULLI writes that to make a bar emit a clear tone, he holds it at a calculated node, while in the published paper he writes that he moves the support until the tone becomes clear. [The former method is the first example in our subject of the use of theory to facilitate experiment.]

28. EULER's treatise on elastic curves (1743). I. Static deflection. The letter of 20 October 1742, in which BERNOULLI mentioned to EULER the piece we have just described, concludes as follows : "I should like to know if your Worship could not solve the curvature of the elastic band in the case that a band of given length be fixed at two points, and thus that also its tangents at these points be given . . . This is the most general idea of the elastica, but in this case I have as yet found no solution except by the isoperimetric method, since I assume that the potential live force resident in an elastic band must be a minimum, as I once informed your Worship [*i. e.*, on 8 November 1738, see above, p. 174]. In this way I get a differential equation of fourth order, which I have not been able to reduce sufficiently to show that it is generally the usual equation of the elastica. Indeed I remember that before this your Worship and I have both doubted whether the ordinary equation of the elastica be general, with the argument that the circle is not included, although an elastic band manifestly can be bent into a circular curvature . . . Apart from this I have since noticed that the idea of my uncle Mr. JAMES BERNOULLI includes all elasticas . . .," whereupon what we should now call an applied couple is visualized as a force applied to the end of a rigid staff attached to the end of the band (*cf.* above, pp. 101—102 and Figure 35). To obtain the circular form, BERNOULLI supposes the staff infinitely long. "May your Worship reflect a little whether one could not deduce the curvature . . . directly from the principles of mechanics, without the intervention of the staff. In any case, for a naturally straight elastic band I express the potential live force of the curved band by $\int \frac{ds}{r^2}$, taking the element ds as constant . . . Since no one has perfected the isoperimetric method as much as you, you will easily solve this problem of rendering $\int \frac{ds}{r^2}$ a minimum." [Thus DANIEL BERNOULLI has introduced, in a special case, the *stored energy* of an elastic body and has proposed as the criterion of equilibrium that the *stored energy is extremal relative to compatible deformations*.]

On 12 December 1742 BERNOULLI writes, "I am glad you are so pleased with my principle for finding the elastica by the isoperimetric method. Indeed, I have solved the problem likewise but have not reduced the equation so far as to be able to see that it agrees with the general equation of the elastica (which also I found) . . . But I repeat that I assert neither to have proposed nor discovered anything worthy of attention, and if I had found everything by myself, I would not even claim anything if someone else believed on good faith he had found something before me[1])." The postscript to this letter suggests a printer for EULER's "Isoperimetric Treatise", and on 23 April BERNOULLI proposes that to this treatise be added the analysis of the elastica by the minimal principle. "I see easily that also the curvature of the chain and of the oscillating elastic band may be reduced to this [*i. e.*, to a variational principle], but I haven't yet thought out the way. Most mechanical curves will also be so reducible." EULER adopts the suggestion and within a few months has completed a timeless masterpiece in our subject, for on 4 September 1743 BERNOULLI writes that he has received EULER's "isoperimetric additions" and will forward them at once to the printer.

The *Addition on Elastic Curves*[2]) is a work indeed published as an appendix to a book on the calculus of variations, [but only tenuously connected to it, being in fact *the first treatise on any aspect of the mathematical theory of elasticity*. The opening section, besides explaining why variational principles were prized, is a magnificent declaration of the spirit of many mechanical researches in the late baroque period[3]).]

"All the greatest geometers have long since recognized that the method presented in this book is not only of the greatest use in analysis itself but also that it helps much in the solution of physical problems. For since the fabric of the universe is most perfect and the work of a most wise Creator, nothing at all takes place in the universe in which some rule of the maximum or minimum does not appear. Therefore there is no doubt whatever that

1) This whining tone in DANIEL BERNOULLI's letters steadily grows as his output declines. EULER took pains to cite DANIEL BERNOULLI's private letters repeatedly; DANIEL BERNOULLI rarely cited even the published papers of EULER. DANIEL BERNOULLI's claim, "My nature is certainly far distant from all jealousy . . ." (9 February 1743), gives the opposite impression; indeed, he shows repeatedly, often by disclaimers that scarcely ring true, the same proprietary spirit as his father and uncle, but unsupported by a like capacity for work.

2) "*Additamentum I de curvis elasticis,*" *Methodus inveniendi lineas curvas maximi minimive proprietate gaudentes,* Lausanne & Geneva, 1744 = Opera omnia I 24, 231—297. In the Opera omnia, §§ 91—92 are erroneously numbered 90 and 91. A German translation with helpful notes is given by H. LINSENBARTH, *Abhandlungen über das Gleichgewicht und die Schwingungen der ebenen elastischen Kurven*, OSTWALD's Klassiker der exakten Wissenschaften No. 175, Leipzig, 1910. A number of errors were noted and corrected by LINSENBARTH but overlooked in the reprinting of the original in the Opera omnia. An excellent English translation of EULER's work and an imperfect translation of LINSENBARTH's notes, along with the correction of one more error, are given by W. A. OLDFATHER, C. A. ELLIS, & D. M. BROWN, "*LEONHARD EULER's elastic curves,*" Isis **20**, 72—160 (1933).

3) *Cf.* EULER's later statement in § 4 of E145, cited below, p. 217.

all effects of the universe can be explained equally happily from final causes by the method of maxima and minima and from the effective causes themselves. There are such fine examples of this fact here and there that we scarcely need more to prove it; rather, what remains is but to find in each type of scientific question the quantity taking on the maximum or minimum value, a matter which seems to belong rather to natural science (philosophia) than to mathematics. Since then there are two ways of learning the effects of nature, the one through the effective causes, usually called the direct method, the other through the final causes, the mathematician uses each with equal success.

"If the effective causes are too hidden, while the final causes escape us less easily, the question is commonly solved by the indirect method; on the contrary, the direct method is brought to bear whenever the effect may be determined from the effective causes. But above all it is to be shown that each method lays open a road to the solution; thence not only does the one greatly strengthen the other, but also we take the highest pleasure in their agreement. Thus the curvature of the rope or hanging chain has been discovered by two methods, the one *a priori* from the loading due to gravity, the other by the method of maxima and minima, since it was recognized that the rope must take on such a curvature as to render its center of gravity the lowest possible. Similarly the curvature of rays through a transparent medium has been determined both *a priori* and from the principle that they must reach a given point in the shortest time. Many other such examples have been brought forward by the very famous BERNOULLIS and others . . .

"Nevertheless . . . the maximum or minimum is often hard to recognize, even if the solution has [already] been found *a priori*. Thus although the curved shape assumed by an elastic band has long been known, nevertheless the investigation of that curve by the method of maxima and minima, that is, by the final causes, has as yet been achieved by no one. Therefore, since the most famous and in this higher kind of natural science most perspicacious DANIEL BERNOULLI pointed out to me that the entire force stored in the curved elastic band may be expressed by a certain formula, which he calls the *potential force*, and that this expression must be a minimum in the elastic curve . . ., I cannot let pass this most desired opportunity of illustrating the usefulness of my method while also publishing this remarkable property of the elastic curve discovered by the very famous BERNOULLI. For this property involves second derivatives, so that the methods treated above for solving isoperimetric problems do not suffice to develop it."

[This passage is perplexing. First, a variational principle for the elastica had been asserted by JAMES BERNOULLI in his first paper (above, p. 95), is proved in Ch. I, § 76, and is generalized in Ch. VI, § 24, of EULER's book; it is this principle that shows at once the identity of the elastica and the lintearia. Moreover, in Ch. V, § 46, EULER had shown that *the elastica is the curve of given length between two points such that when revolved about* *the chord it generates the greatest volume.* Thus *two* variational principles for the elastica were known already and would seem to suffice for the general program laid down above. Presumably EULER laid greater importance on DANIEL BERNOULLI's because of the mechanical significance of the stored energy, but this he nowhere says. Second, long before, on 24 May 1738 DANIEL BERNOULLI had proposed to EULER the isoperimetric problem $\int r^m\, ds =$ extreme and on 8 November 1738 he had mentioned the special case (140) in reference to the elastica. Third, EULER had solved the problem in the general and special cases and had noticed that the solution of the latter leads to the rectangular elastica; these results are given in the paper E 99 (cited above, p. 172), awaiting publication when he wrote the treatise in 1742—1743. In the treatise itself he does not mention that problem, and why BERNOULLI's repetition in 1742 should have excited EULER's interest is a mystery[1]). Fourth, the formulae in the treatise are indeed insufficient to treat the problem, and EULER in effect writes out the solution without proof. Fifth, out of the 97 sections of the appendix only §§ 1—4 and §§ 41—43 concern the variational problem at all[2]). The remainder is an independent work on elastic bands, or, more properly, two works:

I. Determination of the finite static deflection of an elastic band under various loading (§§ 5—40, 44—62.)

II. Determination of the proper frequencies and simple modes of variously supported elastic bands in free infinitesimal vibration (§§ 63—97).

From EULER's notebooks we know that during the two years prior to DANIEL BERNOULLI's suggestion he had been working frequently on problems of these two kinds[3]). Apparently

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1) However, the fact is uncontestable. On pp. 346—348 of Notebook EH4 we read, "The famous DANIEL BERNOULLI communicated to me a new method for finding the elastic curve . . .," and then follows the analysis leading from (165) to (170).

Here I remark that CARATHÉODORY's "complete index" of EULER's variational problems (Opera omnia I 24, LVI—LXXII) contains no reference whatever to the *Addition on Elastic Curves* nor to the variational problem treated there. E 99 is cited only under No. 26, which is not an isoperimetric problem.

2) The problems of §§ 47—62 do not even fall within the scope of the variational principle.

3) The first relevant entries are on p. 481 of Notebook EH3 (1736—1740) and on pp. 52—54 of EH4 (1740—1744), where we find (91) in a different notation, followed by developments leading to (193). This material, which went into §§ 55—60 of the *Addition,* is closest to EULER's earlier work and furthest from the main contents of the treatise. Immediately following, on pp. 55—60 of Notebook EH4, we find some of the material on initially curved bands and the first attempt at classification of the bent forms of an initially straight band. Notebook EH4 contains many entries which will be cited below in connection with other passages of the treatise. As noted in footnote 1, the first entry referring to DANIEL BERNOULLI's letter of 20 October 1742 occurs hundreds of pages later than the notes for some parts of the *Addition.*

he seized the opportunity afforded by the book to get into print[1]) the great body of beautiful and important results he had obtained.

The contents of the first part of the *Addition* are entirely new. In the fifty years since the derivation of the differential equation of the elastica by JAMES BERNOULLI, scarcely anything had been learned concerning the shape of the curve[2]). EULER now determines and exhausts once and for all the shapes that an initially straight elastica subject to terminal load may assume, and also he solves most of the other elastic problems set by JAMES BERNOULLI.]

On the curvature of the uniform elastic band

According to DANIEL BERNOULLI, for an elastica naturally straight and of uniform 2
thickness, width, and elasticity, the problem to be solved is

$$\int \frac{ds}{r^2} = \text{Minimum}, \quad \begin{cases} \text{length fixed,} \\ \text{endpoints fixed,} \\ \text{slopes at endpoints fixed;} \end{cases} \tag{165}$$

it is plausible that this problem has a unique solution. Setting $p \equiv dy/dx$, $q \equiv dp/dx$, 3
we have $ds = dx\sqrt{1+p^2}$ and $ds/r^2 = Z dx$ with $Z = q^2/(1+p^2)^{\frac{5}{2}}$. EULER infers from (165) the differential condition

$$\frac{d^2Q}{dx^2} - \frac{dP}{dx} + \alpha \frac{d}{dx}\frac{P}{\sqrt{1+p^2}} = 0\,, \qquad Q \equiv \frac{\partial Z}{\partial q}\,, \qquad P \equiv \frac{\partial Z}{\partial p}\,, \tag{166}$$

where α is what has come to be called a "Lagrangean multiplier". One integration is immediate; since $P dp = dZ - Q dq$, another is easy, yielding

$$\alpha\sqrt{1+p^2} + \beta p + \gamma = Z - Qq\,, \tag{167}$$

where β and γ are constants of integration. Substituting the explicit forms of Z and Q and solving for q yields after rearrangement of constants

$$q = (1+p^2)^{5/4}\sqrt{\alpha\sqrt{1+p^2} + \beta p + \gamma} = \frac{dp}{dx}\,, \tag{168}$$

whence follows also an expression for dy/dp as a function of p only. While neither of the 4
quadratures $x = \int dp/q$ or $y = \int p\,dp/q$ is elementary, there is an intermediate integral:

$$\frac{2\sqrt{\alpha\sqrt{1+p^2} + \beta p + \gamma}}{(1+p^2)^{1/4}} = \beta x - \gamma y + \delta\,. \tag{169}$$

1) *Cf.* the complaints of DANIEL BERNOULLI, below, p. 254. At this time, papers contributed to the Petersburg Memoirs were delayed as much as eleven years.

2) The discussion of the quadratures by MACLAURIN, §§ 569, 927—928 of *op. cit. ante*, p. 150, adds nothing.

Rotation and translation of co-ordinates allows us to choose a system in which, in effect, $\gamma = 0$ and $\delta = 0$. By solving the resulting equation for p and again choosing new co-ordinates it follows that

$$(170)\qquad dy = \frac{(\alpha + \beta x + \gamma x^2)\,dx}{\sqrt{a^4 - (\alpha + \beta x + \gamma x^2)^2}}\,, \qquad ds = \frac{a^2\,dx}{\sqrt{a^4 - (\alpha + \beta x + \gamma x^2)^2}}\,,$$

where the constants have been renamed. [This result is equivalent to JAMES BERNOULLI's formulation (57).]

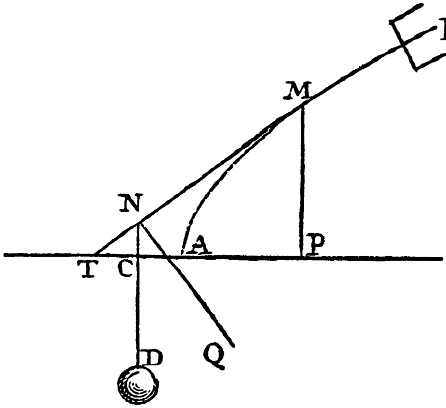

Figure 65. EULER's diagram for the elastica subject to terminal force and couple

5 The same is derivable directly; according to the conception of JAMES BERNOULLI [as recently rephrased by DANIEL BERNOULLI] (Figure 65), to the end A of the elastica is attached a rigid staff AC of length c, at the end of which acts the load CD of amount P. The origin of co-ordinates is A, with $AP = x$ and $PM = y$. In this notation, the mechanical hypothesis [as follows from (91) adjusted to allow for a couple] reads[1])

$$(171)\qquad \mathcal{M} = P(c + x) = \frac{\mathcal{B}}{r} = -\,\mathcal{B}\,\frac{d^2y}{dx^2}\left(\frac{dx}{ds}\right)^3.$$

6—7 Integration yields $(170)_1$, provided we put $P = -\,2\,\mathcal{B}\gamma/a^2$. EULER uses the notation Ek^2 for the bending modulus $\mathcal{B}$, noticing that Ek^2/a^2 is "equivalent to a pure force, and this force is determined from the elasticity of the band." [He appears to have forgotten his unpublished result (86), equivalent to (190).]

8—10 If we think of the part MB of the band as cut off and replaced by a rigid staff MN, tangent to the curve at M, while to the end A another staff AD is attached, tangent at A, so that both staves extend to the perpendicular NCD to the original staff AC, then forces of equal magnitude P but oppositely directed at the ends N and D suffice to maintain the
11—12 curvature of the band AM. Alternatively, to dispense with the staff, we may decompose the force P at D into components tangential and normal to the curve; the tangential component we may regard as acting at any point we please, such as A, while the normal com-

1) The analysis is given on p. 354 of Notebook EH4.

ponent may be replaced by a pair of oppositely directed normal forces p and q acting at any points we please, so long as the resultant force and moment are the same. When these quan- 13
tities are introduced into $(170)_1$, we easily show that when the load consists in "equal but oppositely directed forces" [*i. e.* a couple of moment hq], the equation becomes integrable, and the resulting form of the curve is a circle[1]) of radius $\mathcal{B}/(hq)$. Indeed, this follows by inspection of (171).

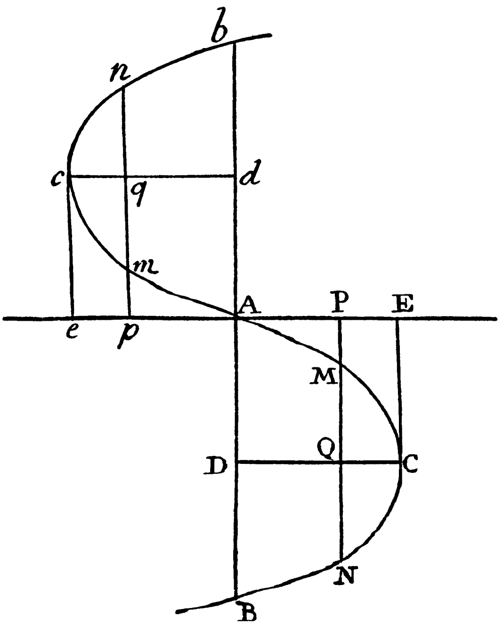

Figure 66. Co-ordinates used in EULER's treatment of the elastica, showing an elastic curve of the second class

The nine types of elastic curve

This classification exhausts the curves re- 14
presentable by the elliptic integrals following by quadrature of $(170)_1$; the analysis is carried out by determination of the critical points and of the behavior of the curve nearby, almost solely on the basis of the formula $(170)_1$ for the slope and almost completely without numerical or algebraic calculation[2]). A change of co- 15
ordinates reduces (170) to the form

$$(172)\qquad \frac{dy}{dx}=\frac{(a^2-c^2+x^2)}{\sqrt{(c^2-x^2)(2a^2-c^2+x^2)}}\,,\quad \frac{ds}{dx}=\frac{a^2}{\sqrt{(c^2-x^2)(2a^2-c^2+x^2)}}\,,$$

where the origin is now at A (Figure 66), the y axis ADB points positively downward in the direction of the force $P=2\mathcal{B}/a^2$, and the x axis is APE. The sine of the slope angle 16
at A is $1-c^2/a^2$. [If α is the angle MAD between the direction of the load and the tangent at the end A, then the result just stated by EULER may be written in the form

$$(173)\qquad \frac{c}{a}=\sqrt{2}\sin\tfrac{1}{2}\alpha=\frac{2}{a}\sqrt{\frac{\mathcal{B}}{P}}\sin\tfrac{1}{2}\alpha\,.$$

The quadratures of (172) may be expressed by elliptic functions, but we follow the simple and direct analysis of EULER.]

If $a=\infty$, the bending force vanishes, and the curve is a straight line; this is the *first kind* of elastic curve. More generally, if a^2 decreases, so does the slope of the curve at A, 17

1) Given on p. 354 of Notebook EH4.

2) On pp. 55—60 of Notebook EH4 two forms are found; the full classification is given on pp. 366—370.

to be expected since the force increases. If $a^2 = c^2$, the curve is tangent to the axis; if $a^2 < c^2$, the slope is opposite to that shown in the figure; if $a^2 = \frac{1}{2}c^2$, the tangent is vertical; while if $a^2 < \frac{1}{2}c^2$, the curve does not exist at A. The curve is an odd function
18 of x. Its maximum excursion is given by $x = \pm c$, since the tangents there are vertical and since for greater values of $|x|$ the slope is imaginary. This gives the form of the stretches Ac
19—20 and AC. To continue the curve past C, transform to new co-ordinates with C as a new origin. Since a parabolic form results, the part CNB is congruent to the part CMA, and thus knowledge of the part AMC determines the entire curve. [While the inference is not strict, the result is true. This is the first observation of the periodicity of the elliptic functions.]

21 The radius of curvature[1]) at x is $\frac{1}{2}a^2/x$. Thus at the point A it is infinite, while at C, the point of maximum excursion, it is greatest.

22—23 The ordinate y and the length s may be determined from the quadratures

$$y = \int \frac{(a^2 - z^2)\,dx}{z\sqrt{2a^2 - z^2}}, \qquad s = \int \frac{a^2\,dx}{z\sqrt{2a^2 - z^2}}, \tag{174}$$

24 where $z \equiv \sqrt{c^2 - x^2}$. For the point of maximum excursion, $x = c$, Euler evaluates these quadratures in series of powers of c^2/a^2:

$$\begin{aligned} AC = f = s(c) &= \frac{\pi a}{2\sqrt{2}}\left\{1 + \sum_{n=1}^{\infty} \frac{1}{2^n}\left[\frac{(2n-1)!!}{(2n)!!}\right]^2 \left(\frac{c}{a}\right)^{2n}\right\}, \\ AD = b = y(c) &= \frac{\pi a}{2\sqrt{2}}\left\{1 - \sum_{n=1}^{\infty} \frac{2n+1}{2^n(2n-1)}\left[\frac{(2n-1)!!}{(2n)!!}\right]^2 \left(\frac{c}{a}\right)^{2n}\right\}. \end{aligned} \tag{175}$$

[In later developments these formulae, which give relations for the *quarter periods* of all inflectional forms of the elastica, are of major importance. The meanings of the symbols are indicated for future reference in Figure 67, which is sketched for the case when $0 < \alpha < \frac{1}{2}\pi$, but the series (175) are convergent in the full range $0 \leqq \alpha \leqq \pi$, and for negative as well as positive b. In view of (173), the series (175) may be written in the forms

$$\begin{aligned} f &= \tfrac{1}{2}\pi\sqrt{\frac{\mathcal{B}}{P}}\left\{1 + \sum_{n=1}^{\infty}\left[\frac{(2n-1)!!}{(2n)!!}\right]^2 \sin^{2n}\tfrac{1}{2}\alpha\right\}, \\ b &= \tfrac{1}{2}\pi\sqrt{\frac{\mathcal{B}}{P}}\left\{1 - \sum_{n=1}^{\infty}\frac{2n+1}{2n-1}\left[\frac{(2n-1)!!}{(2n)!!}\right]^2 \sin^{2n}\tfrac{1}{2}\alpha\right\}.\,] \end{aligned} \tag{176}$$

Euler asserts [but does not prove] that when c and b are given, a may be calculated from $(175)_2$; hence AC is determined by $(175)_1$. Conversely, from the given length AC and from a, which is determined by the bending force, we may obtain c and b by (175).

1) This is easy to verify directly; Euler had already remarked it in Ch. 5, § 46 of the treatise.

To the first class of curves, which includes the straight line corresponding to $c = 0$, 25
should be assigned also the curves for which c/a is small. Since x^2 cannot exceed c^2, x^2 is also to be neglected in comparison with a^2, and $(172)_1$ reduces to

$$\frac{dy}{dx} = \frac{a}{\sqrt{2(c^2 - x^2)}}\,, \tag{177}$$

the solution of which is

$$x = c \sin \frac{y\sqrt{2}}{a}\,. \tag{178}$$

Since $f = AC \approx AD = \frac{a}{\sqrt{2}} \cdot \frac{1}{2}\pi$, "the force required to produce this infinitely small curvature of the band is a finite quantity,"

$$P = \tfrac{1}{4}\pi^2 \frac{\mathcal{B}}{f^2}\,. \tag{179}$$

"That is, if the ends A and B are tied together by a thread AB, then this thread is pulled

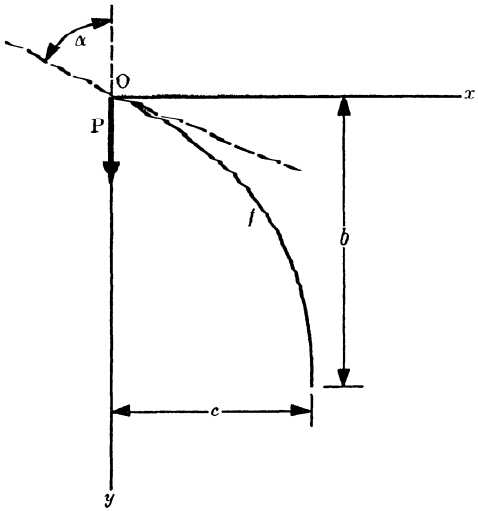

Figure 67. Quantities associated with the quarter period of an inflectional elastica

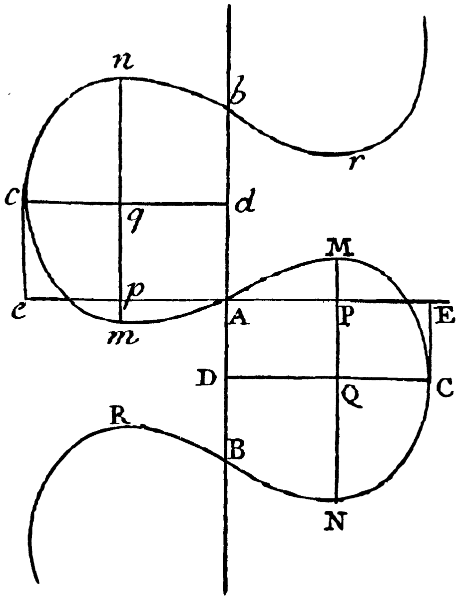

Figure 68. Elastic curve of the fourth class

by the force $\frac{1}{4}\pi^2\mathcal{B}/f^2$." [The first class thus consists in the forms for which the deflection is infinitely small; the form is as in Figure 66, with the curve being a sine curve of small amplitude.]

The second class is given by $0 < c < a$. The angle α is then less than a right angle, 26
and the form of the curve is as in Figure 66. Also $f > \pi a/(2\sqrt{2})$, and

$$P > \tfrac{1}{4}\pi^2 \frac{\mathcal{B}}{f^2}\,, \quad [\text{or} \quad P > P_c\,, \quad \text{where } P_c \equiv \tfrac{1}{4}\pi^2 \frac{\mathcal{B}}{f^2}\,. \tag{180}$$

This result, which will later appear to be of major importance, is barely noticed by EULER; it is an immediate consequence of $(176)_1$ and thus is valid for all inflectional elasticas.]

27 In the third class, defined by $a = c$, or $\alpha = \frac{1}{2}\pi$, the load is normal to the curve, yielding the rectangular elastica. "Although neither b nor f can be determined exactly as a function of a, I have shown elsewhere that a remarkable relation holds between these quantities", *viz* (141). EULER then calculates the numerical value $f/a = 1{,}311006$ [but makes a slip[1]) in calculating b/a; the just value is $b/a = 0{,}59896$. These results, as corrected, improve JAMES BERNOULLI's bounds (52).]

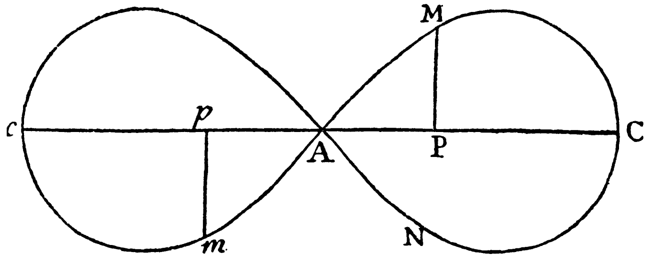

Figure 69. Elastic curve of the fifth class

28—29 We now consider the case when $c > a$, or $\alpha > \frac{1}{2}\pi$; equivalently, the angle PAM is positive. The fourth class is then defined by the condition $b > 0$; the fifth, by $b = 0$. Thus these two classes yield curves such as are shown in Figures 68 and 69, respectively. EULER remarks that in the fourth class "the humps m and R ... may not only touch one another but even may intersect ..." The limiting angle PAM, achieved in the fifth class, is found by putting $b = 0$ in (175) and solving numerically; the result is $\frac{1}{2}c^2/a^2 = 0{,}825934$, corresponding to $\alpha = 130°\ 41'$.

30 The sixth class is defined by the inequality $0{,}825934 < \frac{1}{2}c^2/a^2 < 1$. This gives a curve such as is shown in Figure 70; the angle α is now greater than $130°\ 41'$ but less than $180°$.

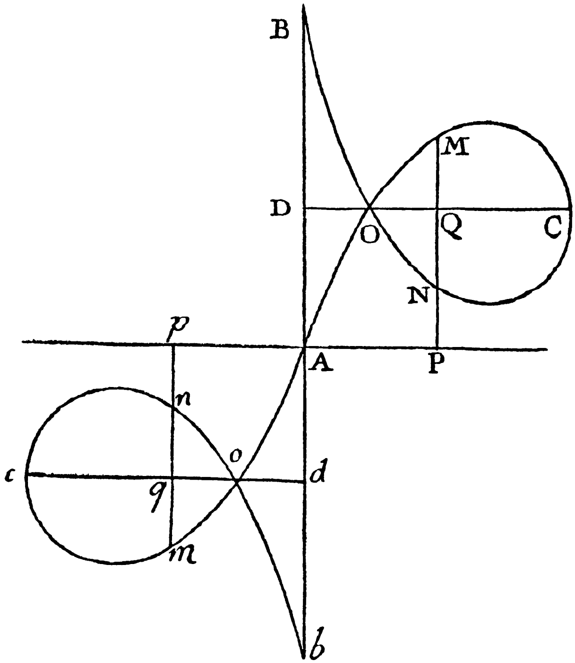

Figure 70. Elastic curve of the sixth class

31 The seventh class is the limit case when $c^2 = 2a^2$, so that formally a slope of $180°$ at the origin results. However, from (172) we now obtain

1) His result, uncorrected in the Opera omnia, is 0,834612. The correction is due to LINSENBARTH, *op. cit. ante*, p. 200.

$$\frac{dy}{dx} = \frac{a^2 - x^2}{x\sqrt{2a^2 - x^2}}\,, \tag{181}$$

whence it is easy to see that the curve does not pass through the origin, the y axis being in fact an asymptote. Since the series $(175)_1$ now diverges to ∞, in order that the length f be finite we must have $a = c = 0$, again yielding a straight form, but subject to infinite force. However, if the length f is infinite, we may integrate (181) and obtain

$$y = \sqrt{c^2 - x^2} - \tfrac{1}{2}c \log \frac{c + \sqrt{c^2 - x^2}}{x}\,. \tag{182}$$

To calculate the location of the double point O, we set $y = 0$. A clever numerical calculation yields $x/c = 0{,}288\,4191$ and for the angle QOM the value $56^\circ\ 28'$, showing that the angle at the double point is greater than for the fifth class. The angle at the double point in the sixth class lies between the values found for the fifth and seventh classes (Figure 71).

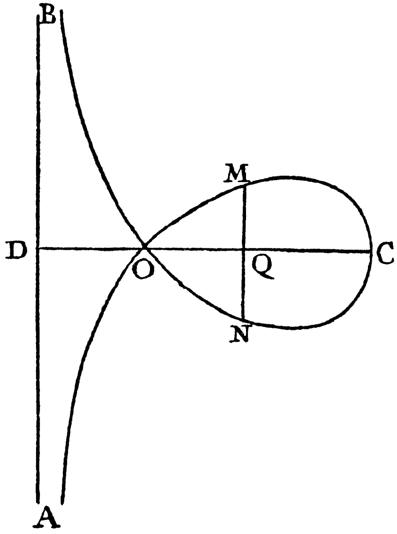

Figure 71.
Elastic curve of the seventh class

Finally, to treat the eighth class, when $c^2 > 2a^2$, 32
Euler sets $c^2 = 2a^2 + g^2$ in (172) and obtains

$$\frac{dy}{dx} = \frac{x^2 - \tfrac{1}{2}c^2 - \tfrac{1}{2}g^2}{\sqrt{(c^2 - x^2)(x^2 - g^2)}}\,. \tag{183}$$

The entire curve now lies between the lines $x = c$ and $x = g$, which are tangent to it (Figure 72). Since (183) remains unaltered when c and g are permuted, it makes no difference whether $g^2 < c^2$ or $g^2 > c^2$. There are no points of inflection. The angles at the double points are greater than in the previous case.

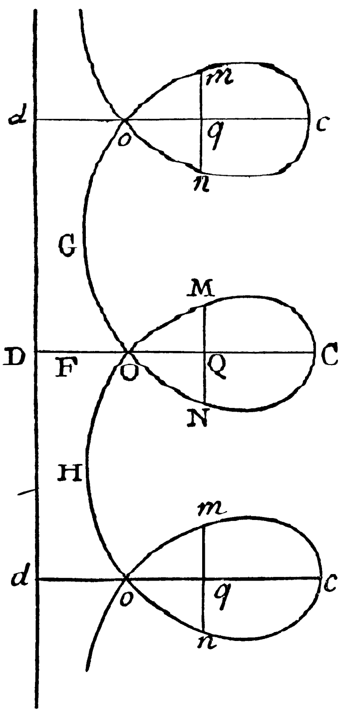

Figure 72.
Elastic curve of the eighth class

There remains the case $g = c$, defining the ninth 33
class. On the basis of the construction for the eighth class, the curve would vanish. If we regard c and g as both infinite, this does not follow. We may put $g = c - 2h$, $x = c - h + t$ and let c approach ∞; thus follows from (183) the equation $\dfrac{dy}{dt} = \dfrac{t}{\sqrt{h^2 - t^2}}$. Therefore the curve

of the ninth class is a circle. [The limit process is unnecessary and misleading : In (171), replace $P(c+x)$ by $L+Px$, allowing for a couple L independent of the force P; in the case when $P=0$, (171) is a differential equation for circles. *Cf.* what EULER put into § 13.]

34 "After the enumeration of these curves, in each case it is easy to specify the class to which the resulting curve belongs. Let the elastic band be built into a wall at G (Figure 73), and let a weight P hang at the end A, so that the band takes on the form GA. Construct the tangent AT, and the discrimination is possible by means of the angle TAP [*i. e.* the angle α] alone. If this angle is acute, the curve belongs to the second class ; if it is a right angle, to the third, that of the rectangular elastica. If the angle is obtuse but less than $130°\,41'$, the curve belongs to the fourth class, and to the fifth if the angle TAP equals $130°\,41'$. For a greater angle the curve belongs to the sixth class. It belongs to the seventh if that angle equals two right angles, which cannot occur in reality. This class, along with the last two, cannot be produced by applying a weight directly to the band."

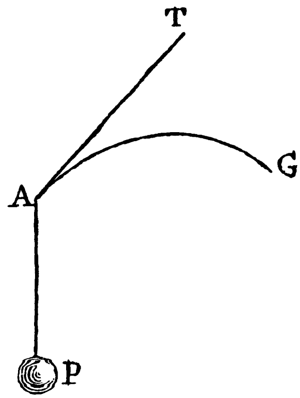

Figure 73. EULER's diagram for determination of the type of elastic curve from the applied load

35 To visualize the last classes, look again at Figure 65, where the weight P acts at the end of a rigid staff of length $AC=h$, and choose the origin at C rather than at A. An appropriate change of variable in (172) enables us to conclude that

$$c^2 = \frac{2(1+m)\mathcal{B}}{P} + h^2 \,, \tag{184}$$

36 where m is the sine of the angle MAP. "Therefore the curve belongs to the second kind if . . . $P < -2m\mathcal{B}/h^2$. Thus if the angle PAM is not negative, the force P must be negative, and the staff must be drawn upward at C. The curve belongs to the third class if $P = -2m\mathcal{B}/h^2$. The fourth class results if $2\beta\mathcal{B} > 2m\mathcal{B} + P/h^2 > 0$, where $\beta =$ 0,651 868. If $P = 2(\beta - m)\,\mathcal{B}/h^2$, the curve belongs to the fifth class. If

$$2(1-m)\,\mathcal{B} < P/h^2 < 2(\beta - m)\,\mathcal{B} \,,$$

the curve is of the sixth class. The seventh class results if $P/h^2 = 2(1-m)\mathcal{B}$, and the eighth if $P/h^2 > 2(1-m)\mathcal{B}$. If the angle PAM is a right angle, then $1-m=0$, and the curve belongs to the eighth class. Finally, the ninth class results if $h=\infty \cdots$"

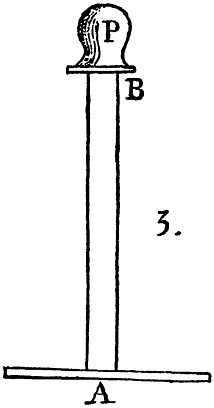

Figure 74. EULER's first diagram for the buckling problem

On the strength of columns

"What was noticed above concerning the first class can serve to determine the strength of columns. Let AB (Figure 74) be a vertical column standing on the base A, and let it bear the weight P. Let the column be so arranged that it cannot slip. If the weight P is not too great, then the most to be feared is a bending of the column. In this case the column may be regarded as endowed with elasticity. Let the absolute elasticity of the column be $\mathcal{B}$, its height be $2f = l = AB$. In § 25 we have seen that the force needed to bend the column ever so little is $\frac{\pi^2 \mathcal{B}}{4f^2} = \frac{\pi^2}{l^2}\mathcal{B}$. Therefore, unless the weight P to be supported satisfies 37

(185) $$P > \pi^2 \frac{\mathcal{B}}{l^2} \quad [\equiv P_c] ,$$

there is no fear of bending. If, on the other hand, P is greater, the column cannot resist bending. If the elasticity and thickness of the column remain constant, then the load P that it may bear without danger varies inversely as the square of the height. A column twice as high will thus be able to bear but the fourth part of the weight. This can be put to use especially for wooden columns, since they are subject to bending."

[EULER appears to have realized in retrospect the importance of what he had found in §§ 25—26. This is the first appearance of the celebrated "EULER buckling formula" or "EULER critical load", which will form the subject of several further researches in the eighteenth century before being forgotten during most of its "practical" successor[1]). In view of inaccurate statements by historians and unnecessary approximate theories given by later theorists down into the present century, we must pause long enough to fix precisely what EULER has proved. *First,* he has obtained (180) for *all inflectional elasticas;* the proof is *rigorous,* following at once from the exact formula $(176)_1$ and not resting in any way upon the linearized theory leading to (177) and (178). *Second,* since f is the length of the quarter period, the result (180) is valid for *all types of buckling* of a straight band by compressive terminal load. *Third,* EULER in taking $l = 2f$ has stated clearly that the corollary (185) is appropriate to the case when *both ends are pinned,* not clamped or free; his figure might mislead one into thinking the upper end is free. *Fourth,* EULER has determined exactly *all the bent forms possible,* and it is easy to write his results in terms of the ratio P/P_c, since by (180) we have $\mathcal{B} = 4f^2 P_c/\pi^2$, so that by (173) follows $\sin \frac{1}{2}\alpha = \frac{\pi c}{4f}\sqrt{\frac{P}{P_c}}$,

1) *E. g.,* PEARSON in § 102 of *op. cit. ante,* p. 11, refers to (185) as "this curious result".

and hence $(176)_1$ may be put into the form[1])

$$\sqrt{\frac{P}{P_c}} = 1 + \sum_{n=1}^{\infty} \left[\frac{(2n-1)!!}{(2n)!!}\right]^2 \left(\frac{\pi c}{4f}\sqrt{\frac{P}{P_c}}\right)^{2n} . \tag{186}$$

Reversion of this series determines c/f when P/P_c is given. Many approximate formulae purporting to yield such a result have been published in later times[2]). Still another alternative form of EULER's series $(175)_1$ is immediate from $(176)_1$ and the definition of P_c, *viz*

$$\begin{aligned} \sqrt{\frac{P}{P_c}} &= 1 + \sum_{k=1}^{\infty} \left[\frac{(2n-1)!!}{(2n)!!}\right]^2 \sin^{2n} \tfrac{1}{2}\alpha \ , \\ &= \frac{2}{\pi} K(\sin \tfrac{1}{2}\alpha) \ , \end{aligned} \tag{187}$$

where $K(k)$ is the complete elliptic integral of the first kind. It is essentially on the basis of $(187)_1$ that EULER gives his final description of the first six classes in § 34. EULER's classification by means of the values of α may be converted at once, by the aid of his series (187) and by use of numerical values given by him, into a classification in terms of P/P_c:

	I	II	III	IV	V	VI
α	0	0—90°	90°	90°—130° 41′	130° 41′	130° 41′—180°
$\frac{P}{P_c}$	<1	1—1,403	1,403	1,403—2,022	2,022	2,022—∞

Given P/P_c, we may determine $\sin \frac{1}{2}\alpha$ by inversion of (187); then

$$c/f = \frac{4}{\pi} \sin \tfrac{1}{2}\alpha \Big/ \sqrt{\frac{P}{P_c}} \ ,$$

and b/f follows at once from $(176)_2$. For completeness, we append a modern figure[3]) of the

1) This was observed by Е. Л. Николаи, *«О работах Эйлера по теории продольного изгиба,»* Ученые Записки Ленинградского Унив. 1939, No. 44, pp. 5—19, see §§ 4—5.

2) Some of these are criticized by v. MISES, "*Ausbiegung eines auf Knicken beanspruchten Stabes,*" Z. angew. Math. Mech. 4, 435—436 (1924). If we truncate EULER's series (186) after the first term, we obtain at once

$$\frac{c^2}{f^2} \approx \frac{64}{\pi^2} \frac{\sqrt{\frac{P}{P_c}} - 1}{\frac{P}{P_c}} \approx \frac{32}{\pi^2}\left(\frac{P}{P_c} - 1\right),$$

the result with which v. MISES emerges.

3) For precise drawings of numerous elastic curves and photographs of experiments in which they are realized, see the classic prize essay of M. BORN, *Untersuchungen über die Stabilität der elastischen Linie in Ebene und Raum, unter verschiedenen Grenzbedingungen,* Göttingen, 1906.

quarter periods and table of values for nine particular elastic curves from classes II, IV, and VI.

α	20°	40°	60°	80°	100°	120°	140°	160°	176°
P/P_c	1,015	1,063	1,152	1,293	1,518	1,884	2,541	4,029	9,116
b/f	0,970	0,881	0,741	0,560	0,349	0,123	−0,107	−0,340	−0,577
c/f	0,220	0,422	0,593	0,719	0,792	0,803	0,750	0,625	0,421

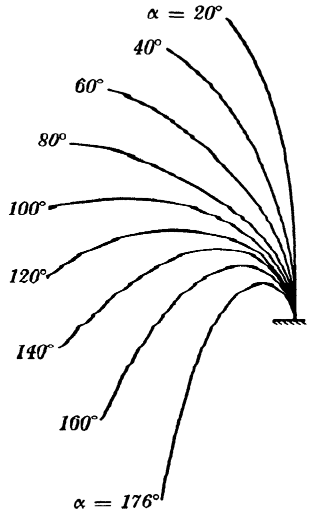

Figure 75.
Modern drawing of the quarter periods of inflectional elasticas

All the results just summarized are either given explicitly by EULER or are immediate corollaries of the formulae and numerical values he did obtain. That many of these results are often attributed to later authors may be due to the fact that EULER's work became more generally known through later publications in which he gave more verbal description but less mathematical detail. In particular, the later literature, including EULER's own subsequent papers, unfortunately emphasizes the misleading connection between buckling and the proper numbers of the linearized theory, insufficient to predict the magnitude of the bending. In the process of discovery presented here, the linearized theory in § 25 is but a brief interjection in the rigorous development.

It is strange that EULER did not remark that the dependence on length predicted by his formula (185) is that given by MUSSCHENBROEK's experimental law (94).]

Determination of the absolute elasticity by experiment

Approaching the same problem as that treated at the end of E 830 (above, p. 170), 38—39
EULER by approximate integration now obtains in place of (137) the formula[1])

(188) $$\mathcal{B} = \tfrac{1}{3}\frac{g^3 P}{\delta}\left(1 - \tfrac{3}{2}\frac{\delta}{g}\right),$$

where $g = x(l)$ is the length of the projection of the deformed band onto a line perpendicular to the direction of the load force and to the wall into which one end of the band is built.

1) As observed by LINSENBARTH, note 29 of *op. cit. ante*, p. 200, several formulae here, including that for the shape of the curve, are not right.

40 "The absolute elasticity $\mathcal{B}$ depends first upon the nature of the material from which the band is made. Second, it depends on the breadth of the band, so that, if all other things are kept constant, $\mathcal{B}$ should be proportional to the breadth. Third, the thickness of the band plays a great part in the determination of the value of $\mathcal{B}$, which seems to be proportional to the square of the thickness . . . Therefore experiments . . . can compare and determine the elasticities of all materials." [Thus EULER somewhat guardedly asserts that[1])

$$\mathcal{B} = GD^2B \ , \tag{189}$$

where G is a material constant having the dimensions [Force]/[Length]. This is inconsistent with his own unpublished theoretical result (86); however, a factor D^2B had been appearing in formulae for the strength of beams from GALILEO's time onward, and, in particular, putting (189) into (185) yields a result compatible with the formula (94) that MUSSCHENBROEK had inferred from his experiments on the collapse of columns. Of course (189) is wrong and (86) is right, at least for materials that fail in consequence of elastic bending. Fortunately EULER does not use (189), so his subsequent results are not affected by it, though later authors are often to point to it as one of his errors.]

On the curvature of a band which is not uniformly elastic

41—43 If we allow the absolute elasticity $\mathcal{B}$ to be a function of arc length, then the isoperimetric principle

$$\int \frac{\mathcal{B}\, ds}{r^2} = \text{Minimum}, \tag{190}$$

generalizing (165), yields the same differential equation as does the direct method based
44 upon the hypothesis (171). The absolute elasticity may then be measured directly from the
45—46 curvature by means of (171). As a specimen, the equations for a band of triangular plan and uniform thickness are set up and manipulated somewhat[2]).

On the curvature of elastic bodies that in their natural state are not straight

47—48 For a band which is initially curved with radius of curvature R, the hypothesis (171)
49 is to be replaced by (69)[3]). When the band is initially a circle of radius a, we thus have

$$P(c + x) = \mathcal{B}\left(\frac{1}{r} - \frac{1}{a}\right) \tag{190A}$$

1) Although "crassities" sometimes means "cross-sectional area", here it certainly means "depth" or "thickness", while "latitudo" means "breadth". These interpretations are made certain by EULER's usage in § 45, where he considers a beam of uniform thickness but triangular plan.

2) Even here the erroneous formula (189) does not affect the results, since D = const., and only B varies along the length of the band.

3) This result is given in EULER's first notes toward the present work, Notebook EH4, pp. 55—60, where are given also the integrals (192) and a description of the spiral curve they represent.

in place of (171); equivalently

$$P\left(c+\frac{\mathcal{B}}{aP}+x\right)=\frac{\mathcal{B}}{r}\,, \tag{191}$$

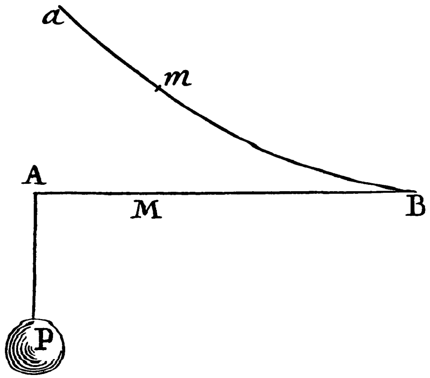

Figure 76. EULER's figure for determining the form of an elastic band that shall be straightened by an assigned load

so that the same bent forms are possible for a circular band as for a straight one, providing that in the circular case we visualize the force P acting at a distance $c+\mathcal{B}/(aP)$ rather than c. When $c=\infty$, the circular band can be bent straight. For a band 50
of any form, if $c=\infty$ the moment of the load is constant, and from (69) we see that the bent form may be calculated by quadratures.

We may also determine the form amB that a 51—54
band should have in order that a terminal load P acting normally to its tangent at the wall $\mathcal{B}$ shall deform it into the straight line AMB (Figure 76). Putting $AM=am=s$, from (69) we obtain [JAMES BERNOULLI's result] (68); the integral is given by

$$x=\int ds\sin\frac{s^2}{2a^2}\,,\qquad y=\int ds\cos\frac{s^2}{2a^2} \tag{192}$$

with $a^2\equiv\mathcal{B}/P$. The curve is thus a spiral closing down to a particular point as $s\to\infty$, but this point "seems very difficult to find." EULER is unable to effect these quadratures; the power series expansion are of no use for large values of s, and another series he obtains for $s=\infty$ is of no help. [Many years later[1]) he is to show that for $s=\infty$ we have $x=y=\frac{1}{2}a\sqrt{\pi}$.]

On the curvature of an elastic band subject
to arbitrary forces acting at its several points

The direct method leads to (91). The result (92) is generalized by 55—58, 59

$$rF_n+\int F_t ds=\mathcal{B}\left[\frac{1}{2r^2}-r\frac{d}{ds}\left(\frac{1}{r^2}\frac{dr}{ds}\right)\right]\,, \tag{193}$$

derived by manipulation of (91). From these formulae, all known results on perfectly 60
flexible strings follow easily by setting $\mathcal{B}=0$.

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1) §§ 125—135 (§§ 2—12 of the reprint) in E675, "*De valoribus integralium a termino variabilis $x=0$ usque ad $x=\infty$ extensorum,*" *Inst. calc. integral.* 4, 337—345 (1794) = Opera omnia I 19, 217—227. Presentation date: 30 April 1781.

On the curvature caused in an elastic band by its own weight

61 If the total weight of a uniform horizontal band of length l is W, the vertical load per unit length is W/l; differentiation of the appropriate special case of (91) yields

$$-\mathcal{B}\frac{dr}{r^2}=\frac{Ws}{l}dx\ , \tag{194}$$

and with $\int ds/r = u$ this becomes

$$\frac{l\mathcal{B}}{W}\frac{d^2u}{ds^2}=s\sin u\ , \tag{195}$$

62 but no further reduction is possible. If, however, we consider the form of a band loaded by the pressure n [per unit breadth] of a fluid at rest, from (91) we obtain

$$\frac{\mathcal{B}}{r}=P_y(c+x)-P_xy=\tfrac{1}{2}n(x^2+y^2)\,; \tag{196}$$

by a change of co-ordinates this becomes

$$x^2+y^2=A+\frac{\mathcal{B}}{r}\ . \tag{197}$$

In one case the integration is elementary.

[In all this, there is scarcely a line which does not shed a strange brilliance. It is the first treatise on finite elastic deflections, and the most extraordinarily successful ever, being the realization of JAMES BERNOULLI's program (above, pp. 95—96), proposed a half century earlier and still *untouched*.

What EULER gives us first is a golden analysis of the forms an elastic band may assume. It is a treatise on the nature of certain elliptic integrals in which scarcely any integrals are evaluated. After the declaration of faith in § 1, the style is unusually austere, as if in an effort toward conciseness. With astonishing ease, a few simple inequalities suffice to derive everything directly from the differential equation. The conclusions are always right and in most cases are really proved; here and there is an unobtrusive numerical calculation, and these would be still less noticeable had EULER checked his love of long decimals.

Immediately after the summarizing criteria of §§ 34—36, by which the appropriate one of the nine types of elastic curve is determined from the angle between the applied force and the tangent to the band at its end, EULER reminds us again of the first kind of curve and reads off, as a climax, the great buckling formula.

A major addition to elastic concepts comes in §§ 47—48 with the proposal that the *difference of curvatures* is to be the measure of strain in an initially curved band. While this was known to JAMES BERNOULLI and had been derived by EULER himself in E 831, here we encounter its first publication.

§ 51 contains a jewel rarely noticed by later writers: EULER's development of the *inverse problem* suggested by JAMES BERNOULLI. For the elastica, the inverse problem turns out to be mathematically easier than the direct one.]

In EULER's notebooks is stated an important problem on the elastic band which was not included in the treatise[1]): For an elastica of given form AMB, *to find the force and moment* that has to be applied at M if the part AM is cut away and the part MB is to retain its form. The solution, obtained by a complicated calculation, [is not important; in the problem we recognize *a further major step toward the concept of internal stress*, since here, for the first time, it is said explicitly that the resultant force exerted by one part of an elastic band upon its neighbor is generally not tangential. Thus we have the earliest occurrence of the concept of *shearing force*. This idea will be exploited many years later in one of EULER's finest contributions to our subject (§ 58).

EULER's inability to formulate in variational terms all the problems treated in the *Addition* becomes understandable when we remember that neither he nor DANIEL BERNOULLI had found the "potential live force" of an elastica except *a posteriori;* it was not yet known how to define the work done by a couple[2]).] EULER finally attained clarity in the course of his studies of the principle of least action. In his *Researches on the maxima and minima which occur in the actions of forces*[3]) he faces the difficulty inherent in varia-

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1) Pp. 355—356 of Notebook EH4, shortly before the classification of elastic curves.

2) From his letter of 4 September 1743 we learn also that DANIEL BERNOULLI did not have time to read the *Addition* before sending it to the printer. He suggests the principle (190) for the non-uniform elastica. Also "Bands not naturally straight require indeed another calculation, but no other method. If, however, the band is curved also by its own weight, then it is difficult to determine the maximum or minimum that pleases nature. I conjecture that here we have to seek a maximum maximorum, if a twofold consideration comes in." For a naturally straight band loaded by its own weight, he asks if among all isoperimetric curves with the same value of $\int ds/r^2$, the desired form results when the center of gravity is lowest. "We have both determined this curve directly; the question is, would the same curve result from this principle? . . . but I am not convinced of this principle . . ." On 25 December 1743 BERNOULLI says he is pleased that EULER is investigating the principle proposed. "I doubt if it can be shown *a priori* that the elastica must generate the greatest solid. I regard this as a property yielded by the calculation, one that nobody would have been able to predict from new principles . . ."

On 10 December 1745 EULER writes to MAUPERTUIS to the effect that (165) "flows very naturally from your principle," *i. e.* the principle of rest. The first results of this kind appear on p. 265 of notebook EH 5. On 8 May, 9 May, 4 June, 8 June, and 14 June 1748 EULER writes to MAUPERTUIS concerning the above-described variational principle for elastic and flexible lines. *E. g.*, on 8 June, "Above all I was transported with joy when I saw that the action of elasticity, which up to now was an insoluble knot for me, follows perfectly the same laws as the action of ordinary forces . . ."

3) E 145, "*Recherches sur les plus grands et plus petits qui se trouvent dans les actions des forces,*"

tional principles: Equations of equilibrium obtained by direct methods require specification of forces only in the actual configuration assumed, but in a variational treatment we have to specify the "action" in all possible configurations. Thus many different extremal principles can lead to the same result. Euler verifies that six different definitions of "action" lead to appropriate special cases of (91). Let $X(x)$ and $Y(y)$ be forces in the x and y directions, given as functions of x and of y, respectively, and let $V_k(v_k)$ be a force directed toward a fixed center P_k and given as a function of the distance v_k from it; let $\sigma(s)$ be the line density; then all of Euler's cases are subsumed under the single formula

$$\int \sigma\, ds \left(\int X\, dx + \int Y\, dy + \sum_{k=1}^{n} V_k\, dv_k + \frac{\mathcal{B}}{2r^2} \right) = \text{extreme}. \tag{197A}$$

The previously known variational principles for the catenary and the elastica are included. [What is really new is the factor $\frac{1}{2}$, which Euler tries to explain:]

"Thence it seems that the quantity of action of the elasticity is determined in a manner altogether different from that which serves for the true applied forces, since there is no likeness between the formulae

$$\int V\, dv \quad \text{and} \quad \frac{\mathcal{B}}{2r^2}.$$

Nevertheless the coefficient $\frac{1}{2}$ makes me conjecture that the quantity $\frac{\mathcal{B}}{2r^2}$ could have originated from an integral formula such as

$$\int \frac{\mathcal{B}}{r} \cdot d\left(\frac{1}{r}\right),$$

a formula which begins to look very like $\int V\, dv$." Euler then proceeds to develop the "analogy" between the two terms, finding that "the differential of the quantity $\frac{1}{r}$, taken negatively, represents the path which the force of elasticity causes the element Mm to traverse ..." From analysis of the infinitesimal motion arising from a moment he concludes that if $\mathcal{M}$ is any "force of elasticity" [*i. e.*, any moment], not necessarily $\mathcal{B}/r$, then its "quantity of action" is $\int ds \int \mathcal{M}\, d\varkappa$, where $\varkappa$ is the curvature. "Hence it is plain that this rule is precisely the same as that we have found for ... other forces ... Thus the rule which Mr. de Maupertuis has given ... is much more general than one would think, since it holds not only for all kinds of forces directed toward fixed centers but also for elastic forces, and there is no doubt that it is still more general."

[In the static case, Maupertuis' principle is no more than the principle of minimum

Mém. acad. sci. Berlin [4] (1748), 149—188 (1748) = Opera omnia II 5, 1—37. Presentation date: 19 December 1748.

potential energy, nowadays called "DIRICHLET's principle" but deriving, in special cases, from classical antiquity. In fact, what EULER has done here is to calculate *the work done by a couple*, antedating by forty years the work of LAGRANGE on this problem (below, pp. 409—410).]

29. EULER's treatise on elastic curves (1743). II. Vibrations. [While DANIEL BERNOULLI was carrying out the researches on the vibrations of bars we have described in § 27, EULER was working on the same problems, and he put his results into the second half of the *Addition*. All evidence indicates that the work of DANIEL BERNOULLI and EULER on elastic bands was independent; that BERNOULLI's was done somewhat earlier; that while BERNOULLI's was closer to experimental phenomena, EULER's was clearer, more accurate, and far more thorough[1]).

At § 63 begins a second treatise, [hardly related to the foregoing and evolved at a lesser tension]. The subject is the small vibration of an initially straight elastic band. In principle, there is no advance beyond the papers and letters described above[2]) except in numerical calculation of proper frequencies. EULER treats in detail the four kinds of oscillation mentioned by DANIEL BERNOULLI (above, p. 196) : I (§§ 69—79), II (§§ 80—90),

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1) EULER was in Berlin, BERNOULLI was in Basel. Although EULER was acting as virtual editor of the Petersburg Memoirs, ordinarily he first saw the contents in proof; BERNOULLI's papers described in § 27 appeared in print only in 1751. *Cf.* p. 166, above, and also footnote 4, p. 254, below.

In a letter of 19 June 1742 to CLAIRAUT, EULER explains the problem of the vibrating band and states that BERNOULLI and he had long ago derived (125), which he has now integrated; he gives the root $\zeta = 1{,}875104$ and the solution (147) with numerical values for the coefficients, adding "I do not know if Mr. BERNOULLI has pushed his researches this far..." Unfortunately, in the correspondence between BERNOULLI and EULER from this period EULER's side does not survive or at least is not presently available. From BERNOULLI's letters of 12 December 1742 and 9 February 1743 we learn that EULER had written some of his calculated values to BERNOULLI, who replies that his own methods for calculating proper frequencies do not yield the accuracy EULER claims: "Please indicate your method to me in a few words."

The results communicated to CLAIRAUT, and also the explicit frequency equation (134), are given on pp. 280—281 of Notebook EH4. Thus far all of EULER's results seem to be confined to the clamped-free modes. In the letter of 9 February 1743 BERNOULLI describes to EULER some of the results in his second memoir, which concerns mainly the free-free modes. These first appear on pp. 351—354 of Notebook EH4, where the treatment is rather awkward because the displacement of an end is left in all the formulae. The fundamental frequency is calculated to many figures and the results are compared, in the style of BERNOULLI, with those for clamped-free modes. The nodes of the fundamental mode of free-free vibration are calculated on pp. 384—385.

2) In the following account we do not describe material included also in DANIEL BERNOULLI's papers summarized above, almost certainly written earlier (*cf.* the foregoing footnote). These papers, however, did not appear until much later, and priority in publication belongs to EULER.

III (§§ 94—97), IV (§§ 91—93). The conditions defining a pinned[1]) end, [mentioned vaguely by DANIEL BERNOULLI,] here are stated explicitly :

$$y = 0\,, \quad \frac{d^2y}{dx^2} = 0\ . \tag{198}$$

82 For modes of the second kind, EULER includes both cases of (163) in the single equation

$$\cosh \zeta \cos \zeta - 1 = 0\ , \tag{199}$$

95, 92 which follows also for the third kind, while for the fourth kind

$$\sin \zeta = 0\ . \tag{200}$$

EULER proves that for each kind the equation of proper frequencies has an infinite number of roots, and thus an infinite number of distinct modes of isochronous vibration are possible. The modes are distinguished by the number of points, other than ends, where the curve crosses the axis ; each such point remains permanently on the axis and thus is a node.
76, 86, 90 In all cases the frequencies are given by (136) ; thus the law of proportion stated after (136) is extended to all four kinds of vibrations. "If two bands differ only in their lengths, . . . that twice as long will emit a tone lower by two octaves. However, a stretched string emits a tone only one octave lower. This shows clearly that the tones of elastic bands follow a thoroughly different law than do the tones of a stretched string." [Thus mathematical form is given to the long recognized difference between the responses of bodies grown elastic through tension and those naturally elastic ; *cf.* the remarks of MERSENNE, above, p. 31.]

70 EULER gives a great deal of attention to calculating the proper frequencies. For example, he replaces (134) by

$$e^{\zeta} = \frac{-1 \pm \sin \zeta}{\cos \zeta}\ . \tag{201}$$

72 Putting $\zeta = (2r+1)\cdot\frac{1}{2}\pi - \varphi$ yields $e^{\zeta} = \tan\frac{1}{2}\varphi$ or $\cot\frac{1}{2}\varphi$ for all non-negative integers r. Setting $\frac{1}{2}\varphi = \frac{1}{2}\pi - \frac{1}{2}\delta$ shows that the equations so obtained are pairwise identical. Hence follows the definitive system

$$\zeta_r = (2r+1)\cdot\tfrac{1}{2}\pi + (-1)^r\varphi = \log\cot\tfrac{1}{2}\varphi\ . \tag{202}$$

Each member is easily seen to be satisfied by one and only one acute angle φ_r, and as r
73—74, 78—79 increases, these roots approach 0. Numerical calculation for $r = 0$ and $r = 1$, followed

1) EULER's word is "fixus"; DANIEL BERNOULLI had said "extremitates . . . innituntur saltem." The term "simply supported" is often used nowadays. What we translate as "clamped" EULER calls "in muro firmiter infixus"; DANIEL BERNOULLI, "infixus".

by a rough estimate for greater values of r, yields the following sequence for clamped-free vibrations:

$$\zeta_r \approx \begin{cases} 1{,}8751040813\,, & r = 1\,, \\ 4{,}6940910795\,, & r = 2\,, \\ \frac{5}{2}\pi + \dfrac{1}{1 + \frac{1}{2}e^{\frac{5}{2}\pi}}\,, & r = 3\,, \\ \frac{7}{2}\pi - 2e^{-\frac{7}{2}\pi}\,, & r = 4\,, \\ (2r-1)\cdot\frac{1}{2}\pi\,, & r \geqq 5\,, \end{cases} \tag{203}$$

[improving (161)].

For modes of the second and third kinds, the counterpart of (202) is $\zeta = 0$ for 80—82
$r = 0$ and

$$\zeta_r = (2r-1)\cdot\tfrac{1}{2}\pi + (-1)^r\varphi = \log\cot\tfrac{1}{2}\varphi,\ r \geqq 1\,, \tag{204}$$

Hence EULER finds that $\zeta_1 = 4{,}7300350232$, but for the remaining values of ζ_r he gives 85, 90
only $(164)_2$.

For modes of the fourth kind, by (200) we have 92—93

$$\zeta_r = r\pi\,. \tag{205}$$

In discussing the free-free modes EULER notices that if $r = 1$ the moments acting 83
on the band do not add to zero; he then [fallaciously] assumes the same is true for all odd
values of r, which he therefore excludes. In his letter of 4 September 1743 DANIEL BERNOULLI expresses amazement at this passage: "These motions actually occur, and I have calculated various of their properties and even set up many beautiful experiments, agreeing beautifully with the theory, on the position of the nodes and the pitch of the tones." He then gives EULER some of the results from his paper awaiting publication, to which he suggests EULER refer. "I hesitate whether I ought not strike out the few words you say on
this subject[1])." The error here results in EULER's later denying a strict correspondence be- 96
tween the second and third kinds of vibration. Shortly afterward EULER published a note of rectification[2]).

EULER also determines in all cases the ratios of the constants A, B, C, D to the ampli-
tude $\mathfrak{A}$, both algebraically and numerically. For the first kind of vibration he obtains 75

1) These remarks are repeated in BERNOULLI's letters of 25 December 1743 and 4 February 1744. The piece from April or May 1744 printed by FUSS, p. 553 of *op. cit. ante*, p. 165, indicates that EULER by that time had seen the force of DANIEL BERNOULLI's objections, which are mentioned again in the letter of 29 August 1744.

2) E84, *Animadversio ad libri praecendentis paragraphum 83 et sequentes de curvis elasticis*, Nova acta erud. 1746, 92—95 = Opera omnia I 25, 81—83.

(206)
$$\frac{A}{\mathfrak{A}} = \frac{\tan \frac{1}{2}\varphi}{2(1+\tan\frac{1}{2}\varphi)}\,, \qquad \frac{B}{\mathfrak{A}} = \frac{1}{2(1+\tan\frac{1}{2}\varphi)}\,,$$
$$\frac{C}{\mathfrak{A}} = \frac{-1+\tan \frac{1}{2}\varphi}{2(1+\tan\frac{1}{2}\varphi)}\,, \qquad \frac{D}{\mathfrak{A}} = \frac{1+\tan\frac{1}{2}\varphi}{2(1+\tan\frac{1}{2}\varphi)} = \tfrac{1}{2}\,.$$

88 For the second kind,

(207)
$$\frac{2y}{\mathfrak{A}} = \frac{\cosh\frac{z}{K}}{\cosh\frac{l}{2K}} + \frac{\cos\frac{z}{K}}{\cos\frac{l}{2K}}\,,$$

89 where z is the co-ordinate from the midpoint. Putting $y = 0$ yields an equation for the nodes; in the lowest mode $(r = 2)$, the root is $\frac{z}{\frac{1}{2}l} = 0{,}551685$, corresponding to a nodal ratio of 0,22416. "Therefore this vibratory motion, which would be difficult to produce by a direct shock, can easily be effected. If the band is fastened at [the nodes], so determined,
90 it will continue to vibrate as if it were entirely free." A similar effect follows for the higher
92 modes if the band is fastened at only two of its nodes. In the fourth kind we obtain $y = \mathfrak{A}\sin\frac{x}{K}$; only for this case are the nodes equally spaced along the band.

97 The treatise closes by repeating the recommendation from E40 and E830 (above, pp. 169, 171) that the absolute elasticity be measured by comparing the frequency of oscillation with the known frequency of a consonant string.

[In the foregoing description we have omitted Euler's interpretations of the calculated frequencies as musical intervals, since by this date such interpretations had become straightforward.]

30. Euler's equations of finite motion for linked systems and for the continuous string (1744). [In the preceding sections we have learned that for vibrating rods and chains, though, curiously enough, not for strings except in verbal comments, the simple modes and proper frequencies were gradually reduced to calculation. While we now see at once how primitive were these theories, obtained without use of the equations of motion but only through a special device to eliminate the reaction of inertia, and thus offering no possible connection between the several modes, the successful calculations of results conforming with experiment might well have left little cause for dissatisfaction to the savants of the day.] Not so with Euler: Far from content, he saw exactly what was lacking. In 1744 he wrote[1]),

1) Introd. to E165, "*De motu corporum flexibilium,*" Comm. acad. sci. Petrop. **14** (1744/1746), 182—196 (1751) = Opera omnia I **10**, 165—176. Presentation date: 9 January 1744. This paper, obtaining the *equations of motion* for two linked bodies on a smooth table, seems to be a preliminary for E174. On 12 December 1742 Daniel Bernoulli writes to Euler, "The subject of linked rods may indeed become of great importance in your hands; since I did not perfect this matter but rested content with the first idea, I did not do much with it . . ." *Cf.* also Daniel Bernoulli's letter of 13 June 1744.

Similar problems, though not including models for flexible bodies, were solved by Clairaut,

"... even the first principles from which the motion of flexible bodies is to be determined remain unknown. Although indeed the most celebrated DANIEL BERNOULLI and I have happily explained the oscillatory motion of such bodies, nevertheless, since we considered only very small oscillations, we were able to do so without using ... the true principles..., since the principles of statics were sufficient."

To find the *true principles*, EULER turns again to discrete models : the weighted string and the chain with flexible joints, for which he now succeeds in establishing *the general and exact equations of motion in the plane.*

His paper, *On the motion of flexible bodies*[1]), [is twice formidable : First, lack of indicial notation results in an opaque mass of equations, and second, from lack of a clear mechanical objective EULER loads the development with trials of special cases offering no great interest. However, while less than a year before[2]) EULER had still approached this class of problems through the balance of angular momentum and other special devices, *here for the first time*] *the balance of linear momentum in a fixed rectangular co-ordinate system,* [*i. e.* "NEWTON's equations",] *is taken as the basic mechanical principle,* and all results are derived by integration of this system. [In other words, this paper introduces the now usual approach to problems of this kind. It is likely that EULER took his inspiration here (as in his hydrodynamical theory[3])) from JOHN BERNOULLI's work, just published, concerning special cases[4]). The contributions of JOHN BERNOULLI to mechanics, while few in number, are of great worth.]

"*Sur quelques principes qui donnent la solution d'un grand nombre de problèmes de dynamique,*" Mém. acad. sci. Paris 1742, 4to ed., Paris, 1—52 (1745), beginning, "Nearly all the problems I solve in this memoir were proposed to me by the learned Messrs. BERNOULLI and EULER." The only problem really close to the present subject is that of finding equations of motion for two linked bodies on a smooth table (§ XXXIII). CLAIRAUT's work is perhaps the first in which the principle of apparent forces in a rotating co-ordinate system is stated (§ I). As this fact suggests, he does not use rectangular Cartesian co-ordinates or succeed in finding equations of motion for bodies of many degrees of freedom.

On pp. 348—351 of Notebook EH4 is EULER's first attempt to treat the general motion of linked bodies. There he calculates the accelerating forces and torques but does not obtain differential equations of motion. *Cf.* the earlier attempt in EH3, described above, p. 181. On pp. 451—453 of EH4, EULER finally achieves the differential equations for three linked rods. On p. 454 follows an abortive attempt "to determine the motion of a uniform flexible thread cast arbitrarily upon a horizontal table." Soon thereafter, in passages to be cited below, are most of the results published in E174.

1) E174, "*De motu corporum flexibilium,*" *Opusculi* [*varii argumenti*] **3**, 88—165 (1751) = Opera omnia II **10**, 177—232. Presentation date: 5 November 1744.

2) In E165, cited above, p. 222.

3) See pp. XXXII—XXXIII, XLI of my Introduction to L. EULERI Opera omnia II **12**.

4) We recall the following earlier attempts:

a. TAYLOR in 1713 obtained the correct formula (74), one of the two equations governing finite motion of a continuous string, but did not write it or use it as a differential equation. The co-ordinates implied are intrinsic.

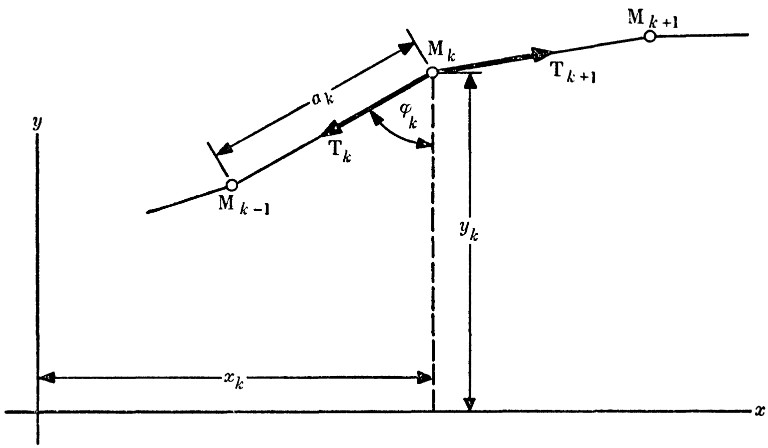

Figure 77. Variables used by EULER in obtaining the general equations of motion for a loaded string

In § 26 of EULER's paper we reach Problem 4: *To find the equations of motion of $n+1$ mass-points connected by rigid massless links and lying in a horizontal plane.* [This is the general problem usually called that of the loaded string.] In the previous sections EULER has derived the equations for $n = 1$ and $n = 2$. We present his general result, expressing his variables in simpler notation (Figure 77); *cf.* Figure 61 for similarities and differences. The rectangular Cartesian co-ordinates of the k^{th} mass M_k are x_k, y_k, the length of the k^{th} link is a_k, and its inclination from the line $x_k =$ const. is φ_k. Then we have the following equations of constraint:

$$x_k - x_{k-1} = a_k \sin\varphi_k, \quad y_k - y_{k-1} = a_k \cos\varphi_k, \quad k = 1, 2, \ldots, n\,. \tag{208}$$

The equations of motion are

$$\left.\begin{aligned} M_k \ddot{x}_k &= T_{k+1}\sin\varphi_{k+1} - T_k \sin\varphi_k, \\ M_k \ddot{y}_k &= T_{k+1}\cos\varphi_{k+1} - T_k \cos\varphi_k, \end{aligned}\right\} \quad k = 0, 1, \ldots, n \tag{209}$$

where T_k is the tension in the k^{th} link and $T_0 \equiv T_{n+1} \equiv 0$. [*Cf.* the earlier special case (78) and the partly more and partly less general (154).] Summing $(209)_1$ and $(209)_2$ on k yields

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b. For small motion of a weighted string, at least for a number of weights up to 6, JOHN BERNOULLI in 1727 had obtained the correct equations for the velocities according to the principle of live forces, but he did not write or use the result as a differential equation. The co-ordinates are rectangular Cartesian.

c. For the string loaded by two weights JOHN BERNOULLI in 1742—1743 had obtained the correct differential equations of finite motion but had used them only through the energy integrals (150). The co-ordinates are intrinsic.

d. For the small motion of a string loaded by n equal and equally spaced weights, JOHN BERNOULLI in 1742—1743 had described a process which leads to the correct and general differential equations provided one realizes that the centripetal acceleration $y_k\omega^2$ in (157) should be replaced by the acceleration $\ddot{y}_k$. The co-ordinates are rectangular Cartesian.

e. In 1743 D'ALEMBERT, using special resolutions of accelerations, had obtained the differential equations of small motion for the weighted string and for the uniformly heavy cord.

$$\sum_{k=0}^{n} M_k \ddot{x}_k = 0, \quad \sum_{k=0}^{n} M_k \ddot{y}_k = 0\,; \tag{210}$$

thus [the integrals of linear momentum] are

$$At + a = \sum_{k=0}^{n} M_k x_k, \quad Bt + b = \sum_{k=0}^{n} M_k y_k, \tag{211}$$

expressing the unaccelerated motion of the center of mass. By combining (209) and (208), EULER derives also [the equation of energy] in the form

$$\sum_{k=0}^{n} M_k (\dot{x}_k \ddot{x}_k + \dot{y}_k \ddot{y}_k) = 0\ . \tag{212}$$

While in the previous special cases he has used this equation to effect the solution, he does not apply it to the general case.

Rather, he uses (210) and (208) to express the co-ordinates in terms of the angles. First, we have by (208) the geometrical identity

$$\begin{aligned} \sum_{k=0}^{q} M_k x_k &= \sum_{k=0}^{q} M_k \left[x_0 + \sum_{r=1}^{k} a_r \sin \varphi_r\right]\ , \\ &= \mathfrak{M}_q x_0 + \sum_{r=1}^{q} a_r \sin \varphi_r \sum_{k=r}^{q} M_k\ , \\ &= \mathfrak{M}_q x_0 + \sum_{r=1}^{q} (\mathfrak{M}_q - \mathfrak{M}_{r-1}) a_r \sin \varphi_r\,, \end{aligned} \tag{213}$$

where $\mathfrak{M}_q = \sum_{m=0}^{q} M_q$. Putting $q = n$ and writing $\mathfrak{M} = \mathfrak{M}_n$ for the entire mass, by (211) we obtain

$$At + a = \mathfrak{M} x_0 + \sum_{r=1}^{n} (\mathfrak{M} - \mathfrak{M}_{r-1}) a_r \sin \varphi_r\ . \tag{214}$$

This equation expresses x_0 in terms of the angles φ_r; similar results hold for x_k, y_k; thus, in general, it is enough to know the *angles* φ_r as functions of time.

To obtain differential equations for the angles, first eliminate the tensions from (209).

$$\frac{\sin \varphi_{q+1}}{\cos \varphi_{q+1}} = \frac{\sum_{k=0}^{q} M_k \ddot{x}_k}{\sum_{r=0}^{q} M_r \ddot{y}_r}\ , \qquad q = 0, 1, \ldots, n-1\ . \tag{215}$$

Substituting for the sums their values as given by (213) and then replacing x_0 by its value as calculated from (214), we obtain[1])

$$\frac{\sin \varphi_{q+1}}{\cos \varphi_{q+1}} = \frac{(\mathfrak{M} - \mathfrak{M}_q) \sum_{r=1}^{q} M_{r-1} a_r \frac{d^2}{dt^2} \sin \varphi_r + \mathfrak{M}_q \sum_{r=q+1}^{n} (\mathfrak{M} - \mathfrak{M}_r) a_r \frac{d^2}{dt^2} \sin \varphi_r}{(\mathfrak{M} - \mathfrak{M}_q) \sum_{r=1}^{q} M_{r-1} a_r \frac{d^2}{dt^2} \cos \varphi_r + \mathfrak{M}_q \sum_{r=q+1}^{n} (\mathfrak{M} - \mathfrak{M}_r) a_r \frac{d^2}{dt^2} \cos \varphi_r}\,, \tag{216}$$

1) This result, as well as some special cases, occurs on pp. 454—457 of Notebook EH4.

This system of n differential equations for the n angles φ_r EULER regards as the definitive statement of the problem, [but he is unable to draw any conclusions from it].

Problem 5, in § 32, demands the corresponding equations for a continuous string by *passage to the limit* as the number of particles becomes infinite and the distance between them becomes zero. [To understand the difficulty of this problem] we must notice that EULER is studying *finite motion*, not infinitesimal vibration, [and that he apparently does not wish to make any hypothesis regarding the tension in the string.] Thus he chooses to pass to the limit not in (209) but in the various consequences from which the tensions T_k have been eliminated. Let s be arc length in a string of total length L, let $\Sigma(s)$ be the mass from 0 to s, let φ be the complement of the slope angle, so that $dx = ds \sin\varphi$, $dy = ds\cos\varphi$. A passage to the limit[1]) is then effected for a number of the results from Problem 4. [This is, moreover, the first example of a genuine and complete limit process of the type b 1 described in § 19, apart from HUYGENS' treatment of the catenary.] From (215) thus follows[2])

$$\frac{\sin\varphi}{\cos\varphi} = \frac{\dfrac{\partial^2}{\partial t^2}\int_0^s x\,d\Sigma}{\dfrac{\partial^2}{\partial t^2}\int_0^s y\,d\Sigma} \,. \tag{217}$$

The kinematical formula (213) yields

$$\begin{aligned}\int_0^s x\,d\Sigma &= \Sigma\left(x_0 + \int_0^s ds\sin\varphi\right) - \int_0^s \Sigma\,ds\sin\varphi \,,\\ &= \Sigma x_0 + \int_0^s d\Sigma\int_0^s ds\sin\varphi \,.\end{aligned} \tag{218}$$

Accordingly, if we set

$$\begin{aligned}U &\equiv \Sigma x_0 + \int_0^s d\,\Sigma\int_0^s ds\sin\varphi \,,\\ V &\equiv \Sigma y_0 + \int_0^s d\Sigma\int_0^s ds\cos\varphi \,,\end{aligned} \tag{219}$$

from (217) follows

$$\frac{\sin\varphi}{\cos\varphi} = \frac{\dfrac{\partial^2 U}{\partial t^2}}{\dfrac{\partial^2 V}{\partial t^2}} \,. \tag{220}$$

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1) The formulae for p and x on p. 205 of the reprint in the Opera omnia are not correctly copied from p. 201; from the right-hand sides should be subtracted

$$a\sin\zeta + b\sin\eta + c\sin\varphi + \cdots$$

and

$$a\cos\zeta + b\cos\eta + c\cos\varphi + \cdots \,,$$

respectively.

2) Part of the results here are given on p. 461 of Notebook EH4.

Directly from (219) we have

$$\frac{\partial^2 U}{\partial \Sigma^2} = \frac{ds}{d\Sigma}\sin\varphi\,, \qquad \frac{\partial^2 V}{\partial \Sigma^2} = \frac{ds}{d\Sigma}\cos\varphi\;. \tag{221}$$

Hence the system composed of (220) and (221) may be replaced by

$$\frac{\dfrac{\partial^2 U}{\partial t^2}}{\dfrac{\partial^2 V}{\partial t^2}} = \frac{\dfrac{\partial^2 U}{\partial \Sigma^2}}{\dfrac{\partial^2 V}{\partial \Sigma^2}}\,, \qquad \left(\frac{\partial^2 U}{\partial \Sigma^2}\right)^2 + \left(\frac{\partial^2 V}{\partial \Sigma^2}\right)^2 = \left(\frac{ds}{d\Sigma}\right)^2. \tag{222}$$

When a solution U, V of this system is known, the angle φ may be calculated from (220) or (221).

Beyond certain reductions in special cases, EULER is unable to draw other conclusions from (222) except that rigid motion is one possibility. [It is astonishing that at this time, when the partial differential equation for small motion of a string, while seemingly in the grasp of anyone, was not yet written down, EULER succeeds here in deriving a complete partial differential system governing *arbitrary finite motion*. It is clear also that after this splendid piece of virtuosity EULER has no idea what the equation (222) signifies or what might be done with it.] His only comment is, "Therefore the solution of this mechanical problem is reduced to an analytic problem; this must be agreed." [To find out what EULER really has accomplished, we write (219) in the form

$$U = \int_0^s x\,d\Sigma + U_0(t)\,, \quad V = \int_0^s y\,d\Sigma + V_0(t)\;. \tag{223}$$

Thus (220) is equivalent to

$$\frac{\partial x}{\partial s}\left[U_0'' + \int_0^s \frac{\partial^2 y}{\partial t^2}\,d\Sigma\right] = \frac{\partial y}{\partial s}\left[V_0'' + \int_0^s \frac{\partial^2 x}{\partial t^2}\,d\Sigma\right]\;. \tag{224}$$

Setting $s = 0$ shows that

$$U_0''\left.\frac{\partial x}{\partial s}\right|_{s=0} = V_0''\left.\frac{\partial y}{\partial s}\right|_{s=0} \equiv T_0(t)\left.\frac{\partial x}{\partial s}\right|_{s=0}\left.\frac{\partial y}{\partial s}\right|_{s=0}, \tag{225}$$

say. Thus we may write (224) in the form

$$\frac{\partial x}{\partial s}\left[T_0\left.\frac{\partial y}{\partial s}\right|_{s=0} + \int_0^s \frac{\partial^2 y}{\partial t^2}\,d\Sigma\right] = \frac{\partial y}{\partial s}\left[T_0\left.\frac{\partial x}{\partial s}\right|_{s=0} + \int_0^s \frac{\partial^2 x}{\partial t^2}\,d\Sigma\right] \equiv T\frac{\partial x}{\partial s}\,\frac{\partial y}{\partial s}\,, \tag{226}$$

say. Differentiation with respect to s yields

$$\sigma\frac{\partial^2 y}{\partial t^2} = \frac{\partial}{\partial s}\left(T\,\frac{\partial y}{\partial s}\right), \qquad \sigma\frac{\partial^2 x}{\partial t^2} = \frac{\partial}{\partial s}\left(T\,\frac{\partial x}{\partial s}\right), \tag{227}$$

while $(222)_2$ takes the form

$$\left(\frac{\partial x}{\partial s}\right)^2 + \left(\frac{\partial y}{\partial s}\right)^2 = 1\;. \tag{228}$$

This is the correct system governing finite plane motion for a string with line density $\sigma = d\Sigma/ds$. (It is to be derived, in precisely this form, by EULER in about 1750; see below, p. 254.) From (228) and (226) we obtain the following expression for the tension T:

$$T = \frac{\partial x}{\partial s}\left[T_0 \frac{\partial x}{\partial s}\bigg|_{s=0} + \int_0^s \frac{\partial^2 x}{\partial t^2} d\Sigma\right] + \frac{\partial y}{\partial s}\left[T_0 \frac{\partial y}{\partial s}\bigg|_{s=0} + \int_0^s \frac{\partial^2 y}{\partial t^2} d\Sigma\right] . \tag{229}$$

For small motion we have $x \approx s$ and $\frac{\partial y}{\partial s} << 1$, so that (229) yields $T = T_0$, and thus $(227)_2$ is satisfied, while $(227)_1$ reduces to the now familiar but then unknown equation

$$\sigma \frac{\partial^2 y}{\partial t^2} = T \frac{\partial^2 y}{\partial x^2} .$$

That EULER failed to see this connection shows the lack of sufficient *general* principles of mechanics at this date. EULER did not yet know *what was wanted* as the mathematical statement of the mechanical configuration of a continuous body. While indeed the original statements (208) and (209) express the most concise and general mechanical principles for the *discrete* case, the brilliant analysis of the continuous string seems to arise from formal insight alone.]

The paper concludes with Problem 7 in § 48: *To find the equations of motion of* $n+1$ *jointed rigid bodies in a horizontal plane*[1]. The problem generalizes the preceding in that the s^{th} link of the chain is now a rigid body of mass M_s and moment of inertia $M_s k_s^2$ about its center of mass, located at the point x_s, y_s, distant by the amounts l_s from one junction and r_s from the other (Figure 78). With φ_s the complement of the slope angle of the s^{th} link, the equations of constraint are

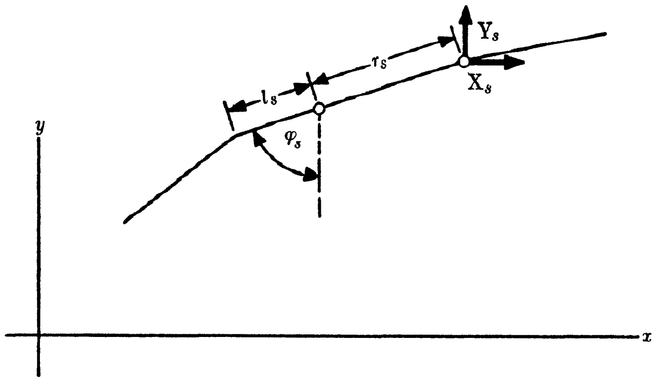

Figure 78. Variables used by EULER in obtaining the general equations of motion for a chain with rigid links

$$\begin{aligned} x_s - x_{s-1} &= r_s \sin \varphi_s + l_{s-1} \sin \varphi_{s-1} , \\ y_s - y_{s-1} &= r_s \cos \varphi_s + l_{s-1} \cos \varphi_{s-1} . \end{aligned} \tag{230}$$

Forces X_s and Y_s, acting in the x and y directions, are supposed given at each junction, with $X_0 = Y_0 = X_n = Y_n = 0$. The balance of linear momentum is then expressed by

$$M_s \ddot{x}_s = X_s - Y_{s-1}, \quad M \ddot{y}_s = Y_s - Y_{s-1} ; \tag{231}$$

1) The analysis is given on pp. 459—460 of Notebook EH4. On pp. 480—483 are the details of integration for the case of three bodies.

of angular momentum by

$$(232) \qquad M_s k_s^2 \ddot{\varphi}_s = X_s r_s \cos \varphi_s - Y_s r_s \sin \varphi_s + X_{s-1} l_s \cos \varphi_s - Y_{s-1} l_s \sin \varphi_s \ .$$

After writing down these equations, EULER derives the integrals of linear momentum and energy. Apart from some fully worked out special cases, [he draws no conclusion from this system, nor does he attempt a passage to the limit to obtain equations for the continuous band.

From this paper comes our knowledge of how to set up the equations of motion of a linked system in what today seems the most straightforward way. This is the first instance in which the principles of *both linear and angular momentum* are set down as the sole and basic and independent principles of mechanics. Moreover, the allowance for *arbitrary forces at the junctions* reflects EULER's growing realization that *shear forces* act upon elastic bands; this idea will mature many years later (below, § 57).

The amazing derivation of the partial differential system governing finite motion, obtained by a limit process from the discrete case, was put in too obscure and complex a form to be understood, and when EULER came back to the subject thirty years later (pp. 291—292 below), he had apparently forgotten this early analysis[1]).]

31. EULER's general solution for longitudinal vibration of the loaded elastic cord (1748). [With the experience of the preceding paper behind him, EULER is now able to derive the equations of motion for all sorts of linked systems.] In his paper, *On the propagation of pulses through an elastic medium*[2]), he sets up as a model for explaining the transmission of a sound in air a set of equal masses M connected by like springs. EULER's verbal con- Summ., 58

1) Here we notice a work that belongs to the period 1743—1744 but is isolated in content, EULER's *Neue Grundsätze der Artillerie* . . . (E77), Berlin, 1745 = Opera omnia II 14, 1—409. EULER's third remark on the ninth law in Chapter I concerns the strength of cannons. After mentioning the need for a factor of safety, since the theory assumes the material uniformly strong, while in casting there are always imperfections, EULER attempts a local theory of the equilibrium of a hollow cylinder. He isolates a sector of finite angle 2φ and balances the forces acting upon it, supposing the inside subject to uniform pressure m, while the action of the remainder of the cylinder upon the sector is assumed to be a pure tension of amount n. Balancing these forces correctly yields $ma = nb$, where a is the inner radius and b is the thickness. Astonishingly enough, however, EULER fails to integrate to get the resultant force of the pressure, and also he supposes it sufficient that the resultant force of the ring tension exceed that of the pressure. More important than these errors is that from this example we see clearly that EULER, while he had begun to see the need for *resultant* shear stress (above, p. 217), was unable to treat a problem in which *interior* shear stress is of major importance.

This work may be related to what seems to be an erroneous theory of the arch on pp. 488—489 of Notebook EH4. There EULER seems to take account only of the normal component of load; he may be attempting to find that form of arch in which the tension is constant.

2) E136, "*De propagatione pulsuum per medium elasticum,*" Novi comm. acad. sci. Petrop. 1 (1747/8), 67—105 (1750) = Opera omnia II 10, 98—131. Presentation date: 2 September 1748.

clusions are somewhat negative: Since "the motion of two or more particles is no longer oscillatory, but by so much the more different from it, the greater the number of particles, [the author] says that sound may not at all be understood as propagated through the air as some able men would have it, when they assert that when a string or other sounding instrument is set in motion there are in the air particles of this sort which take on an oscillatory motion and excite the organ of hearing." [The analysis does not justify such sweeping conclusions, although of course it shows that the resulting motion is not a *simple* oscillation.]

4 EULER takes the elastic force as proportional to the ratio $\frac{\text{initial length}}{\text{stretched length}}$, [but this rather
8—19 extraordinary law introduces no error since only small displacements are considered]. He begins by solving the problem for the case of two masses (Figure 79). [We do not reproduce the details of these examples, since below we give EULER's treatment of the general case; however, we remind the reader that this is the first time that *the general motion of an oscillating system is obtained*, *i. e.*, for the first time we see a theory sufficient to allow *more than one simple mode to be excited at once*. This justifies the detail in which EULER treats these simple cases.] First he considers only one mass to be displaced initially, both masses being released with zero velocity. The two bodies then begin to move toward each other. As a measure of the time for a pulse to travel from one to the other, EULER first calculates
15 the time when the second body acquires its maximum speed away from the first. He finds that both initial velocities are of the order of t. [Apparently disturbed by this fact,] he concludes only that the possible types of motion are very different and goes on to a second approach, in which he replaces the condition of no initial displacement for the second par-
16 ticle by that of no force, *i. e.*, no acceleration. The initial velocity of the second body is then of the order t^3, while that of the first remains of the order t. This case EULER considers a
17—18 better model for the propagation of a pulse. He finds that when $t = \frac{\pi}{1+\sqrt{3}}\sqrt{\frac{M}{K}}$, K being the force constant of the springs, the first particle acquires its maximum speed; at time $2t$, the second particle attains its greatest speed. Therefore the time t is that required
19—20 for the pulse to travel from the first to the second body. If the spacing of the particles at rest is a, then the velocity of propagation is

Figure 79.
EULER's treatment of longitudinal oscillation of an elastic cord carrying two masses

$$v = \frac{a}{t} = \frac{1+\sqrt{3}}{\pi}\sqrt{\frac{K}{M}}\cdot a. \tag{233}$$

This result EULER compares with measured values of the speed of sound in air, using, as usual, the idea that the "elasticity" is the weight of a column of air[1]).

1) See p. XXXV of my introduction to L. EULERI Opera omnia II 13, giving the history of the theories of the speed of sound in air.

EULER notes that it is possible to start the motion so that it is "regular . . ., like that of an oscillating pendulum"; [*i. e.*, by specially selected initial conditions, either of the two simple modes may be excited separately.] 21

A similar treatment of the case of three bodies leads to a less satisfactory result, and EULER abandons the idea of calculating the speed in this way. However, from the results for one, two, and three bodies he is led to conjecture that the proper frequencies for a system of n bodies are obtained by multiplying $\frac{1}{\pi}\sqrt{K/M}$ by 22—29 30

$$\cos\frac{\frac{1}{2}\pi}{n+1}, \quad \cos\frac{2\cdot\frac{1}{2}\pi}{n+1}, \quad \cos\frac{3\cdot\frac{1}{2}\pi}{n+1}, \ldots, \quad \cos\frac{n\cdot\frac{1}{2}\pi}{n+1}. \tag{234}$$

To treat the general case, EULER *introduces the indicial notation*[1]) $x, x^{\mathrm{I}}, x^{\mathrm{II}}, x^{\mathrm{III}}, \ldots, x^{(\mathrm{V})}, \ldots, x^{(\lambda-2)}, x^{(\lambda-1)}$ for the displacement which we here write as x_k. The whole length of the line is $(n+1)a$, and $x_0 \equiv x_{n+1} \equiv 0$. The general equations [converted to general units] are 31—33

$$M\ddot{x}_k = K(x_{k+1} - 2x_k + x_{k-1}), \quad k = 1, 2, \ldots, n. \tag{235}$$

[While EULER does not mention it, by comparing (235) with JOHN BERNOULLI's well known result (78) any reader could see at a glance that the present problem is mathematically analogous to that of transverse oscillation of a taut loaded string. To read off results for the latter, replace K by T/a.]

Assume a solution 34

$$x_k = \mathfrak{A}_k \cos 2\sqrt{\frac{K}{M}}\,pt, \qquad \mathfrak{A}_0 \equiv \mathfrak{A}_{n+1} \equiv 0. \tag{236}$$

Then 35

$$-4\mathfrak{A}_k p^2 = \mathfrak{A}_{k+1} - 2\mathfrak{A}_k + \mathfrak{A}_{k-1}, \tag{237}$$

so that

$$\mathfrak{A}_{k+1} = 2(1-2p^2)\,\mathfrak{A}_k - \mathfrak{A}_{k-1}. \tag{238}$$

As suggested by the conjecture in § 30, put 36

$$p = \sin\Phi, \quad \text{so that} \quad 1 - 2p^2 = \cos 2\Phi. \tag{239}$$

Then (238) becomes

$$\mathfrak{A}_{k+1} = 2\cos 2\Phi\,\mathfrak{A}_k - \mathfrak{A}_{k-1}. \tag{240}$$

EULER's argument is obscure, but the idea seems to be as follows. The length of the colum is a; let its area be S. Then $Ka = pS$, where p is the air pressure, and hence $K/M = pS/(Ma) = p/\left(\frac{M}{Sa}\cdot a^2\right) = p/(\varrho a^2)$. Hence (233) yields

$$v = \frac{1+\sqrt{3}}{\pi}\sqrt{\frac{p}{\varrho}}.$$

This is *less* than NEWTON's value, in the ratio $(1+\sqrt{3})/\pi$.

1) Recall that our uses of indicial notations previously (and also subsequently, for the most part) in this history are our own abbreviations for the lengthy notations of the original sources.

Now put $\mathfrak{A}_1 = \mathfrak{A} \sin 2\Phi$; then from (240) follows

$$\mathfrak{A}_k = \mathfrak{A} \sin 2k\Phi \ . \tag{241}$$

Putting $k = n + 1$ yields

$$0 = \mathfrak{A}_{n+1} = \mathfrak{A} \sin (2n + 2)\Phi, \quad \text{or} \quad (2n + 2)\Phi = r\pi \ . \tag{242}$$

By substitution of this last result in (239) we obtain $p = \sin \frac{r \cdot \frac{1}{2}\pi}{n + 1}$, confirming the con-
37 jecture (234). Thus there are "as many values for p . . . as there are bodies . . . ," [and in our notation we write EULER's result in the form

$$\nu_r^{(n)} = \frac{1}{\pi} \sqrt{\frac{K}{M}} \sin \frac{r \cdot \frac{1}{2}\pi}{n + 1}, \quad r = 1, 2, \ldots, n, \tag{243}$$

extending JOHN BERNOULLI's results (77)[1])].

37—38 Substitution into (236) yields the general solution corresponding to zero initial velocities:

$$x_k = \sum_{r=1}^{n} \mathfrak{A}_r \sin \frac{rk\pi}{n + 1} \cos \left(2 \sqrt{\frac{K}{M}}\, t \sin \frac{r \cdot \frac{1}{2}\pi}{n + 1} \right) . \tag{244}$$

39—44 EULER wishes to evaluate the constants $\mathfrak{A}_r$ when $x_1 = -X$ while all other x_k vanish at $t = 0$. After trial of the cases $n = 1, 2, 3, 4, 5, 6$, he conjectures that

$$\mathfrak{A}_r = -\frac{X}{n + 1} \sin \frac{r\pi}{n + 1} \ . \tag{245}$$

That this is so follows from the identity

$$\sum_{k=1}^{n} \sin \frac{k\pi}{n + 1} \sin \frac{rk\pi}{n + 1} = \tfrac{1}{2}(n + 1)\, \delta_{r1}, \quad r \leqq n, \tag{246}$$

which EULER conjectures and goes some way toward proving. Putting (245) into (244) yields

$$-\frac{n + 1}{2} \frac{x_k}{X} = \sum_{r=1}^{n} \sin \frac{r\pi}{n + 1} \sin \frac{rk\pi}{n + 1} \cos \left(2 \sqrt{\frac{K}{M}}\, t \sin \frac{r \cdot \frac{1}{2}\pi}{n + 1} \right) . \tag{247}$$

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1) For comparison, we may express (243) in the context of transverse oscillations of a taut loaded string (*cf.* the remark following (235)). The total mass being nM and the total length being $(n + 1)a$, the mean line density is $\sigma = nM/[a(n + 1)]$, and with ν given by (75) we may put (243) into the form

$$\frac{\nu_r^{(n)}}{\nu} = \frac{2}{\pi} \sqrt{n(n + 1)} \sin \frac{r \cdot \frac{1}{2}\pi}{n + 1} \ .$$

From this result it is immediate that for any *fixed* r we have

$$\frac{\nu_r^{(n)}}{\nu} \to r$$

as $n \to \infty$. *I.e.*, the r^{th} proper frequency of the loaded string approaches the r^{th} proper frequency of the continuous string. These inferences were not drawn by EULER until later (see below, p. 272).

But this is only an auxiliary. "To obtain a case more appropriate to the propagation 45
of pulses, we suppose that initially every body is at rest and the accelerating forces on all
except the first are zero." From (244) follow for $-4\frac{K}{M}\mathfrak{A}_r\sin^2\frac{r\cdot\frac{1}{2}\pi}{n+1}$ the same equations 46
as those satisfied by $\mathfrak{A}_r$ in the previous case. This gives for the accelerating force on the r^{th} particle an expression proportional to the right-hand side of (247). Therefore the velocity of the last particle is a maximum when

$$0=\sum_{r=1}^{n}\sin\frac{r\pi}{n+1}\sin\frac{rn\pi}{n+1}\cos\left(2\sqrt{\frac{K}{M}}\,t\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right); \tag{248}$$

equivalently,

$$0=\sum_{r=1}^{n}(-1)^r\sin^2\frac{r\pi}{n+1}\cos\left[2\alpha(n+1)\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right], \tag{249}$$

where $\alpha\equiv\sqrt{\frac{K}{M}}\frac{t}{n+1}$. Let h be the length of a column whose weight equals the elastic 47
force of the fluid [note that h need not be the barometric height]. Then with $g=$ the gravitational acceleration, we have $a\sqrt{\frac{K}{M}}=\sqrt{gh}$, so

$$\text{velocity}=\frac{(n+1)a}{t}=\frac{a}{\alpha}\sqrt{\frac{K}{M}}=\frac{1}{\alpha}\sqrt{gh}\,. \tag{250}$$

If $h=$ the barometric height, then this formula gives speeds greater than NEWTON's in the ratio $1/\alpha$.

Everything depends on finding α by solving (249), but this EULER is unable to do. To 48—55
fit experimental data, we should get $\alpha=0{,}85$. For $n=2$ and 3 the values are $\alpha=0{,}55$ 56
and 0,76, and these "seem to converge rapidly enough" toward the desired result. [In fact, the limit value of α must surely be 1.] In any case, the speed is proportional to $\sqrt{h}$. "But h 57
is the length of the column of the same fluid whose weight equals the elastic force of the fluid. Thence, if the elastic force is denoted by E and the density by D, the weight of the column will be as E/D. Therefore in various elastic fluids the speeds at which pulses are propagated will be ... as $\sqrt{E/D}$."

[As a contribution to the theory of sound, this paper, brilliant as it is, is a failure. What it attains is the *exact and general* solution of a problem of small oscillation of arbitrarily many masses. The concise and elegant procedure for solution, including the explicit formula (243) for the proper frequencies, could not be improved today. While the conclusion in the last section follows from much more general dimensional considerations, it is the merit of this paper to have *derived*, for the first time, a formula of the type (250) with α given as a root of an explicitly known equation.

But more than this, it is the *approach* to the mechanical problem that initiates the modern period in the study of vibrations. Little as it would nowadays be expected, this paper of 1748, fifty years after NEWTON's *Principia, is the first to solve a problem of the vibration of coupled masses by superposition of simple modes.* For this is precisely what EULER does, without comment, by trying the solution (236). *A fortiori*, (244) is *the first example of a general solution of a problem of n bodies.* Moreover, since this paper is the first to treat an oscillation problem on the basis of the general equations of motion, it is also the first to show that *the simple modes are special solutions corresponding to specially selected initial conditions.* The theory of small oscillations still has far to go, however. To complete the analysis of the present mechanical problem the constants $\mathfrak{A}_r$ in (244) should be determined so that the displacement x_k takes on an arbitrarily assigned value X_k at $t = 0$. This EULER achieves in the special case when only X_1 is different from zero; though he might easily do so by superposition of n solutions of this kind, he does not approach the more general problem, since he wishes to use only initial conditions he thinks appropriate for representing a sound pulse.]

32. Summary to 1748. With some astonishment, we see that from 1691 to 1748 the entire mathematical science of vibration, deformation, and elasticity has been dominated if not monopolized by the geometers of Basel[1]). In particular, the last two decades have seen EULER, DANIEL BERNOULLI, and old JOHN BERNOULLI swiftly create several mathematical theories yielding definite and correct if somewhat isolated explanations of some classes of phenomena having scarcely estimable bearing toward all later work on vibrations and elasticity. In summarizing these brilliant researches, we pass over results that remained unpublished until after 1751 and thus failed to influence the immediately following studies.

I. Static deflection.

1. EULER unified all existing theories by means of the general equation (91), expressing the balance of moments acting upon any deformable line obeying the BERNOULLI-EULER law (89), or, by an immediate generalization, the form (69) appropriate to initially curved elastic bands. Publication : 1732

2. DANIEL BERNOULLI conjectured and EULER verified the principle (140) defining the equilibrium of an elastic band in terms of its stored energy. Publication (by EULER): 1744

3. By a rigorous analysis of the quadratures (172), EULER determined, classified, and sketched all the possible forms an initially straight elastica may assume when subject to terminal force and couple. Publication : 1744

4. As a corollary of the above, EULER obtained the celebrated buckling formula (180). While in this early work he applied (180) only so as to obtain the special case (185) appropriate when both ends of the column are pinned, the result (180) determines all cases

1) Indeed, by others there are but four works of any importance in the whole period of more than fifty years: those of VARIGNON (1704), PARENT (1713), TAYLOR (1713), and D'ALEMBERT (1743).

of buckling in compression (as we shall see below). Moreover, it is a rigorous theorem, not resting in any way on the linearized theory EULER sketched in a few lines of the text. EULER's work, as stated in No. 3, fully determines the bent form subject to any load exceeding the critical load. Publication : 1744

II. Vibration.

5. DANIEL BERNOULLI showed that vibrating systems of many degrees of freedom can oscillate with many different simple harmonic motions at definitely calculable proper frequencies. The different modes of a given system have definite and different numbers of nodes, increasing with the corresponding proper frequency. His direct method, resting at bottom upon the assumption that the accelerations are as the displacements, he applied first to the weighted hanging cord and to the uniformly heavy hanging cord. He indicated that in the former case there are as many modes as there are weights; in the latter, infinitely many. DANIEL BERNOULLI gave some rough calculations of frequencies and nodal distances, using "BESSEL functions" for the continuous cord. EULER, using "LAGUERRE polynomials", gave the explicit solution for the cord loaded by an arbitrary number of equal and equidistant weights. Publication : 1740, 1741

6. DANIEL BERNOULLI and EULER independently obtained the differential equation (125) for the simple modes of transverse vibration of bars, as well as the end conditions (132) appropriate to a free end. EULER obtained the general equation (136) for the frequencies and the root (135) yielding the fundamental frequency for the clamped-free modes. Publication (by EULER only) : 1740

7. EULER obtained the general solution (146) for linear differential equations with constant coefficients, opening the way to manageable solution of many vibration problems. In particular, EULER gave a full analysis of the equation (142) for a harmonically driven oscillator, discovering and emphasizing the resonant case. Publication: 1743, 1750

8. DANIEL BERNOULLI calculated approximately the full set of proper frequencies (161) and (164) for clamped-free and free-free transverse vibrations of a rod. He remarked upon four of the six possible classes of simple modes and gave special attention to the free-free class, for which he determined the nodal distances of the first five modes. EULER derived the equations of proper frequencies for all four of BERNOULLI's classes and calculated the solutions of all, but he emphasized mainly the clamped-free modes. He recommended that the flexural rigidity of bars be determined from their measured frequencies. Publication : 1744 (EULER), 1751 (BERNOULLI)

9. DANIEL BERNOULLI stated that the simple modes of a vibrating system may be excited simultaneously. Since he was not in possession of equations of motion, proof of this principle was out of the question. Publication : 1751

III. Equations of motion.

10. While all work on the motions of complex systems up to 1742 rested on direct and essentially static methods applicable only to small harmonic oscillations, in a paper written by that year JOHN BERNOULLI took a great step toward the equations of motion by being the first to refer all particles to a single rectangular Cartesian co-ordinate system. His equations (154) for the hanging weighted cord come close to being equations of motion, although he employs them only when the reaction of inertia is centrifugal force. He obtained the differential equations of finite motion for the case when $n = 2$. Simultaneously, D'ALEMBERT obtained the differential equations of small motion when $n = 2$ and $n = 3$ by a method which applies for all values of n. He derived also the partial differential equation of small motion of a continuous heavy cord. Publication: 1743

11. Shortly thereafter, EULER obtained the general equations of finite motion (209) for the loaded string, as well as the system (231) (232) for finite motion of a set of bars linked together by hinges where assigned forces act. Publication : 1751

12. By a brilliant passage to the limit, EULER obtained the partial differential system (222) for finite motion of the continuous string. The form of these equations was such as to render them virtually impossible to use, and EULER failed to derive from them the equation for small motions, or in fact to see any use for them. Publication : 1751

13. EULER obtained the differential equations (235) for longitudinal vibrations of an elastic cord loaded by an arbitrary number of masses. In this, the earliest successful analysis of any problem of n bodies, EULER derived the explicit formulae (243) and (244) for the proper frequencies and for the general solution. For this problem, the general motion is thus proved to be resoluble into composite simple harmonic modes. These modes are seen to represent motions corresponding to specially selected initial conditions. For the case when only the end mass is displaced initially, EULER obtained the explicit solution (246) by a method foreshadowing the explicit solution of the general initial-value problem. All EULER's results here are immediately interpretable in the context of transverse vibrations of a taut loaded string. EULER, however, intent upon special configurations he considered appropriate to a model for the transmission of sound in air, did not explain very clearly the significance of this great memoir for the whole science of vibration. Publication : 1750

After this, the remainder of our history must come in part as an anti-climax. Never again in our period for study are we to encounter such a flood of wonderful results, so high a proportion of achievement in every work published. Yet a major want in the whole science, even as viewed from an intelligent eye of 1750, remained : For continuous lines, no manageable differential equations of motion were known. The third part of our history opens with the discovery of such an equation, that for the vibrating string, and describes controvery to which its solution straightaway gave rise.

Part III. The Controversy over Small Plane Vibrations of a String of Uniform Thickness, 1746—1788

33. D'Alembert's first memoir (1746): the partial differential equation and its solution by an "equation". [After the brilliant mechanical researches just described, we must now descend to the celebrated and deplorable quarrel which watered the effort of the principal savants at the middle of the century. What follows confirms the principle that ever the greatest quantity of paper is smeared over with the dullest matter. As a corollary, it is to this part alone of our subject that histories of mathematics or physics give any considerable attention. In the case of the vibrating string, the *linearity* of the problem made it possible to drag in analytical questions but little connected with mechanics. In all the papers we summarize in this introduction, the ratio of content to length, of concrete results to words, is here the least. While I am tempted to leave out the whole matter, the dilettante essays of the last century have spread such misconception that it is best to go over the old ground once more, if only to illustrate that second principle that in the history of science nothing is less welcome and less read than an account of the facts.

Only the prolix sequel is dull: The beginning is as brilliant a research as any in our subject:] By the end of 1746 D'Alembert had completed[1]) his *Researches on the curve formed by a stretched string set into vibration*[2]).

"I propose to show in this memoir that there are infinitely many curves other than I
the companion of the elongated cycloid [sine curve] which satisfy the problem in question." Among the stated hypotheses is $F = T\dfrac{\partial^2 y}{\partial x^2}$, cited from Taylor's book. [Indeed this follows at once from Taylor's formula $(74)_2$ when the slope is small; a hypothesis stronger than this latter was stated by Taylor but not used in this way; by D'Alembert it is used
but not stated. While most of what D'Alembert now proceeds to say seems superfluous[3]), II
we remark for later reference his assertion that] "it is certain that the ordinate y can be
expressed only as a function of the time and of the corresponding abscissa or arc. . . ." The IV

1) This is shown by his letters to Euler on 6 January 1747 and 17 June 1748.

2) "*Recherches sur la courbe que forme une corde tendue mise en vibration,*" Hist. acad. sci. Berlin [3] (1747), 214—219 (1749); the second part, with sections numbered consecutively, is "*Suite des recherches sur la courbe que forme une corde tendue, mise en vibration,*" *ibid.* 220—249.

3) While in general in this history we overlook matters of notation, the reader should remember that here and for some years to come partial derivatives are expressed in terms of differential forms involving various letters. Thus D'Alembert consumes a page in setting $y = \theta(t, s)$, $dy = p dt + q ds$, $dp = \alpha dt + \nu ds$, $dq = \nu dt + \beta ds$, where the coefficient ν is the same in each "by the theory of Mr. Euler" $\left[i.\ e., \dfrac{\partial^2 y}{\partial s\, \partial t} = \dfrac{\partial^2 y}{\partial t\, \partial s}\right]$, in establishing that $\beta = \dfrac{\partial^2 y}{\partial s^2}$, and in identifying α with the acceleration. As usual in the works of D'Alembert, misprints abound. Errata are given in the *Histoire* [6] (1750), 414—415 (1752).

acceleration is $\partial^2 y/\partial t^2$. "It is plain by Lemma XI, Section I, Book I of [NEWTON's] *Principia* that

$$\frac{1}{c^2}\frac{\partial^2 y}{\partial t^2} = \frac{\partial^2 y}{\partial x^2}, \quad c^2 \equiv \frac{T}{\sigma}. \tag{251}$$

[Thus, after so many near misses, *the wave equation finally appears*. True, all D'ALEMBERT has done is to put into the old formula (74) of TAYLOR proper approximations for A_n and r, but indisputably he is the first to do so[1]). It is also noteworthy that in so reviving a forgotten aspect of the otherwise well known paper of TAYLOR, D'ALEMBERT joins EULER[2]) in being *the first to set down the momentum principle as sufficient to derive all the differential equations governing a system of many degrees of freedom*[3]).

VI By change of units, (251) may be written as

$$\frac{\partial^2 y}{\partial t^2} = \frac{\partial^2 y}{\partial s^2}. \tag{252}$$

VII Since in general

$$d\left(\frac{\partial y}{\partial s} \pm \frac{\partial y}{\partial t}\right) = \frac{\partial^2 y}{\partial s^2} ds + \frac{\partial^2 y}{\partial s\,\partial t}(dt \pm ds) \pm \frac{\partial^2 y}{\partial t^2} dt, \tag{253}$$

if (252) holds we have

$$d\left(\frac{\partial y}{\partial s} \pm \frac{\partial y}{\partial t}\right) = \left(\frac{\partial^2 y}{\partial t^2} \pm \frac{\partial^2 y}{\partial s\,dt}\right)(dt \pm ds), \tag{254}$$

"whence it follows

1°, That $\frac{\partial^2 y}{\partial t^2} + \frac{\partial^2 y}{\partial s\,\partial t}$ is a function $t+s$, and that $\frac{\partial^2 y}{\partial t^2} - \frac{\partial^2 y}{\partial s\,\partial t}$ is a function to $t-s$.

2°, That, consequently, we have ...

$$\frac{\partial y}{\partial t} = \varphi(t+s) + \Delta(t-s), \qquad \frac{\partial y}{\partial s} = \varphi(t-s) - \Delta(t-s)\ldots \tag{255}$$

1) Not only were EULER's correct general equations (222) for finite motion of a string still unpublished, but also EULER himself failed to use them or ever to refer to them again.

2) We refer to the then not yet published papers E174 and E136 described in §§ 29—30, the former of which uses the principle of moment of momentum as well.

3) As is shown by his own reference to NEWTON, D'ALEMBERT does *not* use his own statement of "D'ALEMBERT's principle", much less either of the methods referred to in the subsequent literature by that name. Note that essentially this same NEWTONian approach, though less openly, had been used by D'ALEMBERT to derive the partial differential equation of the heavy cord (above, p. 192). TODHUNTER, § 63 of *op. cit. ante*, p. 11, is most misleading when he writes that a certain work is "somewhat obscure for the science of dynamics had not yet been placed on the firm foundation of D'ALEMBERT's *Principle*," since so far as I know not one single partial differential equation of the dynamics of continua was first obtained or subsequently any better established by use of that principle as enunciated by D'ALEMBERT. The description of D'ALEMBERT's derivation by BURKHARDT, § 4 of *op. cit. ante* p. 11, is false.

therefore . . . $y = \int\left(\frac{\partial y}{\partial t}\,dt + \frac{\partial y}{\partial s}\,ds\right)$, or

$$y = \Psi(t+s) + \Gamma(t-s)\ , \tag{256}$$

where $\Psi(t+s)$ and $\Gamma(t-s)$ express functions still unknown." [Equivalently

$$y = \Phi(ct+x) + \Psi(ct-x)\ , \tag{257}$$

and this notation we use henceforth.]

"But it is easy to see that this equation includes an infinity of curves. To show this, consider here only a special case, namely, $y = 0$ when $t = 0$; that is, let us suppose the string, when it starts into vibration, is stretched out in a straight line." Then VIII

$$\Psi(-x) = -\Phi(x)\ . \tag{258}$$

Since at the end point $x = 0$ we have $y = 0$ for all t, it follows that

$$\Psi(u) = -\Phi(u)\ . \tag{259}$$

Thus (258) and the condition that $y = 0$ also at the end point $x = l$ become

$$\Phi(u) = \Phi(-u),\ \Phi(u+l) = \Phi(u-l)\ . \tag{260}$$

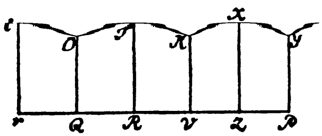

Figure 80.
D'ALEMBERT's solution of the wave equation (1746)

To find a "quantity" satisfying (260) [*i. e.* an even function of period $2l$], D'ALEMBERT employs Figure 80, where $QR = l$ and the part TK is "equal and like" the part OT. "But the geometers know that such a curve can always be engendered by another curve," and D'ALEMBERT goes on to give a construction of a generalized cycloid. IX

Any such function provides a solution for the vibrating string. X

"It is easy to see" that the velocity v is given by XI

$$v\ \left[\equiv \frac{\partial y}{\partial t}\right] = c\Phi'(ct+x) - c\Phi'(ct-x)\,. \tag{261}$$

Hence the initial velocity is

$$V = c\Phi'(x) - c\Phi'(-x) \quad [= 2c\Phi'(x)]\ . \tag{262}$$

By $(260)_1$, Φ is even; therefore Φ' is odd; from (262), "the expression for the initial velocity . . . must be such that when reduced to a series it includes only odd powers of x. Otherwise . . . the problem would be impossible, that is, one could not assign a function . . . such as to represent in general the value of the ordinate of the curve for any abscissa x and any time t."

The rambling and disjointed second part of the memoir interweaves positive results
XXIII and negative restrictions, which we here separate. To apply (257) to the general problem where

$$y(x, 0) = Y(x), \quad v(x, 0) = V(x) \ , \tag{263}$$

the end condition $y(0, t) = 0$ yields (259) as before. Hence

$$\begin{aligned} y &= \Phi(ct + x) - \Phi(ct - x) \ , \\ Y &= \Phi(x) - \Phi(-x) \ , \\ V &= c\,[\Phi'(x) - \Phi'(-x)] \ , \\ \frac{1}{c}\int V\,dx &= \Phi(x) + \Phi(-x) \ , \end{aligned} \tag{264}$$

"and thus the problem is impossible unless $Y(x)$ and $V(x)$ are odd functions of x, that is,
XXVIII functions where only odd powers of x enter . . ." From $(264)_{2,4}$ follows

$$\Phi(x) = \frac{1}{2c}\int V(x)dx + \tfrac{1}{2}Y(x)\,, \quad \Phi(-x) = \frac{1}{2c}\int V(x)dx - \tfrac{1}{2}Y(x)\,. \tag{265}$$

XXIV The end condition $y(l, t) = 0$ yields $(260)_2$, or

$$\Phi(u + 2l) = \Phi(u) \ . \tag{266}$$

XXI TAYLOR (§ 16, above) considered only sinusoidal solutions. When $Y = 0$, by a long calculation D'ALEMBERT obtains $\Phi(u) = \mathfrak{A}\cos\frac{\pi u}{l}$, and he says a similar result holds when
XXII $V = 0$. In these two cases, $y = f(x)\,g(t)$, and D'ALEMBERT asserts that they are the only such cases. For then $y(x, t_1)/y(x, t_2) = f(t_1, t_2)$; "by the ordinary method" one then finds that $f(x) = \sin Mx$, [but while indeed the most general solution of this kind is $y = \mathfrak{A}\sin(Mx + N)\sin(Mct + P)$, D'ALEMBERT's remarks do not constitute a proof].
XLII As for TAYLOR's assertion that a non-sinusoidal vibration settles into sinusoidal form, D'ALEMBERT [rightly] says that TAYLOR's argument is faulty.

XII The curve $z = \Phi(u)$ D'ALEMBERT calls the "generating curve". ". . . the general
XXX solution of the problem of the vibrating string is reduced to two things : 1°, to determine the generating curve in the most general way, 2°, to find the curve from the values of
XXXIV Y and V." No. 2 is solved by (265). "But one must take heed that Y and V may not be given at will, since they must satisfy certain other conditions, as has been seen above in this memoir." D'ALEMBERT gives a long list of these other conditions, [which arise from his tacit assumption that the *entire* generating curve is given by an "equation".] It suffices here to mention one such condition : $\Phi'(0) = 0$ or ∞. Now in the case $Y = 0$, we have seen that Φ' is odd ; thus [on the assumption that Φ' is continuous at 0] follows $\Phi'(0) = 0$.

[If $V = 0$, then TAYLOR's solution $\Phi = \mathfrak{A} \sin \frac{\pi u}{l}$ is one D'ALEMBERT considers admissible, and for this solution $\Phi'(0) \neq 0$ or ∞; thus the second part of D'ALEMBERT's assertion, as is not infrequent in his work, is simply an error even if one accepts his point of view. But the essential is that D'ALEMBERT *restricts the initial shape and initial velocity* of the string to curves whose "equations" are odd functions of period $2l$:] "One will notice at XXXIV
once that since Y and V are odd functions, the curves whose ordinates are proportional to Y and V must be such that when continued on each side of the origin they have two indefinite and like equal parts, one above and one below the axis."

True, the ordinary means of setting a string into vibration is to give it a triangular or XLIII
polygonal form. To apply our general solution to this case is impossible because of the conditions mentioned. "Therefore there is nothing else to do than to seek the motion of the XLIV
string in regarding it as composed of a great number of points, joined together by inextensible threads." D'ALEMBERT sets up two equations [corresponding to the linearized case of EULER's not yet published system (209), *viz*, EULER's not yet published] system (235), and says they are easy to solve. If the end $x = l$ is free, we do not have (266), XLVII
and hence the solution may be "geometric".

A little later[1]) D'ALEMBERT replaced the "very indirect method" of § XXII by setting I
$\Phi(ct + x) - \Phi(ct - x) = f(t)g(x)$ and then by differentiation obtaining

$$\frac{1}{c^2}\frac{f''}{f} = \frac{g''}{g} = A, \quad \text{say}, \tag{267}$$

since the first two members "must be not only equal but even identical, that is to say, they must be equal to the same quantity, independently of any equation between t and x." Therefore

$$y = f(t)g(x) = (M e^{ct\sqrt{A}} + g e^{-ct\sqrt{A}})(M' e^{x\sqrt{A}} + g' e^{-x\sqrt{A}}) , \tag{268}$$

whence by applying the end conditions it follows that $g(x) = k \sin Nx$, $f(t) = R \sin Nct$ or $B \cos nct$. [As usual, D'ALEMBERT's result is not quite complete, and his analysis is awkward and not quite satisfactory; nevertheless the principle is correct, and this passage contains the *first solution of a partial differential equation by separation of variables.*]

D'ALEMBERT then explains more clearly the restrictions he considers appropriate to II
his solution (257). In respect to the process by which he has satisfied (266) [*i. e.*, to his construction of periodic functions], he writes, "But, lest some readers mistake the meaning of my words, I believe I must give warning here that in order to obtain this generating curve it is not enough to transport the initial curve successively above and below the axis . . ." In addition, Y must be an odd function of period $2l$, "which cannot happen

1) "*Addition au mémoire sur la courbe que forme une corde tenduë, mise en vibration,*" Hist. acad. sci. Berlin [6] (1750), 355—360 (1752).

unless the curve is mechanical[1]) and such as I have described in my memoir. In all other cases the problem cannot be solved, at least by my method, and I do not know but that it will surpass the forces of known analysis. In fact, . . . one could not express y analytically in any way more general than supposing it a function of t and s. But subject to this assumption one obtains the solution of the problem only for cases where the different shapes of the vibrating string may be included in one and the same equation. In all other cases it seems to me impossible to give y a general form[2])."

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1) This sentence does not convey the meaning which I think D'ALEMBERT intends, namely, "which cannot happen, aside perhaps from mechanical [*i. e.* non-analytic] curves, unless the curve is such as I have described in my memoir."

2) Not from any original contribution they contain but rather from their historical interest in reflecting the views of a highly educated and intelligent layman of the Age of Reason, I append here a comment on the publications of DIDEROT concerning our subject. These appear in his *Mémoires sur differens sujets de mathématiques*, Paris, Durand & Pissot, 1748, vj + [6] + 243 pp. The dedication to Madame DE P[RÉMONTVAL] claims that the work "treats subjects which are familiar to you, and does so in a manner not altogether foreign to you." Contrary to the implications bandied in the usual histories of mathematics, DIDEROT shows himself to be not only well acquainted with much of the advanced scientific knowledge of his day, including details of NEWTON's *Principia*, of the *Universal Harmony* of MERSENNE, and various works of EULER, but also to be adept at differential and integral calculus in the style of JOHN BERNOULLI and L'HÔPITAL. While DIDEROT's works reveal a competent mathematician, they are deficient in physical grasp. Among writers of the period DIDEROT is exceptionally scrupulous in acknowledging his sources and exceptionally gentle when he discovers what he considers to be an error.

The first paper, "*Principes généraux d'Acoustique,*" pp. 1—120, contains a critique of the work of TAYLOR. As shown by his remark on p. 21, DIDEROT has seen D'ALEMBERT's "general solution", then awaiting the press, but he shows no evidence of having understood it. Apparently he tries to find what is wrong with the method of TAYLOR, but he is really unable to do so. His Proposition II, "to describe the musical curve of TAYLOR," sets up the problem in JOHN BERNOULLI's style but, as in all previous publications, the possibility of the higher modes as solutions of the ordinary differential equation is overlooked. On p. 35 DIDEROT writes, "It is a matter of experience that a string struck by a bow assumes quickly enough a shape such that all its points arrive at the same time at the line of rest," but his immediately following discussion seems to reflect little comprehension of the higher modes. On p. 38 he writes, "Although the formulae of Mr. TAYLOR do not at first seem applicable to all cases, but only to those when the vibrating string assumes a particular shape, nevertheless they are good in all cases when the points of the string reach the line of rest at the same time... thus, whether the string assume the form given by TAYLOR or whether it assume some other, the time of its vibration will always be the same, and consequently it will cause the same sound to be heard. We rest content with stating these propositions, the rigorous proof of which is difficult and would carry us too far..." DIDEROT's apparatus gives no evidence of principles on which to base such a proof. In § X, the contents of which DIDEROT acknowledges to be due almost entirely to FONTENELLE, he describes the "bizarre phenomenon" observed by SAUVEUR and WALLIS but makes no attempt to connect it with the theory. The reader of this work of DIDEROT will understand better the signal originality of DANIEL BERNOULLI's early researches.

In § VII DIDEROT criticizes EULER's rules for equable sounds (above, p. 154). In § VIII he claims

[Thus D'ALEMBERT contends that his "general solution" (257) is valid if and only if Φ and Ψ are given by "equations" that are odd and periodic of period $2l$. His first purpose, apparently, is only to exhibit infinitely many non-sinusoidal solutions; in his later writing he is to show interest only in finding cases when the problem is "impossible".

In writings of the eighteenth century, the term "continuous function" is often a vague equivalent for what is now called "analytic function". It would not be just to regard this equivalence as precise, since many writers considered "continuous" those functions given by "equations", and occasionally, as will shortly be seen, there occur "equations" such as $y = \sin^{\frac{5}{3}} x$, representing a function not analytic at $x = 0$ but nevertheless fairly smooth there. A "discontinuous function" or "mechanical function" is what is now called a "piecewise smooth function", possibly non-analytic[1]). In D'ALEMBERT's work, every "function" is given by an "equation". While the operations to be allowed in forming an "equation" were not precisely delimited, it was generally accepted that two such expressions agreeing when the variables *lie in a certain interval* will have to agree *outside it as well*[2]). This explains D'ALEMBERT's "impossible" cases. For $V(x)$ and $Y(x)$ are given, indeed, only in the interval $0 \leqq x \leqq l$, whence by $(265)_{1,2}$ follows the "generating curve"

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by some vague generalities about strings to infer MERSENNE's law (9) for bells and EULER's law for rods (above, p. 155).

DIDEROT's "*Examen d'un principe de Mécanique sur la tension des cordes*," pp. 163—168, illustrates the fact that the stress principle is by no means obvious. "But here is a question which has heretofore much troubled the students of mechanics. It is asked if a string $A B$ fixed at B and stretched by any power A is stretched in the same way as it would be if in place of the fixed point B a force equal and opposite to the power A were to be supplied. Several authors have written on this question, first proposed by BORELLI." While DIDEROT confuses the matter by introducing the elasticity of the wire, his argument, like GALILEO's (above, p. 37), consists merely in reaffirmation. It is interesting, however, that DIDEROT then proposes to test the matter by experiment, using the pitch of the string as a measure of its tension. He writes of the experiment as if he had never carried it out.

1) *Cf.* § 5 of BURKHARDT, *op. cit. ante*, p. 11; A. SPEISER, "*Über die diskontinuirlichen Kurven*," EULERI Opera omnia I 25, XXII—XXIV (1952); Part II L of my Introduction to L. EULERI Opera omnia II 13; and also below, p. 247.

2) It would not be just to suppose that only rigorous proofs were lacking in order to make this conclusion true. Such would be the case, indeed, if "function" always meant "analytic function". In fact, however, "equations" more general than those defining analytic functions occur in D'ALEMBERT's examples below, and no amount of historical generosity can render his remarks or those of LAGRANGE correct in principle. In the very paper we are discussing, D'ALEMBERT begins by giving a cycloid as a possible generating curve, though it is not an analytic curve and does not satisfy all the conditions he himself imposes later on in the same paper. EULER's views on this subject are less extravagantly stated and thus more difficult to demonstrate false, yet I think it would be an exaggeration to infer that EULER's concept of the nature of "continuous" functions was clear. Rather, this is a domain of mathematics where precise definitions are of the essence, and the need for precise definitions was not felt in the eighteenth century.

$z = \Phi(u)$ for $-l \leqq u \leqq l$. By (266), the generating curve is then determined for all values of u. But if $z = \Phi(u)$ is to be an "equation", this last step is superfluous, for $z = \Phi(u)$ is then by its "nature" (*i. e.*, in the analytic case, by analytic continuation) already determined outside the interval $-l \leqq u \leqq l$. For the problem to be soluble, *these two continuations must agree*[1]). If $z = \Phi(u)$ is to be an "equation", $(264)_{2,3}$ show that $Y(x)$ and $V(x)$ must be "functions"; that is, unless the initial shape and initial velocity are given by "functions", the problem is "impossible".

To clarify D'ALEMBERT's viewpoint it thus remains only to explain why he requires $z = \Phi(u)$ to be an "equation". He himself, while never giving any reason, shows by his obstinate repetitions from now on until the end of his life that he regards it as entirely obvious that "mechanical" functions are to be exiled from mathematics[2]), or at least from mathematical physics. This is a consequence of LEIBNIZ's *law of continuity* as it was widely interpreted in the eighteenth century[3]) : Only "continuous" functions occur in the solution of physical problems. While nowadays this seems a merely arbitrary prejudice[4]), we must bear in mind that the majority of the geometers and more particularly the physicists of the day shared it. *E. g.*, JOHN BERNOULLI and D'ALEMBERT invoked LEIBNIZ's law in order to justify the application of the laws of physics to infinitesimal elements. Less obvious, perhaps, is the advantage of the resultant uniqueness theorem, indeed not proved but nevertheless correctly believed at the time, by which each soluble physical problem has but a single solution, determinate in principle up to a singularity resulting from its very nature, and indeed such a metaphysics would furnish a basis for regarding differential equations as a correct means of formulating natural laws.]

34. EULER's first memoir (1748): the solution by arbitrary functions. [That any mechanical problem was inherently "impossible" EULER could not for a moment accept.]

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1) *E. g.*, when $V = 0$, suppose the initial shape is the parabolic arc $Y = \alpha x(l - x)$, $0 \leqq x \leqq l$. Therefore $\Phi(u) = \frac{1}{2}\alpha u(l - u)$ for $0 \leqq u \leqq l$. The analytic continuation is

$$\Phi(u) = \tfrac{1}{2}\alpha u(l - u) \quad \text{for} \quad -\infty < u < +\infty .$$

This continuation is non-periodic and thus does not satisfy the conditions of the mechanical problem. On the other hand, the continuation (266) required by the mechanical problem is not given by the same "equation". Thus, according to D'ALEMBERT's view, the problem is insoluble for this initial figure.

2) D'ALEMBERT's later writings indicate that he considered illegitimate the application of differential and integral calculus to "discontinuous" functions.

3) My attempts to trace this law in LEIBNIZ's own writings have found only partial success.

4) On the other hand, those who have tried to teach the mass student will doubtless have encountered his deep-seated reluctance to stray far from polynomial apron strings, and I once had the misfortune to have to endure the harangues of a senior colleague, a famous aerodynamicist, who from a mystic attachment to the symbols he happened to know resolutely refused to allow meaning to any others.

Within a few months of seeing D'ALEMBERT's paper, EULER had written his own essay, *On the vibration of strings*[1]), [which seems at first reading to be largely a repetition of D'ALEMBERT's[2]), but from EULER's publishing it twice and with all possible speed we see how important he regarded its contents. Clearly he wishes to distinguish and perpetuate the true while omitting the false with which it was interwoven in D'ALEMBERT's work.] D'ALEMBERT has given "a very beautiful solution", but, believing he has added important observations, EULER will give his own, though "not very different"[3]). EULER's memoir, while evidently written in haste and not achieving his usual clarity[4]), calls for *the general*

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1) E119, "*De vibratione chordarum exercitatio,*" Nova acta erud. 1749, 512—527 = Opera omnia II 10, 50—62. Presentation date: 16 May 1748. French translation, E140, "*Sur la vibration des cordes,*" Hist. acad. sci. Berlin [4] (1748), 69—85 (1750) = Opera omnia II 10, 63—77.

2) EULER follows D'ALEMBERT in formulating the problem as a statement that two differential forms be exact (§ 14) and in obtaining a solution by rather obscure manipulation of these forms (§§ 15—17). EULER uses the method of his paper E8 (above, pp. 148—149) to calculate the restoring force, but otherwise his derivation (§§ 5—13) of (251) is the same as D'ALEMBERT's. Forgetting that four years earlier he had derived the system (222) governing finite motion, or perhaps distrusting the result, EULER writes here that finite motion is beyond the present reach of mechanics and analysis (§ 2).

3) This is EULER's ordinary graciousness; in principle, his solution is different. To D'ALEMBERT, EULER's solution is "entirely similar to mine . . . but only, it seems to me, a little longer" (§ II of *op. cit. ante*, p. 241).

Here we take notice of the articles D'ALEMBERT began to publish in the French *Encyclopaedia*, of which he was the editor and principal author for mathematics. The first volume carries as frontispiece a magnificent engraving of D'ALEMBERT, who, under the guise of authoritative reviews, filled its pages with shoddy hashes of antiquated science served up with a sauce of his own prejudices, advertisements for his researches, and attacks on his opponents.

In the article "Cordes, Vibrations des" (4 (1754)), as the laws of vibrating strings he gives only GALILEO's proportion (10), citing TAYLOR and JOHN BERNOULLI. After criticizing TAYLOR's attempt to prove that all points of the string cross the equilibrium configuration at once, he cites his own work, and adds, "I believe I am the first to have solved the problem . . . in a general way; Mr. EULER solved it after me, in using almost exactly the same method, with this difference only, that his method seems a little longer." No definite description of vibratory motion is given. D'ALEMBERT does not even mention the frequencies of the overtones and the nodes of the vibrations that produce them; these appear in later and supplementary articles provided by ROUSSEAU, "Sons harmoniques" in 15 (1765) and "Cordes Sonores" in Suppl. 2 (1776), where the experiments of WALLIS and SAUVEUR are summarized. D'ALEMBERT's own polemic supplement will be quoted below, p. 262.

A specimen of D'ALEMBERT's knowledge or ethics is furnished by his wordy article, "Chainette" (Enc. 3 (1753)). Vague and incomplete, it does not even cite a place where a reliable proof of (21) may be found, let alone mention those who first solved the problem.

4) As stated in § 4, EULER's problem is to determine the motion corresponding to arbitrary initial displacement $Y(x)$ when the initial velocity $V(x) = 0$. The condition $V = 0$ is never applied explicitly, however; through § 21 the analysis is valid for arbitrary V, but in § 22 we suddenly find that $\Phi = -\Psi$, as indeed is the case if and only if $V = 0$. Also, EULER fails to remark that D'ALEMBERT has given the formal solution $(264)_1$, (265) for the general case, when neither V nor Y vanishes identically.

solution of the mechanical problem: "In order that the initial shape of the string may be adjusted arbitrarily, the solution must be as inclusive as possible. [From this paper onward, EULER *is never content with less than the most general solution of each partial differential equation occurring*. In cases where he can find only special solutions, henceforth he always stresses that this is so only from "want of analysis" and urges all geometers to join in seeking the general solutions. For D'ALEMBERT, driven by a mysterious desire to confine rather than expand the frontiers of mathematics, this problem is of no interest.] EULER regards (266) as in itself sufficient to achieve the continuation of $\Phi(u)$ outside the range
22 $-l \leqq u \leqq l$, and *no restrictions* are to be imposed on the initial shape: "When such an eel-shaped curve (Figure 81), be it regular and contained in a certain equation or be it irregular or mechanical, has been described thus, its general ordinate will furnish the functions needed . . ." In a later paper[1] EULER explains this viewpoint still more clearly: First, with "absolutely arbitrary" Φ and Ψ, (257) furnishes a solution of (251); this follows by substitution and is independent of all the derivations given. "But, and this is the main thing, these two curves generated in the way shown are equally satisfactory, whether they are expressed by some equation or whether they are traced in any fashion, in such a way as not to be subject to any equation . . .

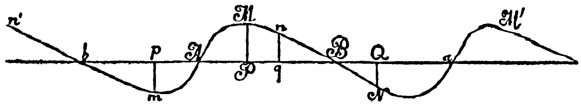

Figure 81. EULER's solution of the wave equation (1748)

"This construction always holds, whatever the nature of the initial form proposed for the curve, and only the part AMB [giving the shape in the interval $0 \leqq x \leqq l$] is relevant here; even when this part has other continuations . . . in virtue of its nature, they are not to be taken into consideration . . . The different like parts of this curve are thus not joined to each other through any law of continuity, and it is only by the description that they are joined together. For this reason it is impossible that all this curve should be comprised in any equation, unless perchance the figure AMB be such that its natural continuation entails all these repeated parts; and this is the case when the figure AMB is TAYLOR's sine curve or a mixture of such curves according to Mr. BERNOULLI[2]. This is also, according to all appearances, the reason that Messrs. BERNOULLI and D'ALEMBERT have believed the problem soluble in these cases only. But the manner in which I have just carried out the solution shows that it is not necessary for the directing curve to be expressed by any equation, and the shape of the curve is itself enough to let us

1) §§ 29—30 and §§ 36—37 of E213, cited below, p. 259. *Cf.* also §§ 4—5 of E305, summarized on pp. XLI—XLV of my introduction to L. EULERI Opera omnia II 13.

2) EULER is here referring to the work of BERNOULLI described in § 36, below.

infer the motion of the string, without subjecting it to calculation. I will make it plain also that the motion is not the less regular [*i. e.* periodic] than if the initial shape were a sine curve, and thus the regularity of the motion cannot be alleged in favor of the sine curves to the exclusion of all others, as Mr. BERNOULLI seems to claim."

[Thus EULER uses "equation" as does D'ALEMBERT, but while "function" means "continuous function" for D'ALEMBERT, for EULER "function" signifies a possibly "discontinuous" function. In the applications to the vibrating string, it is clear from every one of the many examples and discussions given by EULER that for him a "function" is what we now call *a continuous function with piecewise continuous slope and curvature*[1]). When D'ALEMBERT writes $y = f(x)$, he thinks always of an analytical expression, while EULER by $y = f(x)$ means *a curve given graphically.*] As EULER was to emphasize in his later publications, his construction of the entire solution from the given initial shape and velocity, *being purely geometrical, requires no calculation whatever*. Given the initial shape for 23—25
$0 \leqq x \leqq l$, "one repeats it in the reversed situation on each side . . ., and one conceives the continual repetition of this curve in each direction to infinity according to this same law (Figure 81). Then, if this curve is used to represent the functions found, after the time t has passed the ordinate that will answer to the abscissa x in the string in vibration will be

$$y = \tfrac{1}{2}f(x + ct) + \tfrac{1}{2}f(x - ct) \text{ .''} \tag{268A}$$

1) *E. g.*, reading § 5 and § 6 of E317 (cited below, p. 282) shows that EULER regards an "absolutely arbitrary function" and "any curve . . ., irregular or traced at will" as meaning the same. See also E 322, "*De usu functionum discontinuarum in analysi*," Novi comm. acad. sci. Petrop. **11** (1765), 67—102 (1767) = Opera omnia I **23**, 74—91 (Presentation dates: 9 December 1762, 23 May 1763); in § 3, EULER tells what he means by "discontinuous functions": " . . . their several parts do not belong to one another, but rather are determined by no certain equation . . . Also to be included are the lines commonly called 'mixed', where parts cut off from various curved lines are joined together, or parts of the same line are united in a different way," *e. g.* as in a polygonal line. "And thus even if each part is contained in a certain equation, there is not a single equation for the whole extent..." In § 6 of E 339 (cited below, p. 283), EULER's defense of "infinitely small errors" obviously presupposes piecewise smooth functions. Indeed, *precise definitions were lacking.* That today mathematicians easily see the need for precise definitions (though in the natural sciences, such as physics, this need is felt scarcely if at all), does not relieve the historian of the obligation to infer the sense from the use, just as much for one term as for another.

Here the usual acuteness of BURKHARDT lapses: While correctly inferring the meaning of "continuous", he fails to do so for "function" and instead joins the historical tradition in selecting EULER as the scapegoat of the century, to be reproached for applying the calculus to "arbitrary" functions (§ 6 of *op cit.*, p. 11). It is to be remarked that, in contrast to D'ALEMBERT's confusion, *no error results* from EULER's failure to supply precise definitions. (*Cf.* also my Introduction to L. EULERI Opera omnia II **13**, LXI—LXII.)

EULER is to explain this construction again and again in succeeding papers, [but nevertheless the acoustical literature attributes it to THOMAS YOUNG[1]).

While the difference between EULER's view and D'ALEMBERT's might seem a matter of pure mathematics, in fact it is the very opposite. Today it is plain that *the phenomenon of wave motion contradicts LEIBNIZ's law.* This was surely not obvious to NEWTON despite his enormous physical insight, nor to any other early physicist; rather, it is a discovery of EULER, by purely mathematical means. The differential equation (251) certainly has solutions that are not analytic; D'ALEMBERT's formula (257), as EULER interprets it, gives them at will. If (251) is the entire statement of the physical principle governing the motion of the vibrating string, then it follows that non-analytic functions occur in the solutions of physical problems. Since to this everyone today agrees without question, it is now hard to understand that EULER's refutation of LEIBNIZ's law was *the greatest advance in scientific methodology*[2]) in the entire century. Both EULER and D'ALEMBERT realized immediately what was at issue in the otherwise rather tedious problem of the vibrating string. This is the only scientific reason for the sharpness of the controversy that EULER and D'ALEMBERT were to carry on until their deaths at the end of the century.

EULER's first memoir contains other results of value.] First, EULER disposes of the old error[3]) of TAYLOR, recently criticized by D'ALEMBERT [but perhaps still shared by

1) *E. g.* RAYLEIGH, who gives an obscure explanation in §§ 145—147 of *The theory of sound*, 1877. In § 396 of his *Lectures* (cited below, p. 403), YOUNG presents EULER's method in his own typical language: "When a uniform and perfectly flexible chord, extended by a given weight, is inflected into any form, differing little from a straight line, and then suffered to vibrate, it returns to its primitive state in the time which would be occupied by a heavy body falling through a height which is to the length of the chord as twice the weight of the chord to the tension; and the intermediate positions of each point may be found by delineating the initial figure, and repeating it in an inverted position below the absciss, then taking, in the absciss, each way, a distance proportionate to the time, and the half sum of the corresponding ordinates will indicate the place of the point at the expiration of that time." In YOUNG's proof, which is incredibly elaborate, "it may easily be conceived" that a sum of sines may "approximate infinitely near to any given figure."

2) It was unanimously opposed in 1750. By 1810 it was unanimously accepted. See below, p. 295. It is easier to understand the initial opposition to "discontinuous" functions when we recall that the solutions of ordinary linear differential systems with analytic coefficients are analytic functions. No boundary condition can introduce singularities. Thus all the functions arising in the older type of mechanical problem, governed by ordinary differential equations, are indeed "continuous", so that it was most natural, in 1750, to expect that the occurrence of singularities was determined by the differential equation itself. But in so simple a differential equation as $u_x = 0$, an equation with constant coefficients, the general solution is $u = F(y)$, where F is an *absolutely arbitrary* function, about which the differential equation gives no information whatever, while boundary conditions may introduce any sort of singularity. Among all savants of the eighteenth century, only EULER grasped this fundamental truth and understood some aspects of its meaning for physical problems.

3) That EULER had shared this error on 17 March 1747 is shown by his letter to CRAMER of that date.

DANIEL BERNOULLI], that an initially complex vibration will settle with time into a sinus-
oidal one. Indeed, "...if one single vibration conforms to this rule, so must all the 2—3
following ..., and *vice versa*, by the state of the following ones one may conclude the dis-
position of those that preceded. Therefore, if the following vibrations are regular, it cannot
be that the preceding ones have violated this rule; whence it is plain that if the first vibra-
tion were irregular, the following ones could never reach a perfect regularity. But the first
vibration depends upon our choice, since before letting go of the string we may give it any
shape we choose." This is borne out by the explicit solution. 27

From $(264)_1$ and (266) EULER observes that all possible motions of the string are 27
periodic *in time*[1]: "... whatever be the shape of the vibrating string, the vibrations will
not fail to be rather regular, for when we put $ct = 2l$, the string returns to its first con-
dition ..." Thus *in all cases the period is given by TAYLOR's formula* (73), "just as if it
executed its vibrations according to the law of TAYLOR." Moreover, if the initial shape is 28
symmetrical about its middle and if $V = 0$, then the string occupies the straight line
$y = 0$ in the middle of each vibration; thus sinusoidal forms are *not the only ones* having
this property [assumed as the basis of the older investigations of oscillating systems, *cf.*
§§ 13—14 above].

Even though the period in general does not depend on the initial shape, "nevertheless 29
there are singular cases in which the time of vibration can be reduced to the half, the third,
the fourth, or even to any aliquot part of the [fundamental period]." If the string "is
curved initially in such a way as to consist in two parts ... perfectly like and equal to one
another, it will then execute its vibrations as if it were only half as long, and consequently
the vibrations will then be twice as rapid. In the same way, if the initial shape has three
like and equal parts ..., the string will then vibrate as if its length were one third as great,

1) In § XX of his original paper, D'ALEMBERT had come very close to this result, asking "If one wishes to know when will be the times when the string is rectilinear ...," and concluding that "the string will assume a rectilinear form after each time t that contains a certain number of times exactly the time $l\sqrt{\sigma/T}$." While the general proof of the periodicity and calculation of the period thus lies within his hands, D'ALEMBERT goes no further and seems not to see the meaning or importance of the question until after EULER's paper had appeared. In § III of his paper of 1750, cited above, p. 241, D'ALEMBERT, after calling attention to the passage we have just quoted, writes, "this equation will hold, at any rate, if the shape of the string is included in the general equation I determined in my memoir. It is even probable that, more generally, whatever shape the string takes on, the time of one vibration will always be the same; this is what experience seems to show; but it would be difficult, perhaps impossible, to prove it rigorously from theory." Since the inference is so straightforward from the equations, D'ALEMBERT's doubt must be interpreted as relative to its *generality* ("rigor"), since this may be no greater than the generality of the solution, which D'ALEMBERT always contests.

However, DIDEROT wrote in 1748 (p. 21 of *op. cit. ante*, p. 242), "From a memoir which Mr. D'ALEMBERT has sent to the Academy of Berlin, one infers" that the time of a vibration is always the same, "whatever shape be assumed by the string."

and each vibration will be three times shorter; . . . *etc.* . . ." [Thus EULER proves the existence of *special* solutions having frequencies ν_k given by

$$\nu_k = k\nu_1, \ k = 2, 3, \ldots, \tag{269}$$

and these solutions he characterizes correctly. While DANIEL BERNOULLI and EULER had long known the simple modes for the vibrating string, and BERNOULLI had described them in words (above, pp. 158, 180), by a curious oversight no mathematical analysis had yet been published. Thus EULER is not only *the first to obtain the proper frequencies on the basis of the equations of motion*, but also he is the first to publish any mathematical theory of the overtones of a string. Moreover, the present argument, unlike that based on considering one simple mode at a time, shows rigorously that *the frequency is* $k\nu_1$ *if and only if there are* $k-1$ *nodes.* It is this, neither more nor less, that was shown by the experiments of SAUVEUR (above, § 15), which are thus fully explained by EULER's theory[1]).]

30 Pursuing this idea, EULER takes up "some special cases when the curve . . . is continuous, with its parts connected by the law of continuity, so that its nature can be ex-
31 pressed by an equation." Such a solution is

$$y = \Sigma \mathfrak{A}_n \sin \frac{n\pi x}{l} \cos \frac{n\pi c t}{l}, \tag{270}$$

where EULER does not specify whether the sum be finite or infinite. The initial shape is then

$$Y = \Sigma \mathfrak{A}_n \sin \frac{n\pi x}{l}. \tag{271}$$

If there occurs only the term multiplied by $\mathfrak{A}_2$, or by $\mathfrak{A}_3$, *etc.*, the frequency is ν_2, ν_3, *etc.* These are special cases of the motions having the frequencies ν_k described above. [Thus EULER is *first to publish the formulae for the simple modes of a string* and to observe that these may be combined simultaneously with arbitrary amplitudes. While a few years before he had obtained the *general* solution of a problem of finitely many degrees of freedom by the similar formula (244), here he regards the solution (270) as only a special one.]

35. EULER's "first principles of mechanics" (1750). [Before facing the tempest about to break, we must notice a research that will change the whole face of mechanics.

We have seen how the motion of flexible or elastic lines was studied on the basis of special hypotheses, without the general equations of motion. Thus while the simple modes and proper frequencies of small transverse vibration for an elastic rod, for example, had been discovered and calculated correctly, the partial differential equation governing the general vibration of a rod remained unknown, so that it was impossible to begin mathe-

1) There is nothing in any experimental result prior to 1800 that indicated specifically a simple harmonic motion. Expressed in terms of harmonic analysis, the old experiments give no information regarding the presence or absence of the higher overtones corresponding to the frequencies $rk\nu_1$ when the observed frequency is $k\nu_1$.

matical treatment of the initial-value problem. Even for discrete constrained systems of two or more degrees of freedom the problem of setting up the equations was a difficult one, to which much literature was devoted, and the first realization that "NEWTON's equations" in rectangular Cartesian co-ordinate suffice to obtain the equations of motion appears in EULER's papers of 1744—1748 (above, §§ 30—31). EULER himself had obtained a partial differential system for the continuous string only by a limit process from the discrete case, and only in certain corollaries rather than in the basic equations (above, pp. 226—228). In the mechanics of fluids it was just the same[1]). Thus when D'ALEMBERT derived the partial differential equation (251) for the string by simple application of TAYLOR's long known formula for the restoring force to NEWTON's second law, it must have had a sensational effect[2]).

We may justly wonder that it took more than sixty years for so simple an extension of NEWTON's ideas, but the literature of mechanics does not permit us to doubt that it did. As often happens in the history of science, the simple ideas are the hardest to achieve; simplicity does not come of itself but must be created. EULER's researches had moved slowly closer to the general principle of linear momentum, and D'ALEMBERT's work on the string, following upon JOHN BERNOULLI's formulation of hydraulics[3]), must have made it finally obvious to him.] EULER's *Discovery of a new principle of mechanics*[4]) sets down as the axiom which "includes all the laws of mechanics" the momentum principle in the now familiar form

$$(272) \qquad M\frac{d^2x}{dt^2} = F_x, \quad M\frac{d^2y}{dt^2} = F_y, \quad M\frac{d^2z}{dt^2} = F_z$$

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1) *Cf.* pp. XLII—XLIII of my Introduction to L. EULERI Opera omnia II 12.

2) His earlier derivation of (157F), however, appears to have escaped notice prior to the present study.

3) P. XXXVI of *op. cit.* in footnote 1. EULER's own first uses of the momentum principle to derive differential equations of motion all yield only ordinary differential equations:

1. Linked systems of n degrees of freedom, from 1744 (*cf.* §§ 30—31, above).
2. Hydraulics, from 1749 (*cf.* pp. XLIV—XLV of the work cited in footnote 1).

4) E 177, "*Découverte d'un nouveau principe de mécanique,*" Hist. acad. sci. Berlin [6] (1750), 185—217 (1752) = Opera omnia II 5, Presentation date: 3 September 1750.

The importance of this paper is reflected in the English extract, E 177 A, "*Of the general and fundamental principles of all mechanics, wherein all other principles relative to the motion of solids or fluids should be established, by M. EULER, extracted from the last Berlin Memoirs,*" Gentleman's Mag. 24, 6—7 (1754). In this miscellaneous magazine, the contents of which range from heraldry to midwifery, the reviewer translates EULER's work into the notation of fluxions and explains the units used. We are surprised to find understanding unmixed with the sarcasm usually directed toward Continental efforts by English writers of the period: "Consequently the principle here laid down comprises in itself all the principles which can contribute tó the knowledge of the motion of all bodies, of what nature soever they be."

where $\boldsymbol{F}$ is the total static force acting upon the body of mass M, and where it is stated explicitly that in a continuous body M and $\boldsymbol{F}$ are to be replaced by differential elements dM and $d\boldsymbol{F}$.

[The expression of the laws of motion in *rectangular Cartesian co-ordinates* is also of the greatest importance. Today this possibility is so obvious that many scientists seem to believe that NEWTON himself used Cartesian co-ordinates, but of course this is not so,] and LAGRANGE, in 1788 still fairly close to the discovery, after describing the intrinsic resolution always used by NEWTON wrote[1]) "Nevertheless, it is much simpler to refer the motion of the body to directions fixed in space." After stating (272) in words, he added, "this manner of determining the motion of a body impelled by arbitrary accelerating forces is by virtue of its simplicity preferable to all others. It seems that MACLAURIN was the first to employ it, in his *Treatise of Fluxions*, printed in 1742; now it is universally adopted." [The attribution to MACLAURIN is false, however[2]); the method was first used by JOHN BERNOULLI (above, pp. 184—185) and was developed in EULER's papers on special systems having many degrees of freedom (above, §§ 30—31). The importance of the use of Cartesian co-ordinates lies deeper than in mere simplicity; in these co-ordinates the addition of vectors located at different points is so natural as to become customary at

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As we have seen, EULER's first use of (272) for mass-points was in 1744, somewhat foreshadowed by JOHN BERNOULLI's work of 1742. So far as I know, the earliest *statement* of (272) as a *general principle* for mass-points is given in § 18 of E 112, "*Recherches sur le mouvement des corps célestes en général*," Mém. acad. sci. Berlin [3] (1747), 93—143 (1749) = Opera omnia II 25, 1—44. The date of this work is 8 June 1747. In § 22 EULER explains that *the novelty is not the principle itself but the fact that it is general:* "The foundation of this lemma is nothing else than the known principle of mechanics, $du = pdt$, where p is the accelerating power and u is the velocity ... But some reflexion is necessary before one can see that this principle holds equally for each partial motion into which the true motion is thought of as reduced. Moreover, this lemma includes all the principles ordinarily used in the determination of curvilinear motions."

EULER's statement is misleading in its modesty. All of NEWTON's brilliant work on the problem of three bodies was done by the aid of inequalities, groping approximations, and physical insight alone, *without the equations of motion*. To get results by such means required the genius of a NEWTON. His disciples, who might reasonably have been expected to build upon his foundation, did not raise the structure an inch higher. Real further progress came only after the equations of motion had been discovered.

1) *Méchanique Analitique*, Seconde Partie, Section Première.

2) In the book of MACLAURIN, cited above, p. 150, is neither any general statement of the laws of mechanics nor any example formulated in Cartesian co-ordinates. The whole book is a defense of NEWTON's views; thus it is not surprizing that in order to find the center of oscillation, which NEWTON never treated, MACLAURIN does not use any of the ideas of the Basel school or of D'ALEMBERT but, in effect, reverts to the special considerations of HUYGENS.

The reference is one of the rare cases in which LAGRANGE's reluctance to cite EULER carried him to a flat mis-statement; it has been repeated again and again in literature on the history of mechanics.

once, and the possibility of performing this addition lies at the heart of classical the conception of space-time[1]).]

Since the statical forces were already calculated for a variety of mechanical systems, the momentum principle (272) made it possible for EULER to write down the corresponding partial differential equations at once. From this point on[2]) EULER discards all the special mechanical axioms used in his earlier works, the reader of which has grown accustomed to expecting at the head of each a regretful admission that "the principles of mechanics" or the "principles of mechanics and the science of analysis" are not sufficient to determine the motion in general; from now on it is to be only "want of analysis" that holds up the complete solution. Solely[3]) by "adroit" application of the momentum principle (272), which he calls "the first principles of mechanics," from now on EULER is to obtain the general equations governing each mechanical system he treats. [EULER's method is the one used oftenest today. One interpretation of (272) is usually called "D'ALEMBERT's principle" :] *If a body of mass M may be in equilibrium under the system of forces $\boldsymbol{F}$, then to obtain its equations of motion it suffices to replace $\boldsymbol{F}$ by $\boldsymbol{F} - M\boldsymbol{A}$, where $\boldsymbol{A}$ is the acceleration of the material point on which $\boldsymbol{F}$ acts*[4]). This form is often used by EULER.

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1) Consider, for example, the steps in reaching a general view of the motion of the center of mass of a system.

In Corollary IIII to the Laws of Motion in *op. cit. ante*, p. 56, NEWTON wrote in 1687, "The common center of gravity does not alter its state of motion or rest by the actions of the bodies among themselves; and therefore the common center of gravity of all bodies acting upon each other (excluding external actions and impediments) is either at rest or moves uniformly in a right line." The long and involved proof, not mentioning constraints, demonstrates much less than is asserted.

MACLAURIN in § 511 of *op. cit. ante*, p. 150, also gives no real proof at all (1742), but his wording is of interest: "If there was any action without an equal and contrary reaction, the state of the system would be affected by it. And the equality of these being constantly confirmed by experience, it is not without ground that it is held to be a general law ..."

The restricted nature of D'ALEMBERT's statements (1743) has been noted above in footnote 1, p. 188. He is just, however, and so is LAGRANGE (*loc. cit.*, p. 252), in claiming that he has proved more than NEWTON did.

Once the laws of mechanics are stated in the form (272), however, discovery of the properties of the total momentum, moment of momentum, and kinetic energy for any system of mass-points becomes trivial, as any beginner knows. We have remarked in §§ 30—31 upon the easy occurrence of the corresponding integrals in EULER's papers of 1744—1748.

2) On this basis, EULER's papers and even short notes on problems of motion can always be dated at a glance as before or after 1750.

3) Supplemented as needful by the principle of moment of momentum, already used in EULER's work on linked bars (§ 30, above) and later to be given a general form by him. *Cf.* C. TRUESDELL, "*Neuere Anschauungen über die Geschichte der allgemeinen Mechanik,*" Z. angew. Math. Mech. 38, 148—157 (1958).

4) As is evident from the existence of frictional forces, this principle is false in general, though sufficient for "perfect" materials.

While most of Euler's immediate effort is directed toward rigid bodies and perfect fluids, from his notebooks we know that by use of (39) he at once obtained the equations (227) (228) of finite motion for a flexible string[1]; by use of old results giving the restoring forces (above, pp. 160—163), the differential equations for a hanging cord loaded by two or three weights[2]; by use of (130), the partial differential equation for small transverse oscillations of an elastic rod[3]:

$$\frac{1}{c^4}\,\frac{\partial^2 y}{\partial t^2}+\frac{\partial^4 y}{\partial x^4}=0\,. \tag{273}$$

These results he did not publish until many years later.

36. Daniel Bernoulli's memoirs on the composition of simple modes (1753). [Infuriated by the papers of d'Alembert and Euler on the vibrating string, Daniel Bernoulli hastened to describe and publish the ideas he had had for many years[4]).]
I His *Reflections and enlightenments on the new vibrations of strings presented in the memoirs of*

1) P. 320 of EH 5. Here Euler attempts to derive a differential equation for the slope. As we have shown, Euler's published system (222) is equivalent.

2) Pp. 174 and 176—178 of EH 6.

3) P. 175 of EH 6; also p. 80 of EH 8.

4) Daniel Bernoulli's first reaction to d'Alembert's piece is given in his letter of 26 January 1750 to Euler: "I cannot grasp what Mr. d'Alembert intends to say . . . with his infinitely many isochronous vibrations and curvatures . . . He always stays in the abstract and never gives a specific example. I should like to know how he can produce from a string whose fundamental sound is 1 any other sound than 1, namely 2, 3, 4 *etc.* He has tried to ape you; but in his production one sees his taste and little reality." (For substantiation of Bernoulli's specific criticism, note what we have said, above, p. 245, in regard to d'Alembert's article in the Encyclopaedia.)

By the time Euler's paper appeared, there had been a break between him and Bernoulli. In a letter of 7 October 1753, probably addressed to John III Bernoulli and intended for Euler, Daniel Bernoulli writes "I assure Professor Euler of my respects and my perfect esteem. After having read all that he and Mr. d'Alembert have written in the Memoirs of Berlin concerning the new vibrations of strings, I have prepared . . . a memoir which, in my opinion, can explain everything difficult or in any way mysterious in this subject, making it in fact very simple. If Mr. Euler has not lost his taste for these researches, I can have my memoir copied and sent to him . . . I should like to know also what has been printed up to now in the Petersburg memoirs. I have twelve volumes of the old and one of the new. I am surprised not yet to find therein my pieces on the vibrations and sounds of springy bands, while those who have treated this subject after me have published their memoirs long ago. I beg that Mr. Euler inform me if my two treatises, which cost me such thought and trouble, will be printed in the Petersburg memoirs or not. In the latter case I will send them to Paris or to Berlin."

The pieces of his own to which Bernoulli refers are those printed in vol. 13, whose title page bears the date 1751. The reflection on Euler can refer only to the *Additamentum*, published in 1744; as we have seen above, p. 200, it was at Bernoulli's suggestion that Euler added this material to the *Methodus Inveniendi*. Euler's first treatment, E 40, had indeed appeared in 1740, but it had been received in 1735, while Daniel Bernoulli's papers were not sent in until 1742. For details see § 23 above.

the academy for 1747 and 1748[1]) begin by recalling that TAYLOR "proved" the form of a string to be a sine curve; "also, in my opinion, it is only in this form that the vibrations may become regular, simple, and isochrone . . . With this idea, which I have always had, I could only be surprised to see in the memoirs for 1747 and 1748 an infinity of other curvatures [given] as endowed with the same property; nothing less than the great names of Messrs. D'ALEMBERT and EULER, whom I could not suspect of any inadvertence, forced me to investigate whether there could not be some equivocation in the addition of all these curves to that of Mr. TAYLOR, and in what sense they could be admitted. I saw at once that one could admit this multitude of curves only in a sense altogether improper. I do not the less admire the calculations of Messrs. D'ALEMBERT and EULER, which certainly include what is most profound and most advanced in all of analysis, but which show at the same time that an abstract analysis, if heeded without any synthetic examination of the question proposed, is more likely to surprise than enlighten. It seems to me that giving attention to the nature of the vibrations of strings suffices to foresee without any calculation all that these great geometers have found by the most difficult and abstract calculations that the analytic mind has yet conceived." [It is in this wordy sarcasm that the whole memoir is presented.]

BERNOULLI attributes to TAYLOR the whole sequence of simple modes and proper II
frequencies for a string. [While TAYLOR *might* have derived these, in fact he did not say a word about them; that DANIEL BERNOULLI himself has had these results for a long time is shown by the passages quoted above, pp. 158, 180.] These modes, illustrated by figures, III
are "not only an abstract truth" but also can be produced experimentally. "This infinite multiplicity of vibrations manifests itself in all sounding bodies, whatever their nature." The different [harmonic] sounds of horns, trumpets, and traverse flutes follow this same progression 1, 2, 3, 4, . . ., but the progression is different for other bodies; for a closed pipe the progression is 1, 3, 5, 7, ... It is also possible that the sounds have "such a proportion as cannot be expressed by any formula in finite quantities," as is the case for "the sounds, which I calculated formerly[2]), that can be produced in a steel rod struck by light blows . . .

"My conclusion is that every sonorous body contains potentially an infinity of sounds IV
and an infinity of corresponding ways of making its regular vibrations; finally, that in each different kind of vibration the bendings of the parts of the sonorous body occur differ-

1) "*Réflexions et éclaircissemens sur les nouvelles vibrations des cordes exposées dans les mémoires de l'académie de 1747 & 1748,*" Hist. acad. Berlin [9] (1753), 147—172 (1755). This paper and its sequel were received in Berlin before 25 April 1754, the date of EULER's written comments cited below, p. 259.

2) "That was a new problem, which required much circumspection; after having solved it, I proposed it to Mr. EULER, who gave a solution agreeing perfectly with mine, though at first incomplete in that he had left out half of the possible sounds; I told him about this, and he has corrected it in the Leipzig Proceedings." Such is the level to which DANIEL BERNOULLI has fallen.

V ently . . ." The string is not restricted to some one simple mode, "but also it can make a mixture of all these vibrations with all possible combinations; and moreover all these new curves and new kinds of vibration given by Messrs. D'ALEMBERT and EULER are absolutely nothing else than a mixture of several kinds of TAYLOR's vibrations. If that is true, I could not approve the conglomeration of all these new curves, since then . . . the string would not emit one and only one tone, but several at once." Such a vibration one could not
VI call isochrone. The existence of harmonic sounds heard simultaneously with the fundamental is a proof of the existence of these compound vibrations. "If one holds a steel rod in the middle and strikes it, one hears at the same time a confused mixture of several sounds, . . . extremely dissonant, . . . a contest of vibrations that never stop or begin at the same time except through a great chance. Thus . . . the harmony of sounds heard simultaneously in a sounding body is not essential to the matter and should not serve as a principle for musical systems . . . What proves best that the various undulations of the air do not interfere with one another is that at a concert one hears all the parts distinctly . . ."

VIII BERNOULLI illustrates the type of forms that result by superposition of two simple modes. [There are also repetitions of earlier work[1]) and quibbles over the usage of "isochrone".]

XIX—XX BERNOULLI then attempts a general proof of the principle of superposition of isochronous oscillations. 1°, for a motion on a line to be isochronous, the force must be proportional to the distance from a fixed point. 2°, two such forces superposed produce two such motions along different lines. [There is a germ of truth in the idea, but the concept of the problem is far from sufficient. There follow pages of calculation leading to nothing.]

XXI—XXVIII BERNOULLI considers all the results of D'ALEMBERT and EULER explained by his method; if their method "is much more difficult than mine, I admire but the more the superiority of their genius. As for the question whether the new vibrations are really . . . simple and isochrone . . ., or rather are a mixture of several different coexistant vibrations . . ., I have spoken of this only so as better to explain the nature of these vibrations, being far removed from raising an issue with such great men concerning the meaning of certain terms."

Immediately following is his second memoir, *On the mixture of several kinds of simple isochronous vibrations which can coexist in the same system of bodies*[2]). The beginning calculates the simple modes of the weighted string by the old static method [due essentially to

1) In § VII BERNOULLI still "proves" the principle of superposition by saying that since the amplitude of one mode is assumed small, the string is virtually straight, and thus another mode may occur as a vibration about equilibrium. In § XV he again expresses the opinion that the higher modes are more rapidly damped.

2) "*Sur le mêlange de plusieurs especes de vibrations simples isochrones, qui peuvent coexister dans un même systême de corps*," Hist. acad. sci. Berlin [9] (1753), 173—195 (1755).

John Bernoulli[1])]. These modes are then combined so as to fit arbitrary initial displacements with zero initial velocities. Bernoulli considers these results as showing that the compound oscillations are not "regular". While Bernoulli asserts that "... had any X attention been paid to our method, it would have been seen that our theory applies to any number of bodies," he here considers only the cases of two and three masses.

"What I have just said on the nature of the vibrations of bodies attached to a stretched XIII, XVIII string I do not hesitate to extend to all small reciprocal motions that can occur in nature, providing these ... are set up by a permanent cause. For every body that is somewhat displaced from its point of rest will tend toward that point with a force proportional to the small distance from the point of rest." The number of kinds of "simple and regular vibrations" equals the number of bodies in the system. "All these simple and special vibrations do not hinder one another at all, and they will subsist as long as the primitive and permanent cause of these vibrations persists ..." This is "a new truth of mechanical physics."

The paper closes by using the simple modes determined long ago so as to calculate the XIV—XVII motion of a hanging cord loaded by two weights and released from an arbitrary displaced configuration. The general motion is of course not periodic.

Bernoulli's views are expressed somewhat more clearly in a letter[2]). Referring to the trigonometrical series (271), he writes, "But cannot one say that this equation includes all possible curves? By means of the arbitrary constants ... can we not pass the curve through as many assigned points as we like? Has an equation of this sort less extent than the indefinite equation $y = \alpha x + \beta x^2 + \gamma x^3 +$ etc.? On this basis have you not proved your beautiful theorem that every curve has the property in question? [What theorem?] Thus to solve your problem: *Given any initial shape, to find the following motion,* I say that we must determine the quantities $\mathfrak{A}_n$ so as to render our indefinite equation the same as the given curve, and one will have simultaneously the special isochrone vibrations of

1) Daniel Bernoulli is barely correct in saying in § II "I do not remember having seen the solution of this problem, but if any one has given it, I believe that his solution will have consisted only in an analytic expression, very far from letting us know the true nature of these motions; I believe even less that anyone has ever solved this problem when there are arbitrarily many bodies spaced at arbitrary distances ..." Daniel Bernoulli generalizes John Bernoulli's equation (79) to the case of unequal weights and unequal spacing, and for the cases of two and three weights he determines all modes. As for the "analytic expression", Euler's general differential equations (209) were in print, as were his general solutions for the mathematically analogous problem of longitudinal elastic vibration of 1, 2, 3, and 4 not necessarily equal masses, given in §§ 3—29 of E 136, summarized above in § 31 and in print when Bernoulli wrote.

2) Undated, published by Fuss, *op. cit. ante*, p. 165, 2, pp. 653—655, probably addressed to John III Bernoulli and intended for Euler. Fuss places this letter between 1754 and 1766; it seems to have been written after the two papers had been received in Berlin and before Daniel Bernoulli had seen Euler's comments printed in 1755; thus it dates from 1754—1755.

which the desired motion is composed. If, by my method, I have been able to . . . determine the motion of a stretched thread loaded at arbitrary points by any number of weights having arbitrary masses, it seems to me that this problem has greater extent than yours.

"But it is not in this kind of abstract question that I consider the usefulness of my new theory to lie. I admire more the physical treasure which was hidden, that natural motions which seem subject to no law may be reduced to the simple isochrone motions which it seems to me nature uses in most of its operations. I am convinced even that the inequalities in the motions of the heavenly bodies consist only in two, three, or more simple reciprocal motions of different duration and excursion, by which the bodies seem to be alternatively accelerated or retarded and which can coexist in one and the same body while it moves subject to KEPLER's laws; for small forces which are sometimes positive and sometimes negative can hardly produce anything but reciprocal and isochronous motions. Finally I remark in respect to the shape of a stretched string, at least when it is given [as] a curve immediately included [in (271)], that each element of the curve has to make an infinity of infinitely small vibrations, all different among themselves, during a total vibration."

[Had BERNOULLI's two papers been published when their contents were conceived, *i. e.* in 1734—1739, they would have earned a great place in the history of mathematics, especially had they been expressed in the brief style of BERNOULLI's first note on the hanging chain. His boasts of what he can do *without* calculation are in some measure just but would be more convincing if he had refrained from adding pages of calculation to no real end. BERNOULLI has learned nothing in the past decade. That by superposing simple modes he can hack his way to a solution of the initial value problem for systems of two or three degrees of freedom is by now scarcely illuminating. To justify his viewpoint, what is needed is explicit, formal solutions to the initial value problem for the general weighted string and for the continuous string by superposition of harmonic oscillations, along with some measure of analytical rigor, and from both these ends he is hopelessly distant.

The "abstract" mathematics about which BERNOULLI is so sarcastic consists in (1) the concept of partial derivative, (2) the concept of real function of a real variable. These he now and henceforth refuses to recognize. Hence all problems of continuum mechanics based on the governing partial differential equations are forever closed to him. It follows *a fortiori* that proof of his "new truth of mechanical physics" is out of the question. Beyond the heuristic but not compelling observation that for small displacements from equilibrium the restoring forces are linear[1]), the "new truth" will have to remain for BERNOULLI an in-

1) Anything so general as the consequence that the governing partial differential equations are of the form

$$\frac{\partial^2 y}{\partial t^2} = \text{linear expression in space derivatives}$$

is impossible within BERNOULLI's mathematical limitations.

dependent principle of physics rather than a demonstrated consequence of the general laws of mechanics.

The principle of superposition of small harmonic oscillations, indeed of great usefulness in mechanics, is here stated in general for the first time. Granted that to Bernoulli it appears a physical rather than mathematical truth, we should expect some good examples calculated *approximately; e. g.*, an approximate determination of the amplitudes of the first few harmonics of string whose initial form is triangular. Of such an example there is no hint; rather, Bernoulli's laborious exact treatment of systems of two and three degrees of freedom shows that his *mathematical* thinking did not tend toward practical examples. With a clear and sound grasp of the experimental phenomena, he nevertheless failed to bend even his own simple theoretical concepts toward experimentally realizable cases other than those restricted to simple modes or to very simple systems.

It is no wonder that these papers, failing on the one hand to meet the mathematical standards of 1750 and on the other hand to produce any new results that could be compared with important experiments, found little response other than criticism.]

37. Euler's second memoir (1754): the central importance of the partial differential equation. Immediately following the two papers of Bernoulli appear Euler's *Remarks on the preceding memoirs*[1]). [Although most of this somewhat testy reply to the criticisms of d'Alembert and Bernoulli is but a reaffirmation, in clearer terms and with better explanation, of Euler's earlier stand, even in this there is value.] While in his first memoir Euler had followed d'Alembert in regarding the partial differential equation (251) as an intermediate step, now, after the "first principles of mechanics[2])" (above, § 35), he realizes *that in the partial differential equation lies the whole theory of the vibrating string.* "See then 22
to what the problem of the motion of the string has been reduced." We have only to find a solution $y(x, t)$ of (251) subject to appropriate boundary and initial conditions. Let us first seek "all possible functions[3])" satisfying (251). The special character of the solutions ob-

1) E 213, "*Remarques sur les mémoires précédens de M. Bernoulli,*" Hist. acad. sci. Berlin [9] (1753), 196—222 (1755) = Opera omnia II **10**, 232—254. Presentation date: 25 April 1754.

2) The derivation (§§ 17—18) of (251) is still incomplete, since Euler follows d'Alembert in assuming rather than proving that $T = \text{const}$. In contrast to his earlier work (*cf.* § 20 above), Euler now asserts (§§ 13—14) that the assumptions of small motion and perfect flexibility "are made only for ease of calculation; for it is indeed possible to take into account the stiffness of the string and its stretching during the motion, and to allow a finite magnitude to the vibrations; but one would arrive at formulae so complicated as to allow no satisfactory conclusion to be drawn. It is not the principles of mechanics that abandon us . . ., but rather analysis . . ."

In §§ 25—27 Euler gives a new method of deriving (257); as d'Alembert was to remark (below, p. 274), it is faulty.

3) "This is the problem for which Mr. d'Alembert was the first to obtain the general solution;

tained by earlier authors resulted from their having added unnecessary and restrictive hypotheses, such as that the curvature shall be proportional to the displacement or that the solution be "continuous".

22—25 Second, in this paper the equation (251) for the vibrating string *first appears in the notation of partial derivatives.* [Generally in this history I pass over differences of notation, preferring to use modern symbols so as to show most immediately the *ideas* of the creators. Here, however, we must remark the difference between a notation that merely accumulates different letters for the various quantities occurring, such as that employed not only by D'ALEMBERT and others but also by EULER in all his previous treatments, and a notation
19—21 emphasizing the *operations* that are performed.] EULER realizes "the great utility in numerous mechanical and hydrodynamic problems" afforded by the notation of partial differentiation, and he gives an explanation of it, emphasizing the convenience resulting from the commutability of partial derivatives.

1 "There is no doubt that Mr. BERNOULLI has developed the part of physics concerning the formation of sound infinitely better than had any other before him. Previous work . . . stopped short at the mechanical determination of the motion . . ., without looking sufficiently into the nature of the sounds that can be produced from it. Despite the infinite number of ways . . . found possible for a string to be set into vibration, it was not seen how the same string could emit at one time several different sounds; and it is to Mr. BERNOULLI that we owe this happy explanation . . . It is also plain that this beautiful idea is valid also for all sorts of sounding bodies, and that a given body may emit simultaneously all the
2 different sounds which it can emit separately . . ." BERNOULLI has shown all this on the sole basis of TAYLOR's researches. "He claims, disagreeing with Mr. D'ALEMBERT and me, that the solution of TAYLOR suffices to explain all motions that a string may take on. Thus the curves a string assumes during its motion are always either simple sine curves or a
3 mixture of two or more such curves . . ." If all forms of the string were expressed by (271), the opinion of BERNOULLI would be correct. "But, when the number of terms . . . becomes infinite, it then seems doubtful . . . that one may say that the curve is composed of an infinity of sine curves: The infinite number seems to destroy the nature of the composi-
4 tion . . . When the infinite equation is reducible to a finite one . . ., the equation itself furnishes an idea and a construction much more simple . . ." *E. g.*,

$$y = \frac{c \sin \frac{\alpha x}{l}}{1 - \alpha \cos \frac{\alpha x}{l}}, \quad |\alpha| < 1, \tag{274}$$

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and it would be desirable to discover a method proper for solving other similar formulae. Such a method would serve to solve a quantity of problems which one has been obliged to abandon up to now."

for which an infinite trigonometrical series is known. "This string indeed should emit simultaneously an infinity of sounds, the highest of which will become more and more faint, but the equation offers us a much simpler idea of this curve than if we were to say that it is composed of an infinity of TAYLOR's sine curves."

There follows a discussion of the generality of the functions represented by trigono-
metric series. EULER sees that BERNOULLI's solution would be justified *if every function* 9
could be represented by an infinite trigonometric series. This EULER regards as untrue; [then
comes indeed a great rarity in EULER's papers, namely, a wholly fallacious argument.]
In an attempt to show that (271) is not sufficiently general, EULER [falls into D'ALEMBERT's
error of] treating as relevant the nature of the function that (271) may or may not repre-
sent *outside* the range $(0, l)$. [His remarks make it clear that he has no idea that what we 10
now call a non-analytic function may be represented in a finite range by a trigonometric
series.

Returning to safer ground,] EULER says that his own solution "is not limited in any 11
respect[1]) . . . I do not expect that Mr. D'ALEMBERT will say that . . . the motion . . . does 12
not follow any law; it will then be determinable by its nature[2]), and if my solution is false,
no one will be more capable of supplanting it then Mr. D'ALEMBERT himself."

If P, Q, R are solutions of (251), so also is $\alpha P + \beta Q + \gamma R$, where α, β, γ are arbi- 23
trary constants. " . . . and this same composition holds also in all sorts of vibrations, provided they are infinitely small, since the equation that expresses the motion has only the dimension one in all its terms. Thus it is here that we must look for the true foundation of Mr. BERNOULLI's solution." [To BERNOULLI, the *principle of superposition* is a *law of physics*, formulated from experience; to EULER, it is *theorem*, easily *proved* in all cases when the governing differential equation is *linear*[3]).]

EULER shows that all effects predicted by BERNOULLI, such as the combination of 42
tones, follow equally well from (257). BERNOULLI claimed that for isochronous vibration it is necessary that the restoring force be proportional to the displacement. This yields *one* of TAYLOR's sine curves. If *two* are present, the force is no longer proportional to the displacement. In any case, this is an arbitrary hypothesis, and other forces might be possible.

D'ALEMBERT hastened to attack BERNOULLI with a [vague and wordy] article in

1) " . . . at least, I cannot find any fault in it, and no one has yet shown it to be insufficient. It is indeed true that Mr. D'ALEMBERT, after having reproached me for giving a solution not different from his own, has asserted, but without supplying the least proof, that my solution does not extend to all possible forms that the string may be given initially . . ."

2) *Cf.* also §§ 4—5 of E 322, cited above, p. 247.

3) It is difficult to explain why EULER in his letter of 20 May 1760 to LAMBERT chooses to attribute this result to LAGRANGE in the context of aerial propagation.

the *Encyclopaedia*[1]). "In a string which performs its vibrations freely, we observe no other nodes, or points absolutely at rest, except the ends." *I. e.*, he thinks that the small observed motion of the nodes destroys the validity of BERNOULLI's [and also EULER's] explanation of the overtones. [This is to be refuted many years later by the experiments of M. YOUNG (below, p. 294.]

DANIEL BERNOULLI published a reply in 1758[2]). [Justly] he points to the *generality* of
158 his method [in principle]: " . . . my method may be used to determine the vibrations and reciprocal motions in all systems of bodies for which one can determine the simple vibrations; *i. e.*, those such that all parts execute perfectly synchronous vibrations, each by
164—165 itself and according to the law of the simple pendulum." He asserts that by superposing sine curves "one may cause the final curve to pass through as many given points as one wishes and thus identify this curve with the one proposed, to any desired degree of precision," [but he gives no idea how to adjust the coefficients, even approximately, nor does he give any example. Instead,] *he retreats to the finite*[3]): "My method . . . is general and

1) Art. "Fondamental", vol. 7 (1757). For earlier Encyclopaedia articles of D'ALEMBERT, see above, p. 245, footnote 3.

Having omitted all mention of overtones in his article on strings, he now states that a "body" gives out not only its fundamental but also "other sounds, which are, 1°, the octave above . . .; 2°, the twelfth and major seventeenth . . ." He mentions no other overtones, nor is it clear whether he is referring only to strings or also to other bodies. D'ALEMBERT gives no theory of overtones, but in addition to attacking BERNOULLI's theory he uses much space explaining the view, held by most physicists of the day, that the air is composed of little bodies having many degrees of spring.

2) "*Lettre de Monsieur* DANIEL BERNOULLI, *de l'Académie royale des sciences, à M.* CLAIRAUT *de la même Académie, au sujet des nouvelles découvertes faites sur les vibrations des cordes tendues*," Journal des Sçavans. March 1758, 157—166. In this rather nasty letter directed mainly against EULER, BERNOULLI claims that to understand all, one need but *study* his two memoirs: "I say *study*, since the subject is too new and too ticklish to be grasped by a simple reading. I myself, having no love for abstract and ticklish truths, which I see clearly are useless for knowing better the phenomena and laws of nature, . . ." *etc.* (p. 157). He makes a point of his priority for the hanging cord loaded by two weights and for the transverse vibrations of a rod (pp. 157—158). He explains that "for a long time" he shared the opinion of "the geometers" relative to "the bounds of human capacity": Motions other than the simple modes were "entirely irregular and indeterminable . . . I perceived the terrible complexity of such reciprocal motions, and I did not yet suspect that nature acts by laws so simple as those I have subsequently observed, thus producing effects so complicated in appearance." However, he goes on to say that the coexistence of several modes in a vibrating rod, which he had observed sixteen to eighteen years before, "makes the foundation" of his two last papers (pp. 158—159). The letter closes with sarcastically worded criticisms of EULER's treatment, but these arise from BERNOULLI's misunderstandings.

3) Even in his treatment of the finite case he retreats, for instead of using the differential equations of motion he employs, doubtless deliberately, special principles appropriate to linearized motion, which he speaks of as "a problem presenting itself so naturally that all the geometers must have thought of it, yet no one had solved it" (pp. 160—161). In any case, BERNOULLI merely talks about the problem of the loaded string without really solving it (pp. 161—162). When it comes to fitting the initial con-

exact, so long as only finite quantities are involved." As he was to say later[1]), "physical beings cannot be composed of absolutely vanishing parts." [While this statement, which may be true, is encountered in physical circles even today, there is of course no reason to consider a model consisting in equal and equally spaced punctual masses joined by rigid massless links as nearer to physical matter than is a continuous string. Moreover, as is nowadays obvious, use of discrete rather than continuous models usually complicates rather than simplifies the mathematical problems.]

38. LAGRANGE's first memoir (1759): the explicit solution for the loaded string, and a faulty passage to the limit. [By his entry into the controversy then occupying the principal geometers of the age, a previously unknown young man, JOSEPH-LOUIS DE LA GRANGE of Turin, acquired at once a fame which has lasted until today.] EULER's solution in "absolutely arbitrary" functions LAGRANGE claims to establish without using differential or integral calculus, simply by passing to the limit in the solution for the loaded string. [Today it is obvious such a passage to the limit, if correct, is valid only subject to hypotheses essentially the same as those necessary to justify the direct use of appropriate differentiations and integrations ; thus LAGRANGE's claim cannot be valid. Nevertheless, a major part of LAGRANGE's reputation in mechanics rests upon this paper, cited in every biography or description of his career[2]). This reputation is due partly to the brilliance of his algebraic

ditions, while he says he found "no difficulty" in making a trigonometric polynomial of degree 2, 3, . . . , 7 pass through 1, 2, . . . , 6 equally spaced points, so that "one sees clearly how this division could be continued as far as desired" (p. 165), he gives no example and no indication that he could really solve the problem of finite interpolation.

1) Footnote to § 15 of "*Recherches physiques, mécaniques et analytiques, sur le son et sur les tons des tuyaux d'orgues différement construits,*" Mém. acad. sci. Paris 1762, 431—485 (1764). Here he speaks also of his theorem on the coexistence of small oscillations, "by which I unravelled and explained several . . . very paradoxical theorems, to which Messrs. D'ALEMBERT and EULER had been conducted by a very clever and lofty theory, but at the same time too abstract or metaphysical for one to know its correct application and true meaning."

2) Virtually all of these derive from DELAMBRE's "*Notice sur la vie et les ouvrages de M. le Comte J.-L. LAGRANGE,*" Œuvres de LAGRANGE 1, IX—LI. This obituary falls distinctly into two parts. The first deals with works DELAMBRE shows no evidence of knowing at first hand. This part is absolutely without definite content; while it mentions particular studies, it does so in such general terms as to yield only a sauce of mellifluous eulogy. EULER's overgenerous letters of praise to LAGRANGE, mentioned on pp. XXXVII—XLI of my Introduction to volume II 13, are cited as if they were impartial evaluations by a third party and are made the basis of DELAMBRE's rhetorical elaboration. Referring to LAGRANGE's first memoir, DELAMBRE writes " . . . he establishes more solidly the theory of the mixture of simple and regular vibrations of D. BERNOULLI; he shows the limits within which this theory is exact, and beyond which it becomes faulty; then he comes to the construction given by EULER, a construction which is true, although its author arrived at it only by calculations not at all rigorous; he answers the objections raised by D'ALEMBERT; he shows that whatever form is given to the string, the duration of the vibrations will always be the same, a truth of experience which D'ALEMBERT had

manipulations, partly to the political circumstances that caused both D'ALEMBERT and EULER to court his favor and to give to his work an immediate publicity which otherwise it would scarcely have met. These personal factors, as well as the parts of the paper dealing with the propagation of sound, I have assessed elsewhere[1]).]

In the introduction to his *Researches on the nature and the propagation of sound*[2]), LAGRANGE claims to treat the subject "as entirely new, without borrowing anything from any who may have studied it previously." In regard to the loaded string, "I then undertake to solve this problem, where the analysis seems new in itself and interesting, since an infinite number of equations must be solved at once . . . I first consider . . . the case when the number of bodies . . . is finite, and there I easily derive the whole theory of the mixture of simple and regular vibrations, which Mr. DANIEL BERNOULLI found only by special and indirect means. I then pass to the case of an infinite number of bodies . . ., and after proving the insufficiency of the preceding theory for this case, I derive from my formulae the same construction of the problem of the vibrating string that Mr. EULER has given and that has been so strongly contested by Mr. D'ALEMBERT. But more, I give this construction all the generality of which it is susceptible, and by the application I make of it to musical strings I obtain a general and rigorous proof of that important truth of experience, that whatever be the initial shape of the string, the duration of its oscillations is always the same[3]). . . .

"On this occasion I develop the theory of harmonic sounds resulting from one given

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considered it very difficult or even impossible to prove . . ." DELAMBRE goes on to describe how by "this analysis of the most transcendent kind" LAGRANGE "appears at one blow . . . the equal of NEWTON, TAYLOR, BERNOULLI, D'ALEMBERT, and EULER, as an arbiter who, to put an end to a difficult struggle, shows each in what measure he is right and where wrong, judges them, reformulates [their work], and gives to them the true solutions they have sought but failed to reach." These smacks of truth floating upon outright falsehood are repeated in one form or another in the histories of mathematics. The detailed and impartial analysis of LAGRANGE's paper given by BURKHARDT, §§ 10—12 of *op. cit. ante*, p. 11, seems not to have been noticed by the historians.

The second part of DELAMBRE's obituary, especially pp. XXVII—XXX, concerns LAGRANGE's work on celestial mechanics, which DELAMBRE seems to have studied at first hand. Here DELAMBRE finds little to praise and much to criticize in what he considers an excess of long formal calculation to reach awkward results of doubtful accuracy.

1) Introduction to vol. II 12, p. CXIX; Introduction to vol. II 13, pp. XXXV—XXXIX.

2) "*Recherches sur la nature, et la propagation du son,*" Misc. Taurin. 1_3, I—X, 1—112 (1759) = Œuvres 1, 39—148.

3) A footnote here cites the remark of D'ALEMBERT given in our footnote 1, p. 249 above; LAGRANGE then adds, "I recount here these words of so great a geometer only to give an idea of the difficulty of the problem I have solved." In view of what is said in footnote 1, p. 249 above, and since LAGRANGE has just claimed to establish EULER's form of the solution, from this boast we may conclude only that LAGRANGE had not read EULER's memoir with care.

string, and the same also for wind instruments. Although these two theories have been put forward already, the one by Mr. SAUVEUR and the other by Mr. EULER, nevertheless I believe I am the first who has derived them immediately from analysis. [Again LAGRANGE has not read EULER's paper carefully; as we have seen above, pp. 249—250, EULER's theory of overtones follows immediately and rigorously from the general solution. While LAGRANGE's demand for "immediate" derivation strictly excludes DANIEL BERNOULLI's work, he is rudely unjust in overlooking the debt to DANIEL BERNOULLI's many calculations of simple modes and proper frequencies for various mechanical systems, not only the two which he studies but more particularly those considered in the pioneer researches of 1733—1742, a decade including LAGRANGE's date of birth.]

Despite these claims of originality, the formal developments begin with a derivation of (235) for longitudinal motion of a loaded elastic string; [LAGRANGE does not mention any prior work on this problem, although EULER's paper deriving (235) and solving it for the case of zero initial velocities was then nine years in print[1]).] There follows a derivation of the same system for small transverse motion of a taut loaded string, [*i. e.*, the linearized case of EULER's system (209), then eight years in print].

In a critique of the previous work on the solution of the continuous string[2]) LAGRANGE 11—18, 15
writes, "No one could doubt that in algebraic functions all their different values are joined together by the law of continuity; thus it seems indubitable that conclusions drawn from the rules of differential and integral calculus are always illegitimate . . . when this law is not assumed. Thus it follows that since Mr. EULER's construction is derived directly from integration of the given differential equation, by its very nature this construction is applicable only to continuous curves . . ." [On no very definite grounds,] LAGRANGE convinces 15, 18
himself that the only way to establish a solution for the continuous string is *to pass to the limit in the solution for the loaded string*[3]).

1) In his letter of July 1754 [?] to EULER (Œuvres de LAGRANGE 14, 135—138), LAGRANGE claims to know "almost all" of EULER's work published by the academies of Petersburg and Berlin, whence it is natural to suppose he was at least aware of the existence of E 136, published in 1750 in the Petersburg memoirs. The usual notices on the history of mechanics attribute the entire theory of the loaded string to LAGRANGE, while in fact his contribution is but the last step in a development starting with work of HUYGENS.

2) *E. g.*, in § 14, " . . . since Mr. D'ALEMBERT has not brought forward any special reason to substantiate his objection, neither has Mr. EULER, whence it follows that the question remains still undecided."

3) The idea of such a passage to the limit, as we have seen, is due to HUYGENS (above, p. 49) and had been touched with varying degrees of success by JOHN BERNOULLI and EULER. Recently D'ALEMBERT had revived it, attributing it to himself. In § III of *op. cit. ante*, p. 241, he writes, ". . . if one is to determine the vibrations of the string by the method I explained at the end of my memoir, § XLIV [*i. e.* by considering the loaded string], it does not suffice to consider the string loaded by

[Throughout this paper, the logic is tenuous, but LAGRANGE's idea seems to be that while EULER's *derivation* is restricted to "continuous" functions because it employs differential and integral calculus, nevertheless EULER's *result* may possess unrestricted validity as a limit formula from the discrete case.]

LAGRANGE writes (235) in the form [1])

$$(275)\qquad \left.\begin{aligned} \frac{dy_k}{dt} &= v_k\,, \\ \frac{dv_k}{dt} &= C^2(y_{k+1} - 2y_k + y_{k-1}) \end{aligned}\right\} \begin{aligned} & k = 1,\ 2, \ldots, \\ & y_0 \equiv y_{n+1} \equiv 0\,. \end{aligned}$$

His method rests on determining constant multipliers M_k, N_k, and R such that the identity

$$(276)\qquad \begin{aligned} &\sum_{k=1}^{n} (M_k dv_k + N_k dy_k) \\ &= \sum_{k=1}^{n} [N_k v_k + C^2 M_k (y_{k+1} - 2y_k + y_{k-1})]\, dt \end{aligned}$$

reduces to the form

$$(277)\qquad dz = Rz dt\,, \quad \text{so that} \quad z = 2RKe^{Rt}\ .$$

Therefore

$$(278)\qquad RM_k = N_k,\ RN_k = C^2(M_{k+1} - 2M_k + M_{k-1})\ .$$

Elimination of N_k yields

$$(279)\qquad M_{k+1} - \left(\frac{R^2}{C^2} + 2\right) M_k + M_{k-1} = 0\,.$$

Therefore

$$(280)\qquad M_k = Aa^k + Bb^k\ ,$$

where a and b are the roots of

$$(281)\qquad x^2 - \left(\frac{R^2}{C^2} + 2\right) x + 1 = 0\,.$$

Thus

$$(282)\qquad ab = 1\ ,\quad a + b = \frac{R^2}{C^2} + 2\,.$$

Without loss of generality we may impose the conditions $M_0 \equiv M_{n+1} \equiv 0$, $M_1 \equiv 1$. Then

$$(283)\qquad M_k = \frac{a^k - b^k}{a - b} \quad \text{where} \quad \frac{a^{n+1} - b^{n+1}}{a - b} = 0\,.$$

two or three weights, but it is necessary to take a rather considerable number; otherwise there would be grounds to fear the problem was not solved exactly enough."

1) In following the details in this elaborate and obscure paper I acknowledge a great debt to BURKHARDT, § 10 of *op. cit. ante*, p. 11.

The conditions $(283)_2$ and (282) determine R^2/C^2. Indeed, there are n pairs of values a_r, b_r satisfying $(282)_1$ and $(283)_2$, *viz* $a_r = e^{ir\pi/(n+1)}$, $b_r = e^{-ir\pi/(n+1)}$, $r = 1, 2, \ldots, n$, and hence $M_k = M_{kr}$ where 20—21

$$(284)\qquad M_{kr} = \frac{e^{i\frac{kr\pi}{n+1}} - e^{-i\frac{kr\pi}{n+1}}}{e^{i\frac{r\pi}{n+1}} - e^{-i\frac{r\pi}{n+1}}} = \frac{\sin\frac{kr\pi}{n+1}}{\sin\frac{r\pi}{n+1}} .$$

To satisfy $(282)_2$ we must have $R = R_r$, where $e^{ir\pi/(n+1)} + e^{-ir\pi/(n+1)} = 2 + R_r^2/C^2$; hence

$$(285)\qquad R = \pm 2iC \sin\frac{r\cdot\frac{1}{2}\pi}{n+1} .$$

Thus n sets of multipliers are uniquely determined, so that $(277)_2$ holds in the form 22

$$(286)\qquad z_r = \sum_{k=1}^{n} (M_{kr}v_k + R_r M_{kr} y_k) = 2R_r K_r e^{R_r t} ,$$

where $2R_rK_r$ is an arbitrary constant. Set

$$(287)\qquad Z_r \equiv \sum_{k=1}^{n} M_{kr} y_k .$$

By $(275)_1$ we may put (286) into the form

$$(288)\qquad \frac{dZ_r}{dt} + R_r Z_r = 2R_r K_r e^{R_r t};$$

hence

$$(289)\qquad Z_r = K_r e^{R_r t} + L_r e^{-R_r t} ,$$

where L_r is a further constant of integration. From (285) it follows that

$$(290)\qquad \begin{gathered} Z_r = P_r \cos\left(2Ct \sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right) + Q_r \frac{\sin\left(2Ct\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right)}{2C\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}} , \\ P_r = Z_r(0),\ Q_r = Z_r'(0) . \end{gathered}$$

The problem is now to calculate y_k from (287), where Z_r is given by (290). LAGRANGE sets 23

$$(291)\qquad s_r \equiv Z_r \sin\frac{r\pi}{n+1} = \sum_{k=1}^{n} y_k \sin\frac{kr\pi}{n+1} ,$$

where we have used (287) and (289). This relation LAGRANGE then inverts by a long and ingenious calculation[1]), [which there is no point in following. The problem is simply and directly approached by the method already used by EULER in a special case (above, p. 232)] 24—27

1) It is described by BURKHARDT, § 10 of *op. cit. ante*, p. 11.

and soon to be generalized by LAGRANGE (below, p. 278). We have only to replace EULER's formula (246) by the more general identity[1])

$$\sum_{r=1}^{n} \sin\frac{pr\pi}{n+1}\sin\frac{kr\pi}{n+1} = \tfrac{1}{2}(n+1)\,\delta_{pk}\,. \tag{292}$$

For from (291) and (292) we obtain

$$\sum_{r=1}^{n} s_r \sin\frac{kr\pi}{n+1} = \sum_{r=1}^{n}\sum_{p=1}^{n} y_r \sin\frac{pr\pi}{n+1}\sin\frac{kr\pi}{n+1} = \tfrac{1}{2}(n+1)\,y_k\,, \tag{293}$$

where s_r is to be expressed in terms of P_r and Q_r by means of $(291)_1$ and $(290)_1$. By $(290)_{2,3}$, (287), and (284) we have

$$\begin{aligned} P_r &= Z_r(0) = \sum_{q=1}^{n} M_{qr} Y_q = \frac{1}{\sin\frac{r\pi}{n+1}} \sum_{q=1}^{n} Y_q \sin\frac{qr\pi}{n+1}\,, \\ Q_r &= Z_r'(0) = \sum_{q=1}^{n} M_{qr} V_q = \frac{1}{\sin\frac{r\pi}{n+1}} \sum_{q=1}^{n} V_q \sin\frac{qr\pi}{n+1}\,, \end{aligned} \tag{294}$$

where Y_k and V_k are the given initial values $Y_k = y_k(0)$, $V_k = v_k(0)$. From (293) and (294) follows LAGRANGE's *explicit solution of the initial-value problem for the loaded string:*

$$\begin{aligned} y_k = \frac{2}{n+1} \sum_{r=1}^{n} \sin\frac{kr\pi}{n+1} \sum_{q=1}^{n} \sin\frac{qr\pi}{n+1}\,\cdot \\ \left[Y_q \cos\left(2Ct\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right) + V_q \frac{\sin\left(2Ct\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right)}{2C\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}} \right]. \end{aligned} \tag{295}$$

28 Equivalently,

$$\begin{aligned} y_k &= \sum_{r=1}^{n} \varphi_{rk}\,, \\ \varphi_{rk} &= \sin\frac{kr\pi}{n+1}\left[A_r \cos\left(2Ct\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right) + B_r \frac{\sin\left(2Ct\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right)}{2C\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}}\right], \\ A_r &= \frac{2}{n+1}\sum_{q=1}^{n} Y_q \sin\frac{qr\pi}{n+1}\,, \quad B_r = \frac{2}{n+1}\sum_{q=1}^{n} V_q \frac{\sin qr\pi}{n+1}\,. \end{aligned} \tag{296}$$

29 LAGRANGE then examines the particular solutions $y_k = \varphi_{rk}$, [*i. e.* the simple modes]. In order that $\varphi_{rk} = 0$, we must satisfy one of the two conditions

$$\sin\frac{kr\pi}{n+1} = 0 \quad\text{or}\quad A_r \cos\left(2Ct\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right) + B_r \frac{\sin\left(2Ct\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}\right)}{2C\sin\frac{r\cdot\frac{1}{2}\pi}{n+1}} = 0\,. \tag{297}$$

1) This is easily proved directly; also, it follows by induction from (246) and from the similar formula $\sum_{k=1}^{n} \sin\frac{k\pi}{n+1}\cos\frac{rk\pi}{n+1} = 0,\ r > 0$.

The first of these yields $r = p\,(n+1)/k,\ p = 0, 1, 2, 3, \ldots$ Study of the cases $r = 1, 2, 3$ leads Lagrange to conclude that "the polygons have exactly r loops"; [this is true, but the examples he adduces are confined to the case when each node coincides with a mass.]
From $(297)_2$ follows [Euler's] formula (243) for the frequency of the r^{th} mode [of course 30—31
$\sqrt{\frac{K}{M}} = C$ in the present notation]. These results give a purely analytic justification for 32
Daniel Bernoulli's theory, as far as the loaded string is concerned. However, the vibra- 33
tions become "simple and regular" only if but a single mode is excited, and Lagrange determines the corresponding initial conditions. "This problem has already been solved by some geometers in the case of a certain number of bodies, but the route they have followed has always led them to as many equations as there are bodies ..., and the roots of these they have had to seek in each particular case. I do not think anyone has given a general formula such as we have just found."

[This claim is technically true as far as Daniel Bernoulli's prior work is concerned, but in effect it grossly underestimates[1]) the value of the earlier solutions for discrete systems in showing what is to be expected. In regard to Euler's results, at first sight it seems to be only a falsehood, since Euler had published long before the general solution (244). However, there are two differences between Lagrange's analysis and Euler's. First, Euler was content to infer completeness by counting the constants of integration; since a uniqueness theorem was not yet available, this argument was inadequate, but Lagrange's much more involved manipulations carry with them a proof that the solution is indeed general. Second, and more important, Lagrange *determines the constants of integration explicitly in terms of the initial velocities and displacements*, while Euler's search for a model for a sound pulse had led him to consider only special initial conditions. A by-product of Lagrange's analysis, not noticed even by himself until later, is the proof that *a trigonometric polynomial of* n *terms may be made to pass through* n *arbitrary points*. This result would go far to support Daniel Bernoulli's claim for the accuracy of his method (above, p. 256), but Lagrange, as we shall see now, is to misinterpret his own formulae as proving the opposite.]

There follows Lagrange's celebrated passage to the limit in an attempt to derive 34
from (295) the solution (257) for the continuous string. First, in the oscillating functions [but not in the amplitude functions] Lagrange replaces $\sin \frac{r \cdot \frac{1}{2}\pi}{n+1}$ by $\frac{r \cdot \frac{1}{2}\pi}{n+1}$, no matter

1) Earlier, in a footnote referring to John II Bernoulli's attempt to prove that small vibrations are always "simple and regular" because the restoring force is approximately linear (above, p. 171), Lagrange says that if more than one body is present "it is easy to understand" that "the motions ... are no longer restricted to simply isochronism," and in support he cites only d'Alembert's work of 1750 on the loaded string (above, p. 241). It is difficult to see here anything more than quibbling over terms, combined with deliberate oversight of Daniel Bernoulli's long prior work.

35 how large is r. If M is the total mass of the system, M/n is the mass of each particle, and hence $C^2 = \frac{T}{Ml} n(n+1)$, where $l = (n+1)a$, the total length. Setting $c^2 \equiv T/\sigma$,
36—37 $\sigma \equiv M/l$, we thus have $\frac{C}{n+1} = \frac{c}{l}\sqrt{\frac{n}{n+1}} \approx \frac{c}{l}$. Set $y_k(t) \equiv y(x,t)$, $\frac{k}{n+1} \equiv x$, $\frac{q}{n+1} \equiv X_q$, $a \equiv dX_q$. Then (295) becomes

$$\text{(298)}\quad y(x,t) = \frac{2}{l}\sum_{r=1}^{\infty} \sin\frac{r\pi x}{l} \sum_{q=1}^{\infty} \sin\frac{r\pi X_q}{l} dX_q \,.\left[Y_q \cos\frac{cr\pi t}{l} + \frac{l}{r\pi c} V_q \sin\frac{cr\pi t}{l}\right],$$

where n has been replaced by ∞. LAGRANGE regards $\sum\limits_{q=1}^{\infty} \ldots dX_q$ as an integral, replaces Y_q and V_q by $Y(X)$ and $V(X)$, then interchanges this integral with $\sum\limits_{r=1}^{\infty}$, obtaining[1])

$$\text{(299)}\quad \begin{aligned} y = {} & \frac{2}{l}\int_0^l Y(X)dX \sum_{r=1}^{\infty} \sin\frac{r\pi X}{l} \sin\frac{r\pi x}{l} \cos\frac{r\pi ct}{l} \\ & + \frac{2}{\pi c}\int_0^l V(X)dX \sum_{r=1}^{\infty} \frac{1}{r} \sin\frac{r\pi X}{l} \sin\frac{r\pi x}{l} \sin\frac{r\pi ct}{l}\,. \end{aligned}$$

[As has been remarked by BURKHARDT and others, this precipitous interchange not only introduces divergent series in a problem where they need not occur but also prevents
38—40 LAGRANGE from concluding what is now called "FOURIER's theorem".] There follow long and arduous transformations[2]) of these [divergent] series, involving such [dubious] steps as regarding $m(x \pm t)$ as an integer if $m = \infty$. The result is EULER's solution (257), in-
40 cluding the continuation. "There, then, is the theory of this great geometer [EULER] put beyond all doubt and established upon direct and clear principles which rest in no way on the law of continuity which Mr. D'ALEMBERT requires; there, moreover, is how it can happen that the same formula that has served to support and prove the theory of Mr. BERNOULLI on the mixture of isochronous vibrations when the number of bodies is . . . finite shows us its insufficiency . . . when the number of these bodies becomes infinite. In fact, the change that this formula undergoes in passing from one case to the other is such that the simple motions which made up the absolute motions of the whole system destroy each other for the most part, and those which remain are so disfigured and altered as to become absolutely unrecognizable. It is truly annoying that so ingenious a theory . . . is shown false in the principal case, to which all the small reciprocal motions occurring in nature may be related." [For this astonishing and false conclusion no further reason is given[3]).]

Since LAGRANGE has not used the formal rules of differential calculus, he considers that the initial values Y and V need be subject to no law of "continuity". [In this, of

1) The formula as printed by LAGRANGE has dx for dX; we follow BURKHARDT's restoration of what LAGRANGE must have meant, and also we correct a slip of BURKHARDT.

2) They are described by BURKHARDT.

3) In his letter of 4 August 1758 to EULER (Œuvres de LAGRANGE 14, 157—159), LAGRANGE writes that in passage from the finite to the infinite "the whole BERNOULLIAN theory collapses."

course, he has deceived himself. The several dubious limit processes he has carried out could be justified, if at all, only by imposing appropriate restrictions on Y and V. Despite its failure, the attempt to carry through the limit from the discrete solution to the continuous one is a remarkable achievement.

The remaining work on the vibrating string in this paper is borrowed without acknowl- 40—51
edgment from others, mainly from EULER.] An interesting detail is the description of the results of SAUVEUR (above, p. 122), hitherto not mentioned in any theoretical paper. [Indeed, it is curious that LAGRANGE's memoir, although weak in physical principle and unconvincing in drawing a connection between mathematics and experience, should be the first theoretical study to employ the terms used in experimental acoustics; the preceding researches by DANIEL BERNOULLI and EULER, while giving correct theories for important acoustical phenomena, eschewed as if by intention the vocabulary of the subject.] LA- 64
GRANGE, [like MERSENNE,] explains beats in terms of reinforcement and cancellation of vibrations; also he is the first to attempt a theoretical explanation of TARTINI's combination tones.

Meanwhile, EULER had undertaken to reconsider the problem of the loaded string, but his paper, *On the vibratory motion of a flexible thread loaded by any number of little bodies*[1]), [achieves little]. EULER's purpose in treating "this problem, now solved by others," is to Summary
justify his "discontinuous" solution by passage to the limit from the solution for the weighted string, [but this he fails entirely to do, and it is difficult to understand why he published the last part]. EULER remarks that in the discrete case "the sounds are very irrational and therefore highly dissonant with one another."

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1) E 286, "*De motu vibratorio fili flexilis corpusculis quotcunque onusti*," Novi comm. acad. sci. Petrop. 9 (1762/3), 215—244 (1764) = Opera omnia II 10, 264—292. Presentation dates: 15 November 1759 and 1 December 1760. As shown by his letters of 2 and 23 October 1759 to LAGRANGE, between those dates EULER saw LAGRANGE's paper on the loaded string. While LAGRANGE borrowed heavily and without acknowledgment from EULER, now we see EULER refusing to profit in the least from LAGRANGE's work but nevertheless publishing later an inferior analysis of his own. There are two possible explanations.

1. In letters of 27 July 1762 and 21 September 1762 to GERHARD FRIEDRICH MÜLLER, EULER writes that there are great gaps in the proof sheets of his pieces in vol. 8 of the Novi Commentarii, and he proposes that one be carried over to vol. 9. This might explain the unsatisfactory state of the end of E 286. EULER's correspondence with MÜLLER has been published in "*Die Berliner und die Petersburger Akademie der Wissenschaften im Briefwechsel* LEONHARD EULER*'s*," Teil I, ed. A. P. JUŠKEVIČ & E. WINTER, Berlin, 1959.

2. Preliminaries for E 286 are to be found on p. 172 of Notebook EH 6 (1750—1757) and p. 71 of Notebook EH 8 (1759—1760); the latter passage concerns the limit process. Thus EULER may well have had most of the analysis complete before seeing LAGRANGE's paper, upon the arrival of which he may have decided to publish what he had without considering the matter further, as is suggested by the phrase quoted above from the Summarium.

1—6 The differential equations, [which generalize (235) and may be obtained by linearization from (209)], are

$$M_k \ddot{y}_k = T\left[\frac{y_{k+1}-y_k}{a_{k+1}} - \frac{y_k - y_{k-1}}{a_k}\right], \quad k = 1, 2, \ldots, n, \tag{300}$$

7—10 with $y_0 \equiv y_{n+1} \equiv 0$. The method and the ideas are those used in E 136, the only difference being that the possibly unequal spacing and unequal masses introduce complications.

11—14 EULER works out the details for the case when $n = 1$. The lowest frequency is
15—25 obtained by putting the weight in the middle. For $n = 2$, the roots are proved real. The
26—33 string can never occupy the line $y = 0$ unless the frequencies are commensurable. Initial
34—35 conditions for such a "regular" motion are determined. When the weights are equally
36 spaced, the frequency equation is solved explicitly for $n = 3$ and $n = 4$. The form of the frequency equation is then conjectured for general n. Put

$$z \equiv \omega^2/T, \qquad P_k \equiv M_k\left(\frac{1}{a_{k-1}} + \frac{1}{a_k}\right), \tag{301}$$

where ω is the circular frequency. Then if $n \geqq 2$, the equation is

$$0 = 1 - \frac{[M_1 M_2 a_2^2]^{-1}}{(P_1 - z)(P_2 - z)} - \frac{[M_2 M_3 a_3^2]^{-1}}{(P_2 - Z)(P_3 - Z)} - \cdots \tag{302}$$
$$+ \frac{[M_1 M_2 M_3 M_4 a_2^2 a_4^2]^{-1}}{(P_1 - z)(P_2 - z)(P_3 - z)(P_4 - z)} + \frac{[M_2 M_3 M_4 M_5 a_3^2 a_5^2]^{-1}}{(P_2 - z)(P_3 - z)(P_4 - z)(P_5 - z)} + \cdots - \cdots$$

37—44 When the masses and spacings are equal, the results reduce to (243).

49—50 EULER's attempt at passing to the limit of the continuous string [is entirely faulty[1]), except that he infers a result which LAGRANGE might have derived but did not[2])], *viz*, as $n \to \infty$ we have

$$\nu_r^{(n)} \to \frac{r}{2\pi}\sqrt{\frac{T}{Ml}} = \frac{r}{2\pi l}\sqrt{\frac{T}{\sigma}} = \nu_r: \tag{303}$$

For a string loaded by n equally spaced and equal masses, if the length and total mass are held

1) EULER replaces $\sin\frac{\alpha}{n+1}$ by $\frac{\alpha}{n+1}$ in *all* its occurrences, and he neglects to consider the dependence of the coefficients upon the initial displacements. The result is

$$y_k = k \sum_{j=1}^{\infty} b j \cos\left(j\sqrt{\frac{T}{Ml}}\,\pi t\right).$$

It is extraordinary that (1) EULER let stand the ridiculous conclusion $y_k = k y_1$ which follows at once, (2) EULER failed to observe that the result, if correct, would support DANIEL BERNOULLI's viewpoint rather than his own, and (3) EULER failed to profit from LAGRANGE's determination of the coefficients as functions of the initial conditions, although his own earlier work in E 136 had obtained a major special case.

2) Recall that LAGRANGE was intent on his illusion that a harmonic decomposition is *not* generally valid.

fixed as $n \to \infty$, *the proper frequencies approach the* TAYLOR-BERNOULLI *values for the continuous string*. [As remarked in the footnote on p. 232, this follows at once from EULER's old result (243), interpreted in the present context.]

39. Miscellaneous polemics (1760—1767). [All through the eighteenth century the controversy over the vibrating string continued; D'ALEMBERT, BERNOULLI, and EULER each held fast to his original view with little if any modification, while LAGRANGE, ever maintaining a particular opposition to BERNOULLI, drifted slowly toward the opinions of D'ALEMBERT. Nothing decisive in regard to the controversy was added by any of these savants; by the various others who ventured into the field, nothing of any importance whatever. The only gain was EULER's clear and compelling explanation of the progression and reflection of waves, as we shall learn in § 40 from his own words. For completeness, we first summarize the intervening tedious polemic.]

LAGRANGE quickly issued an enormous second memoir, *New researches on the nature and the propagation of sound*[1]). After acknowledging letters from DANIEL BERNOULLI and 5
D'ALEMBERT criticizing his passage to the limit of the continuous string, he gives a new method[2]) for solving the wave equation (251). I remark only a discussion of certain for- 60—64
mulae that arise naturally by trying (236) as a solution for (235), [as EULER had done long before. I do not see how LAGRANGE arrives at these equations, nor do I perceive any definite conclusion resulting from his pages of manipulation.] At the end, however, we read that "this method seems to demonstrate the beautiful proposition of Mr. DANIEL BERNOULLI" on the decomposition of small vibrating motions into harmonic oscillations, "whether the number of bodies . . . be finite or infinite." [That, on equally tenuous grounds, he now reverses the conclusion drawn in his first memoir, LAGRANGE does not find it necessary to remark.]

D'ALEMBERT expressed his violent resentment of EULER's work, as we read in EULER's letter of February 1757 to MAUPERTUIS[3]): "Mr. D'ALEMBERT causes us much annoyance with his disputes, after Mr. FORMEY sent him my answer to his memoir. He points out that he is more than ever convinced of his opinion; that he will show also that he is right in his old disputes with Mr. BERNOULLI on hydrodynamics; though everyone ought to agree that experiments have decided for Mr. BERNOULLI. If Mr. D'ALEMBERT had the candor of

1) "*Nouvelles recherches sur la nature et la propagation du son*," Misc. Taur. 2_2 (1760/1761), 11—172 (1762) = Œuvres 1, 151—316.

2) Since this method contributes nothing toward understanding the mechanical problem, I do not present it here. It is described by BURKHARDT, § 11 of *op. cit. ante*, p. 11.

Much of the remainder of this paper by LAGRANGE is summarized in my introductions to L. EULERI Opera omnia II 12, p. CXXII, and II 13, Part II P. Some other parts are described below.

3) To the same effect, in yet more outspoken terms, EULER wrote to LAGRANGE on 2 October 1759, referring especially to D'ALEMBERT's renown among the "semi-learned".

Mr. Clairaut, he would not hesitate to retreat. But if as things stand the Academy wished to lend its memoirs to his views, the Mathematical Class would be filled for some years only with disputes on vibrating strings leading to absolutely nothing, and therefore in the last assembly . . . it was found good to suppress the memoir of Mr. d'Alembert on this subject. He demanded also that I put in new confessions of a number of things I had robbed from him. But my patience is at an end, and I have let it be known to him that I will do nothing, that he may himself publish his claims wherever he will, and I will do nothing to prevent it. He will have enough to fill up the article on *Claims* in the Encyclopaedia[1])." On 3 September 1757 : "Mr. d'Alembert is not bothering me any more, and I have taken the firm resolution not to cross swords with him again, no matter what he publishes against me."

[When d'Alembert had embroiled himself with nearly all other geometers at home and abroad, so that he could use no ordinary avenue of publication for his quarrelsome if not abusive writings,] he began to issue his *Opuscules,* [collections of papers having little or no solid content, not of a quality or style fit for a learned journal, but nevertheless sufficient, with the renown of d'Alembert's name among the "semi-learned", to be sold successfully by a commercial publisher.] The first paper is called *Researches on the vibra-*
I *tions of sounding strings*[2]). After objecting [with justice] that $c^2\dfrac{\partial^2 y}{\partial x^2}=\dfrac{\partial^2 y}{\partial t^2}$ does not neces-
sarily imply $\pm c\dfrac{\partial y}{\partial x}=\dfrac{\partial y}{\partial t}$, as Euler had claimed[3]), d'Alembert enters a lengthy plea
V—XI that the curvature must be continuous, even at the end points. [D'Alembert's writing and
calculations are obscure,] but it seems that among his objections is the fact that (251)
IX cannot be satisfied unless $\partial^2 y/\partial x^2$ exists, [a matter disregarded by Euler]. At the end
XI points, since $\partial^2 y/\partial t^2 = 0$, the curvature must be zero. The "true metaphysical reason"
for the requirement of continuous curvature is that the accelerating force is not defined at
points "where the radius of curvature has two values." In such cases "the motion of the
string cannot be submitted to any calculation, nor represented by any construction . . ."
XXII [For a discussion of the falsity of this claim, see below, pp. 285—286.] In reply to Euler's
challenge that corresponding to an *arbitrary* initial shape there must be *some* motion, d'Alem-
bert says " . . . the problem cannot be solved ; it will surpass the force of known ana-
XXIII lysis." As for explanation of why the sound emitted is always more or less the same, no
matter how the string is struck, " . . . I am persuaded that this question is not at all
proper for analysis, which has done all that could be expected of it : It is up to physics to
take care of the rest." D'Alembert then challenges Euler to treat the case when the

1) *Cf.* above, footnote 3, p. 245.

2) *Recherches sur les vibrations des cordes sonores,*" Opusc. Math. 1, 1—73 (1761).

3) In § 17 of E 119, cited above, p. 245.

initial shape is a triangle. That he considers this case impossible does not prevent him from reproaching DANIEL BERNOULLI with being unable to solve it, since a trigonometrical series "obviously belongs to a curve whose curvature is continuous . . ." [Thus he himself commits the error of which he had just accused BERNOULLI in connection with the simple modes, *viz*, "drawing conclusions from the finite to the infinite too lightly[1])." XXIV

Finally D'ALEMBERT scrutinizes LAGRANGE's work on the weighted string. In addition to other, less sound, criticisms, D'ALEMBERT here detects the errors that in fact invalidate LAGRANGE's passage to the limit (above, pp. 269—270). Suppl.

LAGRANGE was quick to publish an ineffectual reply[2]). He admits that it is unjust to set $\sin\frac{p\pi}{2n} \approx \frac{p\pi}{2n}$ when p is large, but by calculating and solving the asymptotic form of the equation for R he obtains the same results without using this incorrect step. [On the other points, however, LAGRANGE's answers consist in reaffirmations or evasions[3]).]

About this time D'ALEMBERT visited Potsdam, where, in the midst of intrigues, he lived as the intimate of FREDERICK II and the *de facto* director of the Berlin academy. On 20 and 29 July 1763 D'ALEMBERT wrote to EULER, disposing of his work on strings with Olympian disdain. [These and other letters of this period from D'ALEMBERT reveal him as a scheming politician, while EULER remained steadfast in defence of what he considered to be the truth[4]), regardless of the personal disaster resulting from any opposition to D'ALEM-

1) Calling attention to his earlier remarks on the loaded string (above, p. 241), D'ALEMBERT claims to give a solution better than BERNOULLI's for the case of two weights (§ XXVII). He objects to BERNOULLI's theory of the multiplicity of harmonic sounds because in the case of a discrete system the proper frequencies generally do not harmonize with one another. He doubts the *physical* correctness of BERNOULLI's theory, partly on the basis of different and awkward definitions of his own, which he blames others for not using, of such quantities as the period of a vibration (§ XXVIII).

2) "*Addition à la première partie des recherches sur la nature et la propagation du son, imprimées dans le volume précédent,*" Misc. Taur. 2_2 (1760/1761), 323—336 (1762) = Œuvres 1, 319—332.

3) In some cases there are outright errors, as when (§ III) LAGRANGE says in effect that $\frac{\partial^2 y}{\partial x^2}$ exists and gives the accelerating force even if the curvature is discontinuous. In other cases we find a deplorable kind of logic, as when LAGRANGE observes (§ II) that his solution and D'ALEMBERT's agree when the initial shape is such that the latter is valid, whence, "since his objections do not prevent my solution from being exact when that figure satisfies certain conditions, they do not any the more prevent it from being exact in general"; in other words, LAGRANGE fails to recognize the possibility that falsehood $\rightarrow$ truth. Such feeble reasoning is not to be found in the works of the mathematicians of the older generation.

4) EULER was aware of what was happening. On 7 June 1763 he wrote to GERHARD FRIEDRICH MÜLLER, "That Mr. D'ALEMBERT has refused a highly considerable and profitable position in Russia I should think to ascribe not to philosophy but rather to fear that in the end the matter would turn out badly, since despite his unbearable haughtiness he was easily able to understand that he was not at all suited to that position. In any case, to use Mr. BERNOULLI's expression, his philosophy consists in an impertinent sufficiency, so that he tries to defend all his mistakes in the most shameless way, which but too often hoists him with his own petard, so that these many years from vexation he will have

BERT.] Later D'ALEMBERT published[1]) an extract from a letter of 26 July 1763, in which EULER defends polygonal initial figures but remarks that not only the displacement *but also the slope* of any admissible initial shape must be infinitely small. In a letter[2]) of 20 December 1763 to D'ALEMBERT, EULER explains this idea more fully: "... I find in fact some absolutely necessary limitations without which my solution cannot hold." These are, 1° that the length of the deformed curve differs only infinitely little from that of the original straight form, and 2° that the motion be purely transversal. "To satisfy these two conditions it is not sufficient that all the ordinates be ... infinitely small, but beyond that the tangents ... must have infinitely small slopes." EULER goes on to say that any curve $y = f(x)$ with no vertical tangents will be a possible initial figure, since then for sufficiently small α the figure $y = \alpha f(x)$ has ordinates and slope as small as desired. He explains his solution again and adds that he agrees with D'ALEMBERT's old objections but regards them as applying only to certain cases "which must be excluded before the solution can be applied. Besides, it seems to me that considering such functions as are subject to no law of continuity opens to us a wholly new range of analysis ..."

While DANIEL BERNOULLI was unable to advance the theory at this time, he devised a beautiful experiment to demonstrate harmonic resonance[3]). "Stretch horizontally a long string, say of 24 feet, by such a weight ... that one natural vibration lasts, say, 1/4 second. Near one end of the string place a toothed wheel in a vertical plane, perpendicularly against the string, so as to rattle it in such a way that when the wheel turns, each tooth gives the string a light blow and slips by. If the wheel is turned uniformly, and ... if the passage from one tooth to the next lasts exactly 1/2 second, 1 second, 3/2 seconds, or 2 seconds, the vibrations of the string will become regular and will be well maintained; but without this

nothing to do with mathematics. In his hydrodynamics he most cavalierly contradicted most of the theorems of Mr. BERNOULLI, despite their being confirmed by abundant experience, for his own theorems contradict experience, and he was not able to overcome his haughtiness to the extent of recognizing his patent errors.

"With those who understand these matters, his quarrelings with the thorough Mr. CLAIRAUT can reflect nothing but the greatest shame upon him. Only here [in Prussia] is he called a creative intellect, a man who encompasses all; but from the same reason there is no doubt that he will not come here either ... But after the most urgent persistence he has decided to undertake a journey to Potsdam ... so as to decide the entire fate of our academy ..."

Judging by a letter from SEGNER to EULER of 19 March [1763], EULER had written him to the same effect.

1) In *Opuscules math.* 4, 162 (1768), included in E 365, EULERI Opera omnia II 11, 1—2.

2) The unpublished letter in the Gotha University Library bears no date, but its contents correspond to the paraphrase given as of 20 December 1763 by D'ALEMBERT, *Opuscules math.* 4, 146 (1768) and included in E 365, EULERI Opera omnia II 11, 1—2. Part of the contents is given also in a letter of 24 May 1764 from EULER to JOHN III BERNOULLI; the original is in the Basel University Library.

3) § 16 of *op. cit. ante*, p. 263. *Cf.* the experiments of HOOKE, mentioned above, p. 58.

harmony between the natural vibration of the string and the repetition of the blow of the tooth, the string will not form regular vibrations.

"Accelerate the motion of the wheel; the string will then have irregular motions in all its parts. But when one has succeeded in making the passage from one tooth to another last just 1/4 second, the string will immediately divide in two, forming a node in the middle of two loops; the node will be sensibly at rest, and the vibration of each half will last only 1/8 second. Accelerating the wheel again, when one succeeds in making it give just 6 blows of the teeth in one second, then the string will divide into three equal parts . . . [*etc.*]. All these phenomena can be seen by the eye but will make no effect on the organ of hearing . . ." Still greater speeds of rotation of the wheel produce audible sounds, but only at discrete frequencies. Bernoulli conjectures that the grains of rosin on the bow of a violin excite the string much as do the teeth of the wheel[1]).

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1) The vibrating string is mentioned in the following unpublished letters between Daniel Bernoulli, John III Bernoulli, and Euler:

7 December 1763 (D to JIII).

Undated reply to the above (JIII to D): Euler is at work on the general solution of the initial value problem and has just written to d'Alembert about it (*i. e.*, in the letter of 20 December 1763, quoted above, p. 276). Euler "claims that by your theory you will not be able to solve this problem, or at least the difficulty would approach impossibility because it would be necessary to continue to infinity this sequence of sines."

Undated, 1764 (D to JIII): "I do not think . . . that the string can ever make and continue vibrations in the form of intersecting straight lines . . . I admit ~~I do~~ not like the terms being introduced in consequence of the new theorem of Mr. d'Alembert, namely, the expression of the number of vibrations in a given time without distinguishing the circumstances. If I set a uniform string into a vibration of third order, without any mixture with any other kind of vibrations, it is certain that the string will then make three times as many vibrations as it would in making purely the vibrations of first order; the ear will hear only the twelfth . . . This being so, I do not understand in what sense one can say that the number of vibrations is always the same and consequently one hears only the same tones."

24 May 1764 (E to JIII): Euler has always regarded Bernoulli's "idea of composing the motion of strings from simple and regular oscillations as the happiest discovery for illuminating this spiny matter; and if it is a question of determining all possible motions that the string may undergo, there is no doubt that this method furnishes all the enlightenment that one could wish. But it is also permissible to look at the subject from another point of view . . ." Euler then sets the initial value problem. "From this point of view, we do not directly require the oscillatory motion of the string, or the sound it gives out, but we must determine the shape that the string has at each instant.

"I do not wish to deny absolutely that the equation composed of an infinity of sines includes the solution of this question, since it contains arbitrary constants which it would be possible to determine in such a way that in putting the time $= 0$, [this form] would produce exactly the curve impressed upon the string at the beginning. But your uncle will not disagree that this operation would be infinitely troublesome and even impossible to execute, because of the infinity of coefficients one would have to determine." Euler thinks most commendable a method "enabling us to dispense with the said boring operation of looking for the values of this infinity of coefficients . . ."

In the course of a miscellany[1]) LAGRANGE returns to the problem of the vibrating string. He decides to settle the controversy by solving the functional equations (260); that is, to find the most general odd periodic equation of period $2l$. To this end, he applies a formal series solution derived earlier for a more general problem. The result is a trigonometric series! [LAGRANGE's terms are so vague that I cannot be certain what problem it is he thinks he has solved. Since he speaks explicitly of D'ALEMBERT's solution, rather than his own or EULER's, and of the "equation", it seems that if his formalism were correct it would establish DANIEL BERNOULLI's solution as equivalent to D'ALEMBERT's[2]).] This is borne out by his concluding that "the equation of the initial shape of the string, when it has one," is of the form derived. At this time LAGRANGE seems to believe that (270), being an "equation", is *not* the general solution of the problem of the vibrating string, and thus he pays no further attention to the result he has just concluded.

38 Again considering the loaded string, LAGRANGE now solves (235) by superposition of harmonic solutions (236), [just as EULER had done, many years before, even to using the
39 device (239)]. New, however, is a rearrangement of the explicit solution (295) in the case when $V_k = 0$. While LAGRANGE claims to put the $y_k(t)$ into a form expressed in terms of two functions $\varphi(\zeta)$ and $\psi(\zeta)$ evaluated at the arguments $\dfrac{m \pm ct}{n}$ and $\dfrac{m \pm 1 \pm ct}{n}$,
40—41 [the result is in fact false[3]), since the functions vary with t]. There follows a sketch of a

On 25 July 1765 DANIEL BERNOULLI in a letter to JOHN III BERNOULLI writes "It seems to me more and more that my method is general, though only *potentially*, for I agree that the determination of my coefficients would most often be beyond analysis or rather, beyond its reach."

1) "*Solution de différens problèmes du calcul intégral,*" Misc. Taur. 3_2 (1762/1765), 179—380 (1766) = Œuvres 1, 471—668.

2) A modern reader might be misled into believing this an attempt at proving "FOURIER's theorem", since LAGRANGE uses a general "function" φ, but, as explained in the text above, only "equations" are considered. See also LAGRANGE's letter to D'ALEMBERT of 26 January 1765. The correspondence between D'ALEMBERT and LAGRANGE fills Vol. 13 of the Œuvres of the latter (1882).

It is curious that LAGRANGE's formal result is included as a special case in a remarkable analysis published by EULER a decade earlier. In § 55 of E 189, "*De serierum determinatione seu nova methodus inveniendi terminos generales serierum,*" Novi comm. acad. sci. Petrop. **3**, (1750/1), 36—85 (1753) = Opera omnia I **14**, 463—515, EULER had given formal transformations indicating that the general solution of the equation $f(x) = f(x-1) + X(x)$ is

$$f(x) = \int^{x} X(\xi)\,d\xi + 2\sum_{n=1}^{\infty}\left(\cos 2n\pi x \int^{x} X(\xi)\cos 2n\pi\xi\,d\xi + \sin 2n\pi x \int^{x} X(\xi)\sin 2n\pi\xi\,d\xi\right).$$

EULER's work presumes, among other things, that f is infinitely many times differentiable; thus, like LAGRANGE's analysis described above, it is not directly relevant to the controversy over the vibrating string.

3) LAGRANGE defines φ and ψ as finite trigonometric sums in terms of certain coefficients P_ν and Q_ν; the formulae he gives to define P_ν and Q_ν show that these depend not only on the initial data Y_μ but also on ct. The formal rearrangement is correct, but the result is not of the functional form that LAGRANGE claims it to be and is in fact valueless.

limit process to the continuous case, based upon this form of the finite solution. This time LAGRANGE's assertion is more cautious; he is content to state that with these formulae one can pass a polygon through as many points on the given figure as desired. In the limit there emerges a trigonometrical series for the initial shape. "It is certain that if the generating curve is to be *geometrically* [*i. e.* precisely] the same as the initial curve," this latter must be representable by a trigonometric series. "Whatever is the initial curve," one can always pass a trigonometric series through infinitely many points which are infinitely near to this curve. [The meaning of this statement is not clear; what LAGRANGE's earlier work shows is that a trigonometric polynomial of n terms can be made to pass through any n points with equally spaced abscissae.]

In the long correspondence between D'ALEMBERT and LAGRANGE, the problem of the vibrating string is mentioned again and again. By degrees[1]), LAGRANGE is won over to D'ALEMBERT's viewpoint. On 13 November 1764 LAGRANGE writes, "I am not a little pleased to have come nearer to you on this point," but he still considers his construction valid even when the initial shape cannot be expressed by an "equation". D'ALEMBERT is triumphant, replying on 12 January 1765 that if the initial curve is "traced at will, how can we be sure that $d^n y/dx^n$ has no jump at any point?" (D'ALEMBERT is now claiming that in order to be admitted as a solution, a function must have continuous derivatives of all orders.) He asserts that for all its derivatives to be continuous, a function must have a power series expansion. [The correspondence leaves a poor impression of LAGRANGE's capacities. He seems to rely on algebraic formalism alone and to be unable or unwilling to face the real issues, either in analysis or mechanics.] LAGRANGE's capitulation is formalized by his publishing in his journal in Turin the *Extract from various letters of Mr.* D'ALEMBERT *to Mr.* DE LA GRANGE[2]), written expressly for publication by D'ALEMBERT, who claimed thereby "to have the occasion of rendering you, without any flattery, the justice which you deserve[3])," and where in addition to some of D'ALEMBERT's formal flattery we read[4]) "I am delighted that at last we are almost entirely in agreement . . ." D'ALEMBERT now considers $y = \alpha(\sin \pi x)^{p/q}$ to be a possible initial shape, provided $p > q$, "so that dy is

1) Letters of 27 September 1759 (D), 27 November 1761 (D), 1 June 1762 (L), 15 November 1762 (D), 30 May 1764 (L), 1 September 1764 (L), 16 October 1764 (D), 13 November 1764 (L), 12 January 1765 (D), 26 January 1765 (L), 2 March 1765 (D), 20 March 1765 (L).

2) "*Extrait de différentes lettres de M.* D'ALEMBERT *à M.* DE LA GRANGE *écrites pendant les années 1764 & 1765*," Misc. Taurin. $\mathbf{3}_2$ (1762/1765), 381—396 (1766). See § V.

3) Letter of 2 March 1765. D'ALEMBERT sent the piece to LAGRANGE on 28 December 1765.

4) In his letter of 16 March 1764 to EULER, D'ALEMBERT exults that LAGRANGE "seems very disturbed" by his objections and now does not believe the solution holds for polygonal initial figures. "He begins to doubt also . . . in the case when the curvature has jumps . . .," *etc.* There is also an incomprehensible letter from D'ALEMBERT to EULER on 25 June 1764, where D'ALEMBERT seems to extend his objections also to the loaded string.

nowhere ∞, which is contrary to the hypothesis on which the solution rests. I know that in these curves there are some of the $d^n y/dx^n$ that are infinite, but this does not invalidate the solution . . .; it is enough that $d^n y/dx^n$ makes no *jump*, that is, does not pass *brusquely* from the finite to the infinite . . . or from one finite value to another . . ." [It would be too much to expect precise analytic definitions from any geometer at this time, but this example shows that D'ALEMBERT's intuitive misconceptions are extensive :] He has just claimed categorically that all solutions must have power-series expansions, [while $(\sin \pi x)^{p/q}$ does not have such an expansion unless $p/q =$ a non-negative integer,] and that $(\sin \pi x)^{p/q}$ does not have a trigonometrical series expansion, [which is false. Moreover, D'ALEMBERT's claim that *all* derivatives must be continuous or infinite is a mere pronouncement, for which he never advances any substantial reason.]

It was at this time that D'ALEMBERT was prevailing upon FREDERICK II to replace EULER by LAGRANGE in the Berlin Academy.

The dense calculations in JORDAN RICCATI's paper, *On the vibrations of sounding strings*[1]), are of no value for theory[2]), but inserted among them are two interesting con-
XVIII jectures on the mechanism of hearing. First, the labyrinth of the ear contains a long auditory nerve, which may be represented as a semi-infinite string and hence susceptible of
XIX vibration at any frequency. It resonates in unison with the sound received. Alternatively, "one would suspect that the auditory nerve is composed of a bundle of nerves which by the smallest degrees pass from the lowest tone to the highest, and the one of these that corresponds to unison with the sounding body is set a trembling." This second idea, [partially anticipated by RAMEAU (above, p. 125) and partially anticipating HELMHOLTZ's theory of the ear,] RICCATI gives some reason for rejecting in favor of the first.

RICCATI follows this by a study[3]) of the dimensions to be given to strings so as to
II make equable sounds. He says that the ratio T/A [*i. e.*, the stress] should be constant and

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1) Sched. IV. "*Delle vibrazioni delle corde sonore,*" pp. 65—104 of *Delle corde ovvero fibre elastiche schediasmi fisico-matematici*, Bologna, Stamparia [Sa]n Tommaso d'Aquino, 1767.

2) In disregard of the general theory, RICCATI follows TAYLOR, except that he considers all the simple modes. In § X he repeats TAYLOR's claim that a string initially given a triangular figure will quickly assume sinusoidal shape; he adduces some incomprehensible arguments, starting from the hypothesis that the string gives out "only one sound". § XXV gives an explanation of TARTINI's combination tones; in §§ XXVIII—XXXII is a critique of HERMANN's attempt (above p. 132). In § XLVI RICCATI after mentioning the recent work of D'ALEMBERT, EULER, and LAGRANGE puts himself on the side of BERNOULLI; his attempt to justify this stand merely reveals his own failure to grasp the general principles of mechanics. In the "*Appendice allo schediasma IV,*" pp. 221—246, he draws some figures representing motions compounded of two simple modes (§§ I—VI); then follow pages of calculation supposedly pertaining to combination tones.

3) Sched. VI. "*Delle misure, che debbono assegnarsi alle corde d'uno stromento, ed alle canne d'organo, acciocchè rendano suoni del pari forti, e aggradevoli,*" pp. 122—146 of *op. cit. ante*, footnote 1.

very near the breaking strength of the string, [as had EULER, above, p. 155]. Independently VI, XIV
of this assumption, he takes the kinetic energy of vibration as the measure of loudness; [while his statements are obscure,] he seems to make some attempt to calculate the maximum kinetic energy of the fundamental mode, [and the same results would follow from use of the mean kinetic energy in a period. With $\sigma = \varrho A$, either of these energies is a numeri- III—IX
cal multiple of $c^2 \varrho A \mathfrak{A}^2/l$, where $\mathfrak{A}$ is the amplitude.] With two strings for which T/A and ϱ have the same values, so also does $c^2 = T/(\varrho A)$, and hence for equal kinetic energy we must have

(304) $$A \propto \frac{l}{\mathfrak{A}^2};$$

this is RICCATI's result. He considers it evident that $\mathfrak{A}$ and A for a string of higher pitch X—XI
should not exceed their counterparts for one of lower pitch. The two extremes are given by $A =$ const. and $\mathfrak{A} =$ const., *viz*

(305) $$\mathfrak{A} \propto \sqrt{l} \quad \text{and} \quad A \propto l\,,$$

the latter being EULER's criterion (above, p. 154).

In instruments such as harpsichords, where each string is to give out but one tone, we XII
have full freedom of choice between these extremes, and in practice a mean is used. For the XIII
violin, since more than one tone is to be produced from the same string, we have $A =$ const., and only $(305)_1$ is applicable. To consider the different strings, assume that the bow has the XV—XVI
same action on each and that both T/A and ϱ are the same for each. Then the condition of equal kinetic energy yields

(306) $$\sigma \propto \frac{1}{\nu}.$$

Since the frequency ratio of successive strings of a violin is 3 : 2, according to (306) the higher strings should weigh 2/3 as much as the lower. By weighing the three gut strings of a violin, RICCATI obtains the ratios 6 : 10 : 15, which he considers adequate confirmation of his theory[1]).

40. EULER's researches on the propagation and reflection of waves (1764—1765). A new idea appears in EULER's letter of 24 May 1764 to JOHN III BERNOULLI, intended for DANIEL BERNOULLI. EULER doubts whether a series of sines suffices to represent a function which is zero over part of its interval of definition; "... at least, it seems permissible to doubt whether this would be possible ...," and he gives his own method for solving the problem when the initial shape is of this kind, corresponding to a string disturbed initially along *only a part of its length*. In his reply of 7 May 1765, DANIEL BERNOULLI doubts

1) The paper ends with a theory of the harpsichord; it is based in part on RICCATI's own stress-strain law and the resulting formula for the force required to deflect a string into triangular form (below, pp. 384—385), but I do not follow the details. As mentioned above, p. 116, a preliminary version of these papers was published in 1764.

EULER's solution because, when the disturbance reaches the middle, what reason is there for it to go in one direction rather than the other[1])?

1—2 EULER begins his *Clarification on the motion of vibrating strings*[2]) with the first published clear statement of the assumptions made implicitly by all who have tried to determine the motion of vibrating strings; these are the assumptions stated in his letter to
21 D'ALEMBERT of 20 December 1763 (above, p. 276). Admitting [BERNOULLI's contention] that the infinitely many constants in a trigonometrical series can be adjusted so that the curves representing the initial shape and initial velocity pass through infinitely many points, nonetheless he regards such a solution as "only very particular", for the same reason that power series, while also capable of being fitted to infinitely many points, cannot represent all possible "discontinuous", [*i. e.* non-analytic] curves. Then he puts the
22 challenge he had already written to DANIEL BERNOULLI: Suppose a string of length l be disturbed initially along *only a part* of its length, $0 \leqq x \leqq b$, say; then to get a solution in trigonometrical series we should have to determine the constants so that the series for $Y(x)$ and $V(x)$ reduce to zero for $x > b$, "which is manifestly impossible."

8 EULER now regards the arbitrary functions in the solution of a partial differential equation as analogues of the arbitrary constants in the solution of an ordinary differential
23—26 equation. Thus to verify the generality of a solution, it seems to him sufficient to count the number of arbitrary functions. However, he adds a proof of necessity: Since the initial shape and velocity may be prescribed arbitrarily, the general solution *must* contain two arbitrary functions[3]).

20—22 From the initial conditions (263) alone, independently of the end conditions, follows

$$(307)\qquad \Phi(x) = \tfrac{1}{2}Y(x) + \frac{1}{2c}\int V(x)dx, \qquad \Psi(-x) = \tfrac{1}{2}Y(x) - \frac{1}{2c}\int V(x)dx\,.$$

Thus $\Phi(x)$ and $\Psi(-x)$ are determined at once over any interval in which $Y(x)$ and $V(x)$ are known.

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1) DANIEL BERNOULLI adds in his characteristic way, "I am innerly persuaded that my principle . . . includes everything real on this subject . . ."

In this letter, and also in § 2 of *op. cit. infra*, p. 307, he says he cannot convince himself that the initial shape is "entirely arbitrary": Not only the slope but also the ratio of length to radius of curvature should be infinitely small. As DANIEL BERNOULLI remarked in a letter of 25 July 1765, this condition excludes polygonal figures.

2) E 317, "*Eclaircissemens sur le mouvement des cordes vibrantes*," Misc. Taurin. 3_2 (1762/1765), 1—26 (1766) = Opera omnia II 10, 377—396. Presentation date: 16 February 1765.

3) This EULER remarks also in his letter to LAMBERT of 4 December 1762. The same letter contains a prophetic remark: "If you wish to probe these new mysteries, you will easily reduce your researches on heat to similar equations among three or more variables, especially after having seen how I have reduced all of hydrodynamics to similar very simple formulae." The correspondence between EULER and LAMBERT has been published by K. BOPP, "*EULERS und JOHANN HEINRICH LAMBERTS Briefwechsel*," Abh. Preuss. Akad. Wiss. 1924, No. 2, 45 pp.

Figure 82. EULER's solution for the propagation of a pulse (1765)

The paper ends with what EULER expects to be the death blow to the 47
theories of D'ALEMBERT and BERNOULLI. "In the string AB (Figure 82), let only the part AD be disturbed initially, so as to give it the form AnD, and let it be quickly released . . ." The curve for $f(x)$, according to EULER's construction (above, p. 246), "is composed of the curve AnD and the straight line DB, the continuation of which will form beyond A the curve Ad, and in each direction
from A' the curves $A'D'$ and $A'd'$, equal to AnD, and so on. This case is not 48
properly one of a vibratory motion, but we ask rather how this initial agitation is successively spread out along all the string . . . Consider a point X . . ., which will remain at rest until the time $\frac{XD}{c}$, and then it will begin to be agitated during the time $\frac{2AD}{c}$, after which it will again be at rest until the time t multiplied by c reaches the curve $d'A'D'$, and so on; so that each part of the string will be put alternately into movement and rest. From the start, we shall see the agitation $dAnD$ move as far as B, whence it will return to A, and so on, making each transit in the same time as the string completes one oscillation. Now I shall easily be granted that this motion could in no way be represented by curves of sines."

[While EULER's last sentence is not correct, the example he has just given shows how his old solution predicts *the propagation of a disturbance at speed* c and its *reflection* from the two ends. However, EULER seems to forget the factor $\frac{1}{2}$ in (268 A), and his description of the phenomena is not yet clear.]

EULER's next paper, *On the motion of a string disturbed initially only along part of its length*[1]), works out systematically the important idea just stated.
Directly and easily from the solution (268 A), EULER explains the phenomenon of 20—22
reflection of a pulse, in *reversed form*, from the end of the string. This is most easily seen from his own figures. Figure 83 shows the string of length AB given the initial shape $AMCB$ and works out the construction of the odd periodic function used to determine its motion. The successive figures show the form of the string at the times AD/c, $2AD/c$, . . .

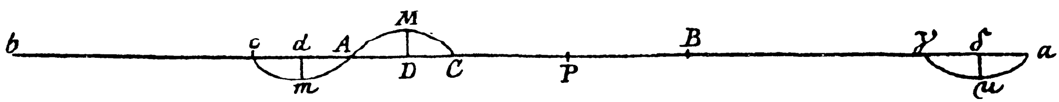

Figure 83. EULER's solution for the propagation of a pulse (1765)

1) E 339, "*Sur le mouvement d'une corde qui au commencement n'a été ébranlée que dans une partie,*" Hist. acad. Berlin [21] (1765), 307—334 (1767) = Opera omnia II 10, 426—450. Presentation date: 18 July 1765.

and finally at time AB/c (Figure 84). [Unfortunately the drawings are not carefully scaled. Also, the "time AD" is special in that its multiples are the only times when the pulse does not have
-25 a tail.] Even more striking is the case of a disturbance CMD initially in the middle of the string AB; it splits into two halves, travelling to the right and the left at the speed c, until each is reflected back from the ends in reversed form (Figure 85).

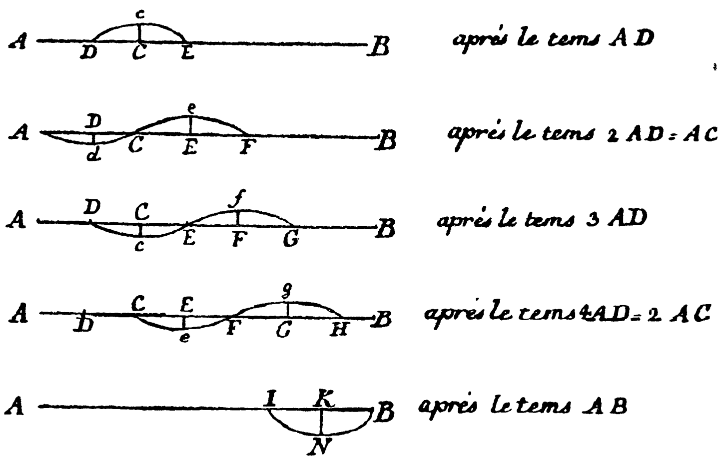

Figure 84. EULER's sketches of successive forms of a string disturbed initially at one end (1765)

[Thus EULER achieves a correct theory of *progressive waves*. In the context of aerial vibration, he gives just at this time a clearer explanation in terms of pulses of zero width, or the *method of images* [1]).]

1 In regard to trigonometric series, EULER begins by asserting that the method of

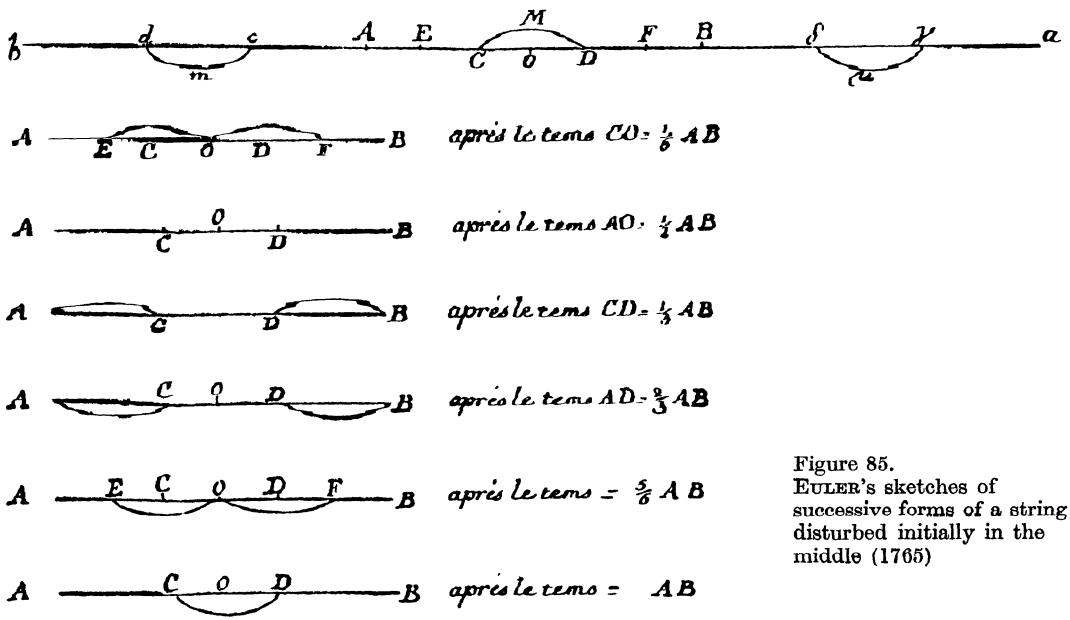

Figure 85. EULER's sketches of successive forms of a string disturbed initially in the middle (1765)

1) *Cf.* pp. LXI—LXII my Introduction to L. EULERI Opera omnia II 13.

Bernoulli is not general enough to include the case when the initial disturbance is zero
along a part of the length of the string; later, however, he admits the possibility that a 7—9
trigonometric series might be found so as to represent a function whose value is zero for a
finite interval, but "the most clever calculator would never come to the end of it." Still 10
later he weakens a little: "... it will be doubtless very difficult, if not to say even im-
possible, to determine the coefficients." Then he objects that such a case cannot be regarded 11—12
as a superposition of several simple and regular oscillations, since each particle remains at
rest a finite length of time. [In all this Euler is wrong, but the burden of proof lay on
Bernoulli to show that such motions could be decribed by his method, and even today it
seems implausible until proved.] Indeed, Daniel Bernoulli did not see how propagating
waves could be explained within the theory based on (251)[1].

To d'Alembert's objection to cases where there are elements at which the differential 5—6
equation is not satisfied, [*e. g.*, when there is a corner,] Euler replies[2]) that "such an error
committed in one or several elements is always infinitely small and will not disturb the
total result of the calculation. ... The same annoyance occurs in virtually all applications
of the integral calculus," where the error made in approximating the area of a curve by
that of a trapezoid is not infinitely small at points where the curve crosses the axis, except
when the tangent at such a point happens to coincide with the axis. "... I do not deny
that in applying the calculus to such a case, one commits some error, but I claim that the
totality of this error becomes infinitely small and entirely zero."

Euler proceeds to consider an illuminating example in which the initial figure has a 27—30
node one third of the way along its length, but the loops are not sinusoidal. He then finds
that "the motion of each element ... is irregular and altogether different from that of a
pendulum ..." The *period* is that for the third harmonic, but the motion is *not* simply harmonic. The elements dividing the string into thirds "seem to complete three vibrations" during one fundamental period; the midpoint changes sides every half period. "Thus the string as a whole emits a certain principal sound; some of its elements seem to emit a tone one octave higher, others a tone higher by a fifth, while others produce the same principal sound. Nevertheless it is necessary to remark that in this case the octave ... is very impure, since the ... times during which the points [which emit it] remain successively above and below the axis are very unequal ..." [This is Euler's nearest approach to what we should call the determination of the relative amplitudes of the harmonics.]

1) On 25 July 1765 he wrote to John III Bernoulli in this context, "I think that in addition to the motion of the parts of the string perpendicular to the axis it is necessary to assume also an infinitely smaller reciprocal motion in the direction of the axis, and above all that the nodes are not at first perfectly at rest and that this rest happens only in formed and permanent sounds."

2) This he wrote also to Lagrange on 16 February 1765.

—48 At the end, EULER adds a "rigorous proof" of his solution[1]). Here he suggests that a polygonal initial form may be justified by deforming the angles slightly so as to form a smooth curve. [This suggestion found no favor in the eighteenth century. Indeed, it does not suffice, but it has since become a favorite tool of physicists, and many a modern textbook derives a jump condition by passing to the limit in appropriately selected continuous solutions.

In EULER's attempts to justify admission of initial figures with corners, we observe the first glimmerings of two ideas since become commonplace :

1. For a fruitful definition of "solution" of a partial differential equation in a region, it is too much to demand that the equation be satisfied on the boundary.

2. A function which is the limit of solutions should also be regarded as a solution.

It is EULER's merit to have sensed these ideas, but it would be too much to say that he formulated them. Considerations of uniqueness, necessary if the usefulness of these ideas is to be apparent, were totally lacking.]

41. Miscellaneous polemics to 1788. D'ALEMBERT, meanwhile, was preparing his *New reflections on the vibrations of sounding strings*[2]), [an accumulation of misunderstandings and of unsubstantiated and often erroneous assertions] written ostensibly in reply to LAGRANGE's reply to D'ALEMBERT's first objections[3]). [As is usual with D'ALEMBERT's polemics, he is so eager to pick flaws that he scans every word and jumps at every minutium, burying his just objections to LAGRANGE's limit process among errors or misunder-
5—0 standings of his own.] *E. g.*, there is a long passage questioning the validity of the series $\sin x = \sum_{n=0}^{\infty} (-1)^n \dfrac{x^{2n+1}}{(2n+1)!}$: "... is it really exact and applicable in all cases?", since it is "very divergent in a great number of its first terms if x is very large ..." Quickly, however, he turns to repeating his old attacks on EULER's solution.

33 As to DANIEL BERNOULLI's preference for finite models, "there may be a great difference between the vibrations of a continuous curve, considered as composed of an infinity of weights, and the vibrations of the same curve considered as loaded by a very great

1) Like his earlier papers, it deals only with the case when $V = 0$; for this and other internal reasons I judge it to be the "new proof, embellished with complete rigor" which EULER wrote in reply to D'ALEMBERT's first attack; *cf.* the letter of EULER to LAGRANGE, 2 October 1759, and my Introduction to EULERI Opera Omnia II 13, p. XXXVIII.

2) *"Nouvelles réflexions sur les vibrations des cordes sonores,"* Opusc. math. 4, 128—155 (1768). In the first supplement immediately following, D'ALEMBERT says this paper was written in 1762, but in it he cites a letter dated 26 July 1763.

3) "Not to prolong this controversy with a savant for whom I am filled with the greatest esteem, and who, moreover, seems to be now almost entirely come over to my opinion, but because it seems to me that [my answer] will cast some enlightenment upon this spiny and delicate discussion, which may be useful on other occasions."

but *finite* number of weights joined by little lines." [The reason for this statement seems to be that in general the proper frequencies are incommensurable in the finite case but commensurable in the continuous limit. When it seemed to his advantage, D'ALEMBERT refused to consider an arbitrarily accurate approximation as a valid answer for a physical problem.]

The following *First supplement*[1]) begins with a sarcastic personal attack on DANIEL 1—12
BERNOULLI. Most of the rest again repeats D'ALEMBERT's old objections against EULER's 29—31
solution. Now, however, D'ALEMBERT has definitely decided that the initial shape
$y = \alpha(\sin x)^{5/3}$, being given by an "equation", is an admissible solution, and since in this
case $d^2y/dx^2 = \infty$ at $x = 0$, he asserts that "for the validity of the solution, it is enough
that $\partial y/\partial t = 0$ when $x = 0\ldots$; it is not at all necessary that $\partial^2 y/\partial t^2 = 0$ [at the end
points], and this does not follow from $\partial y/\partial t = 0\ldots$" Hence the curvature may be infinite 32
at the end points; [D'ALEMBERT seems to realize that this contradicts his earlier violent
contention that the curvature must be zero there,] so he decides that $\partial^2 y/\partial x^2 = \infty$ is permitted only when $t = 0$, not at any other time.

The *Second supplement*[2]), among repetitions of his old claims, takes up LAGRANGE's
last conclusion, *viz*, that if the solution is given by an "equation", it must be representable
as a trigonometric series (above, p. 278). D'ALEMBERT asserts that every trigonometric 11—18
series can be rearranged as a power series; hence LAGRANGE's conclusion implies that every solution [since D'ALEMBERT refuses to admit any not given by "equations"] is representable by a power series. This, however, D'ALEMBERT considers too restrictive, since he now sees that such functions as $(\sin x)^{5/3}$ do not have power series expansions.

Finally D'ALEMBERT insists that $\partial^2 y/\partial x^2 = \partial^2 y/\partial t^2$ cannot hold unless 20
$\partial^n y/\partial x^n = \partial^n y/\partial t^2 \partial x^{n-2}$. [Much as we try to render justice to D'ALEMBERT for his sometimes well taken criticisms, when we run upon nonsense of this kind it is difficult to read further.]

The *Third supplement*[3]) attempts to turn aside EULER's question of what the motion of

1) "*Premier supplément au mémoire précédent,*" Opusc. math. 4, 156—179 (1768). See also the personal remarks on p. x of the Avertissement to the volume. D'ALEMBERT wrote to LAGRANGE on 29 April 1768, "There I handle DANIEL BERNOULLI rather roughly . . ." Indeed, in § 2 he refers to "a famous geometer, who is neither Mr. DE LA GRANGE nor Mr. EULER", *etc.* In reference to DANIEL BERNOULLI's [indeed misty] treatment of the divergent series that often arise at the endpoints in a solution by trigonometrical series, D'ALEMBERT writes "it is not a matter of *conjecturing*, but of *proving*, and it would be dangerous (though, truly, this misfortune is little to be feared) if such a strange kind of proof were to be introduced in geometry. The only surprising thing is that such reasoning should be employed as proof by a famous mathematician . . ." (§ 4). (D'ALEMBERT's own work is full of conjectures, mostly false.)

2) "*Second supplément au mémoire précédent,*" Opusc. math. 4, 180—199 (1768).

3) "*Troisième supplément au mémoire précédent,*" Opusc. math. 4, 200—224 (1768).

2 a string from an arbitrary initial state really is. D'Alembert states that "even the opponents of our opinion" agree that polygonal initial shapes, which are "the most ordinary, and perhaps the only ones that have ever existed for vibrating strings," are excluded. [For Euler's treatment of this case, see below, pp. 289—290.] Experience cannot be adduced in support of Euler's solution, because the theory neglects friction, and "what solid argument can be drawn from an agreement with experience which is not universal in all respects?" [This is a sample of d'Alembert's famous insistence upon the experimental basis of physical science: A theory not representing *every detail* of a physical situation is not fit to be compared to experiment at all!] D'Alembert then goes off into approximate solution of equations representing a vibrating string subject to various laws of resistance. [No definite conclusion results from his pages of formulae.]

Most of these matters are repeated in the *Extract from different letters of Mr. d'Alembert to Mr. de la Grange* [1]), written expressly for publication as a sort of advertisement for
I the paper just described. Here d'Alembert agrees with Daniel Bernoulli that for finite systems, the most general motion may be obtained by superposition of appropriate simple modes, but he denies that "these *multiple* vibrations can be regarded as *really* existing" and that the theory applies in general to a vibrating string loaded with infinitely many
II—V masses. Pages of calculation supposedly show that "the claimed Taylorian multiple vibrations exist only in *idea* and have no more *reality* than they would in a string at rest..." A trigonometrical series cannot represent all motions. 1°, at points where the initial figure has corners, "*dy* has two values", which is manifestly impossible for a trigonometric series; [thus d'Alembert reproaches Daniel Bernoulli with not being able to solve cases which he himself elsewhere categorically asserts to be insoluble]. 2°, $y = \alpha(\sin \pi x)^{5/3}$ gives a solution for which $d^2y/dx^2 = \infty$ at $x = 0$, but any series of sines gives $d^2y/dx^2 = 0$.

VII Euler's proposal to approximate figures with corners by smooth ones d'Alembert rejects flatly as unworthy of geometry [2]).

After he had received this work of d'Alembert, Lagrange wrote on 15 July 1769,

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1) "*Extrait de différentes lettres de Mr. d'Alembert à Mr. de la Grange,*" Hist. acad. sci. Berlin [19] (1763), 235—255 (1770). This work, dated 11 June 1769, was sent to Lagrange with d'Alembert's letter of 16 June 1769.

2) D'Alembert's provocation had finally led Euler to print a sarcastic remark in § 13 of E 339, described above, pp. 283—285): "I have every ground to hope that Mr. Bernoulli will recognize the truth [of my solution], especially when he sees the beautiful agreement with experience; but Mr. d'Alembert doubtless will say that he will refute my solution in some future publication, and for the present he will rest content with notifying the public." To this d'Alembert replies with outraged dignity. "Now I come to the memoir of Mr. Euler ... I pass over in silence the pleasantry which he tries to put upon me on p. 313, since the essential is not to trifle here. You, Sir, who have sometimes rightly opposed me, and without pleasantry, you thought at first as does Mr. Euler, but you have since abandoned that opinion so far that it seems to me you now reduce the solution to too few shapes."

"I admire the constancy with which you are capable of pursuing the same object for so long as time; for my part, unfortunately, if I have to work over the same subject I finally get so violent a revulsion toward it as to make me virtually incapable of coming back to it, and that is exactly what has happened to me in respect to the vibrating string. That is the reason I have always neglected to answer Mr. DANIEL BERNOULLI, though I could do it to advantage." [Indeed, LAGRANGE has retreated from everything he once claimed to have proved regarding the vibrating string and has allowed himself to become a mere foil in the polemics of D'ALEMBERT. He is to publish no further researches on the subject.]

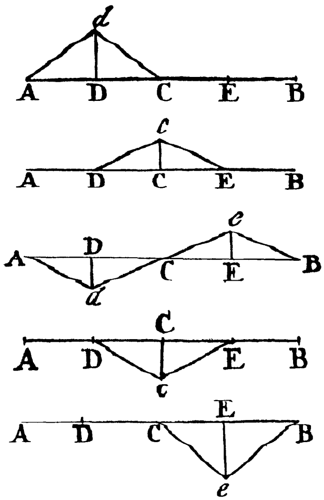

Figure 86. EULER's sketches for the propagation of a triangular disturbance (1772)

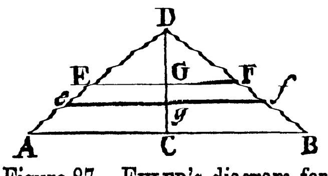

Figure 87. EULER's diagram for determining the motion of a string plucked into triangular form (1772)

EULER's *Further disquisition on vibrating strings*[1]) contains little else than a restatement of his position of twenty years before. In an attempt to justify the use of shapes with corners, he chooses a function with a cusp and verifies that at the cusp (251) is satisfied because each side becomes infinite. [In itself, this is a weak evasion, but it enables us to grasp one of EULER's ideas: if $f(x) = g(x)$ for $x < a$ and for $x > a$, but f and/or g is undefined for $x = a$, we may consider $f(a) = g(a)$; *i. e.*, in modern terms, $x = a$ is a *removable* singularity. However, this does not help to delimit the class of discontinuities consistent with the principles of mechanics.] It was objected, doubtless by physicists, that in fact if a string were bent into a sharp corner, the corner would be smoothed out in the succeeding motion, but according to EULER's solutions the corner remains; to this his answer
is [entirely just]: Physical strings are never entirely devoid of 25
stiffness, while the theory considers only a perfectly flexible
line. The initial form shown in Figure 86 is a striking example 26—32
of the use of "discontinuous" functions, and EULER's exhaustive discussion of the propagation and reflection of the pulse shows his full understanding of the laws of wave propagation[2]).

In reply to D'ALEMBERT's challenge (above, p. 288), 20
EULER discusses an initially triangular form (Figure 87) on

1) E 439, "*De chordis vibrantibus disquisitio ulterior,*" Novi comm. acad. sci. Petrop. 17 (1772), 381—409 (1773) = Opera omnia II 11, 62—80. Presentation date: 24 August 1772.

2) I find no evidence supporting the assertion of HOPPE, p. 133 of *op. cit. ante*, p. 11, that EULER's treatment of reflection here is erroneous. Perhaps HOPPE was misled by EULER's Figures 12 and 14, which are correct for the instants to which they refer but are not typical, and by the fact that EULER does not mention or show the typical trapezoidal form at instants intermediate between those illustrated in his Figures 12—15.

the basis of mechanical principles alone. "Since at the initial instant all elements of the string were at rest and since, as is plain, the tensions in the string are everywhere equal and opposite, so that the several points on the two sides AD and BD are not subject to any force, it is evident that in the first instant all these points take on no motion ...; only the topmost element at the apex D is subject to oblique tensions along the directions DA and DB; thence will arise a force acting along the direction DC, so that the point D will begin to move back in the direction DC, while all the remaining points ... will remain at rest. As soon, however, as this point D begins to move and in the first instant, so to speak, reaches G, now the points E and F are induced to take on motion because the tensions about these points are no longer in equilibrium, while the rest of the points from E to A and from F to B continue to remain at rest. Moreover, the points in the little space EGF, being no longer subjected to force, will hasten toward the axis AB with the motion already acquired. Thus after a certain little time the shape taken on by the string is $AefB$, and so on in this way until it reaches the natural state AB, whence it spreads out in the same way to the opposite side."

[Thus, at last, BEECKMAN's argument is completed, and the problem with which MERSENNE, TAYLOR, and others had struggled in vain is solved once and for all (*cf.* above, pp. 25, 30, 48, 130, 241, 275). But more than this, EULER has at last begun to see how the matter of corners is to be handled: Throwing aside the differential equation, he has appealed directly to the laws of mechanics. The modern student sees that *the laws of mechanics are integral equations*, which imply not only the differential equations of motion but also the conditions of compatibility which must be satisfied at corners or other discontinuities. In this paper of EULER occurs the first dim hint of the correct approach, which was not to be taken up again until CHRISTOFFEL, a full century later, made it the basis of the general theory of singular surfaces in mathematical physics[1]).]

1) In 1877 CHRISTOFFEL wrote that he had been using the method for some years in his lectures, but he did not publish his explanation. See the introduction to his paper, "*Untersuchungen über die mit dem Fortbestehen linearer partieller Differentialgleichungen verträglichen Unstetigkeiten,*" Annali di Mat. pura ed applic. (II) 8, 81—112 (1877) = Werke 2, 51—80. The whole passage is worth noting:

"A very immediate example for this theory occurs in the theory of the taut string. The formulae following from the assumption that the string has everywhere continuous curvature are applied unhesitatingly to the case when there are corners ...; if any reason at all is admitted, it is found in the properties of FOURIER series. But this question has nothing at all to do with FOURIER series, since it depends rather upon two new conditions, a mechanical one for the shock that is experienced by an element of the string which is traversed by a corner, and a phoronomic one, which restricts the discontinuities occurring at a shock in such a way that they do not destroy the connectedness of the string. By the aid of these conditions it is possible to prove, as it has been my custom to do in my lectures for some years past, that indeed the presence of corners has no influence on the end formulae for transverse motion, but this conclusion rests, not upon the properties of FOURIER series, but upon

Euler's *Determination of all motions that may be taken on by a taut and uniformly thick string*[1]) is a short exposition, the object of which seems to be to put into one simple account, free of arguments and proofs, "the very easy constructions, without any calculation . . . by which the form of the string at any time may be drawn . . ."

After a long silence Daniel Bernoulli issued his *More general physico-mechanical paper on the principle of the coexistence of simple undisturbed vibrations in a composite system*[2]). While he writes, "I do not hesitate . . . to place this principle among the most use- 1
ful principles of physical mechanics", [the paper consists in repetitions of old claims and arguments]. After reminding us that his method is good for systems of *finitely* many bodies, 4, 11
no matter how numerous, he claims something regarding the limit to the continuous string, [but what he means is not clear]. He concludes that "if you suspect any restriction in my solution . . ., it consists necessarily in failure to take enough simple vibrations . . .," [but he gives no idea how one would go about determining the proper amplitudes for these vibrations.

Here we insert a description of a paper not pertaining to the controversy but in subject nearer to it than to any other part of our history.] This is Euler's *Consideration of a very special motion possible for a perfectly flexible thread*[3]), which contains the only attempt made in our entire period of study to determine cases of *finite motion* of a string. The 1
want of a theory of finite motion of flexible and elastic threads arises not from any failure of mechanical principles but solely from imperfection of analysis. The equations, set up by 3—6
the balance of moments as in other work of Euler from this period[4]), are

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the fact that the above-mentioned singularities are such as to be compatible with the permanence of the linear partial differential equation, so often treated since Euler's time."

For the details of Christoffel's justification of Euler's solution, see Ch. IX, § 1, ¶ 6 of Ph. Frank & R. v. Mises, *Die Differential- und Integralgleichungen der Mechanik und Physik* 2, Braunschweig, Vieweg, 1935.

Note that Christoffel's objection to justifications based on the theory of Fourier series applies equally to the methods of some modern pure mathematicians who rely on the completeness of certain function spaces, *etc.*

1) E 535, "*Determinatio omnium motuum quos chorda tensa et uniformiter crassa recipere potest,*" Acta acad. sci. Petrop. 1779: II, 116—125 (1783) = Opera omnia II 11, 269—279. Presentation date: 17 October 1774.

2) "*Commentatio physico-mechanica generalior principii de coexistentia vibrationum simplicium haud perturbatarum systemate composito,*" Novi comm. acad. Petrop. 19 (1774), 239—259 (1775).

3) E 618, "*Consideratio motus plane singularis qui in filo perfecte flexili locum habere potest,*" Nova acta acad. sci. Petrop. 2 (1784), 103—120 (1788) = Opera omnia II 11, 355—372. Presentation date: 5 June 1775.

4) *E. g.* in E 481, described below, § 58.

$$\begin{aligned}(308)\qquad &\frac{\partial y}{\partial s}\int\left(\sigma\frac{\partial^2 x}{\partial t^2}-F_x\right)ds=\frac{\partial x}{\partial s}\int\left(\sigma\frac{\partial^2 y}{\partial t^2}-F_y\right)ds\left[=T\frac{\partial x}{\partial s}\,\frac{\partial y}{\partial s}\right],\\ &T=\frac{\partial x}{\partial s}\int\left(\sigma\frac{\partial^2 x}{\partial t^2}-F_x\right)ds+\frac{\partial y}{\partial s}\int\left(\sigma\frac{\partial^2 y}{\partial t^2}-F_y\right)ds\,,\quad\left(\frac{\partial x}{\partial s}\right)^2+\left(\frac{\partial y}{\partial s}\right)^2=1\,.\end{aligned}$$

7—10 "I do not hesitate to publish here my vain attempts, since perhaps they may furnish others occasion to undertake this work with happier event." There follows a transformation of (308) into the form (222), [which EULER apparently forgets he had derived directly in his first attempt, thirty years earlier, to obtain the equations of finite motion].

11 Not having been able to draw any fruit from this direct attack, "I have decided to treat this subject in inverse order; that is, I shall regard the shape of the thread as given at
12 all times and I shall seek the forces F_x and F_y requisite to cause such a motion . . ." EULER then considers the case when the string is a circle of radius r with the end $s = 0$ a fixed point:

$$(309)\qquad x = r\sin\frac{s}{r}\,,\quad y = r\left(1-\cos\frac{s}{r}\right),$$

where $r = r(t)$. EULER supposes $r(0) = \infty$, so that the form is a straight line at $t = 0$.
14 He wishes to impose also the condition $\partial x/\partial t = \partial y/\partial t = 0$ when $s = 0$, but in view of
15—24 the double limit involved is able to assert no precise condition on r. A long calculation leads to the following expressions for the tangential and normal components of the force required to effect the motion:

$$\begin{aligned}(310)\qquad &F_t = F_x\cos\frac{s}{r}+F_y\sin\frac{s}{r} = -\frac{\partial T}{\partial s}+\sigma r''\left[\sin\frac{s}{r}-\frac{s}{r}\right],\\ &F_n = F_y\cos\frac{s}{r}-F_x\sin\frac{s}{r} = -\frac{T}{r}+\sigma\left[r''\left(\cos\frac{s}{r}-1\right)+\frac{r'^2 s^2}{r^3}\right],\end{aligned}$$

25—32 where $T(s, t)$ is the tension. The paper concludes with explicit determination of two of the
33 three quantities F_t, F_n, and T when one is assumed to vanish. EULER remarks on the multiplicity of forces under which the same assumed motion (309) may take place. He considers these results extraordinary because the only ones ever obtained concerning finite motion of a string subject to distributed load.

LAPLACE's *Memoir on sequences*[1]) proposes to resolve the whole problem of the vibrating string by replacing the differential equation (251) by a finite difference equation:

$$(311)\qquad y_{x,\,x_1+1}-2y_{x,\,x_1}+y_{x,\,x_1-1}=y_{x+1,\,x_1}-2y_{x,\,x_1}+y_{x-1,\,x_1}\,,$$

1) "*Mémoire sur les suites,*" Mém. acad. sci. Paris 1779, 207—309 (1782) = Œuvres 10, 1—89. See § XXII. This passage is taken over almost verbatim into § 19 of Book I of *Théorie analytique des probabilités*, 3rd. éd., Paris (1820) = Œuvres 7.

where x_1 is proportional to t. Such difference equations LAPLACE has solved in an earlier part of the memoir. He concludes that "this analysis of the vibrating string establishes ... in an incontestable way the possibility of admitting discontinuous functions in this problem ... Since nothing is neglected in the theory of finite difference equations, it is plain that the arbitrary functions in their integrals are not subject to any law of continuity and that the construction of these equations by the means of polygons holds irrespective of the nature of these polygons. When one then passes from the finite to the infinitely small, these polygons change into certain curves which, consequently, can be discontinuous ..." [Nothing is proved. First, to replace the acceleration by a finite second difference brings us further away, not nearer to the principles of mechanics. Second, that arbitrary polygonal figures are possible for the discrete case is obvious and had never been questioned by anyone. Third, that such polygonal figures may approach in the limit functions which are "discontinuous" is also obvious and does not prove that those functions satisfy the conditions of the continuous problem.] LAPLACE retreats at once with the remark that in order to satisfy a partial differential equation of order n, a function must have continuous derivatives of orders $1, 2, \ldots, n-1$. "This condition is necessary in order that the proposed differential equation can hold .. " [While LAPLACE does not state his opinion clearly, the context makes it plain that he insists that the partial differential equation of the n^{th} order be satisfied *everywhere;* therefore the n^{th} derivatives must exist, but they need not be continuous. Consequently the derivatives of lower order are continuous in each variable separately.] Thus LAPLACE concludes that polygonal initial figures for the string cannot be admitted "geometrically", although "physically ... one sees *a priori*" that the motion differs very little from that of a string with these corners rounded off [as EULER had asserted long before].

D'ALEMBERT, still alive, objected. In his reply[1]), dated 10 March 1782, LAPLACE's attempt to answer D'ALEMBERT's favorite question regarding the accelerating force at a point where $\partial^2 y/\partial x^2$ is discontinuous [shows that LAPLACE is as far as anyone else from a grasp of the mechanical problem]. In his view, $\partial^2 y/\partial x^2$ must exist at all points. However, his geometric definition of $\partial^2 y/\partial x^2$ is equivalent not to $\dfrac{\partial}{\partial x}\left(\dfrac{\partial y}{\partial x}\right)$ but to

$$\lim_{h\to 0}\frac{y(x+h,t)-2y(x,t)+y(x-h,t)}{h^2}.$$

This limit may exist at $x=a$ and may differ from the two limiting values[2]) of $\partial^2 y/\partial x^2$ as

1) Œuvres de LAPLACE 14, 351—354 (1912).

2) It would be prolepsis to expect any mathematician of the eighteenth century to take account of the possibility that there be a differentiable function such that $\lim\limits_{x\to a+0} f'(x) = \lim\limits_{x\to a-0} f'(x) \neq f'(a)$. Indeed, if the limiting values of $f'(x)$ as $x \to a \pm 0$ are *finite*, the foregoing inequality must be replaced by equality.

$x \to a \pm 0$. According to LAPLACE, the accelerating force per unit length is always $T\dfrac{\partial^2 y}{\partial x^2}$. While LAPLACE makes it plain that he considers his solution to supersede at a blow all the results of all prior authors, [he fails to notice that for a polygonal figure, which he had agreed must be excluded "geometrically", the above limit exists and is infinite! This is precisely what the mechanical problem demands. Indeed, the old argument JAMES BERNOULLI used to derive (40) shows that the resultant normal force F_n of the tension on the element ds is given to the first order in ds by $T d\theta = F_n ds$, where $d\theta$ is the angle of contact. For a polygonal figure with small $d\theta$ this formula holds *exactly* as a finite difference expression in ds, with F_n being the force along the bisector of the angle between the segments, but $d\theta \nrightarrow 0$ as $ds \to 0$; hence $F_n \to \infty$. This in no way precludes application of the principles of mechanics, with which EULER's solution for polygonal initial shapes is in perfect accord.]

The first voice from the isles since TAYLOR's analysis of the vibrating string in 1713 is raised by MATTHEW YOUNG, whose book, *An enquiry into the principal phaenomena of sounds and musical strings*[2]), appeared in 1784. Its purpose seems to be to present in non-mathematical language some of the principal results and to show that the view of DANIEL BERNOULLI suffices to account for all the observed phenomena. YOUNG's only originality
28 lies in his experiments. For example, he is able to distinguish the constituent harmonic motions visually by noting that the corresponding speeds of propagation of disturbances are different: "When I pulled [a string] by a point near either of the extremities . . ., I observed two forms of chord, one vibrating very rapidly, while, at the same time, the other appeared to roll slowly backwards and forwards, and to cross the former . . . Sometimes, by striking the chord at random, I have seen three or four of these apparent chords crossing each other with various velocities . . ."

51—53 YOUNG takes pains to controvert experimentally all of D'ALEMBERT's objections that refer to the physical side of the question. *E. g.*, in reference to D'ALEMBERT's claim that the small observed motion at the theoretical nodes contradicts BERNOULLI's theory (above, p. 262), YOUNG, taking a string held taut by a weight and screwing on a heavy plate part way along, finds that both the plate and the weight move somewhat when the string is struck at a point between them, but the string of given length and tension emits the same note, "whether both extremities were fastened, or permitted to vibrate freely."

23—43 While it is BERNOULLI's theory to which his experiments refer directly, M. YOUNG

2) (vi) + 203 pp., London, G. Robinson, 1784. Except for the works of HOOKE and the short notes by WALLIS and ROBERTS, this seems to be the only work in English pertaining to any part of our subject.

develops the properties of vibratory motion of strings by what amounts to EULER's method, without equations [or reference to EULER[1])].

LAGRANGE's "violent revulsion" toward the problem seems to have continued until 1788, since in his *Méchanique Analitique* he does not treat it[2]).

42. Summary of the theory of the vibrating string to 1788. Despite the great attention given by historians and dilettantes to this controversy, it has been little understood. From the foregoing extracts I draw up a summary.

1. While TAYLOR in 1713 gave all the analysis a modern student needs to derive the partial differential equation (251), neither he nor any of his contemporaries understood the mechanical problem. TAYLOR was the first to calculate the fundamental frequency (75) correctly from dynamical principles. He failed to observe the possibility of any kind of motion other than the fundamental mode.

2. In 1727 JOHN BERNOULLI proposed the model of the massless string loaded by n

1) *Cf.* the similar borrowing by T. YOUNG as described above, p. 248. That M. YOUNG knew EULER's papers at first hand is shown by his remarks and references in §§ 44—45.

2) In an offhand way he mentions the loaded taut string in ¶ 34 of Seconde Partie, Sect. V, § III; the continuous string, in ¶ 42.

When the second edition appeared in 1811, all the original parties to the dispute except LAGRANGE himself were dead, and he as dean of the French geometers could write anything he pleased without fear of being questioned. He added a new section on vibrating systems. In the "Avertissement" he says this new section "ends with the theory of the vibrating string, first achieved by me and published in Volume 1 of the *Turin Memoirs*, which is presented here in a simpler way, exempt from the objections ... made by D'ALEMBERT in the first volume of his *Opuscules*." In ¶¶ 33—38 of Sect. VI of Part II we find essentially LAGRANGE's second treatment (above, pp. 278—279) of the loaded taut string, now based on his general theory of small oscillations and generalized to three-dimensional small displacement. ¶¶ 44—55 contain the limit process just as in LAGRANGE's first attempt (above, pp. 269—270), now supported by such phrases as "it follows from the known theory of these series." Referring to the work of DANIEL BERNOULLI, LAGRANGE writes in ¶ 47 that in order to explain harmonic sounds on the basis of a trigonometric series solution, we should have to suppose the first coefficients "much greater than all the rest taken together ..." and that "the coefficients ... form extremely convergent series. But, from the manner in which these coefficients depend upon the initial values ..., one sees that this supposition is inadmissible if we regard the initial state as arbitrary; one sees even that in most cases the coefficients form divergent series, but this does not prevent the string from making isochronous vibrations ..." LAGRANGE still claims that in the limit to the continuous string "the series which could give these different sounds disappears," but then he goes on to explain how the initial shape could be regarded as being the sum of appropriate curves with 1, 2, 3, ... like branches, but "such a composition being only hypothetical, its consequences ... would be altogether precarious" (¶ 59). In ¶¶ 61—62 LAGRANGE in summarizing the controversy between EULER and D'ALEMBERT reverts entirely to the view expressed in his own first paper, namely, that his limit process justifies EULER's solution. But he adds that "the principle of the discontinuity of the functions is now generally received for the integrals of all differential equations ...," citing in support the geometrical researches of MONGE.

equally spaced and equal discrete masses, calculated the restoring force (78), derived the equation of the proper frequencies for $n = 1, 2, \ldots, 6$, but gave the solution only for the fundamental frequency in each case[1]). He seemed not to realize the existence of other possible frequencies.

3. From 1733 onward DANIEL BERNOULLI studied the simple modes of many dynamical systems of finitely or infinitely many degrees of freedom. He calculated the proper frequencies and shapes of the simple modes correctly, and indeed he was the first to grasp the totality of simple harmonic motions of which such systems are susceptible. By 1739, if not earlier, he had been led by these special cases to infer the general principle of the coexistence of small harmonic oscillations as sufficient to yield the most general motion of any vibrating system, but he prepared this material for publication only in 1753, in particular reference to the vibrating string. DANIEL BERNOULLI regarded his principle as an *a priori* law of physics rather than a demonstrated theorem of rational mechanics.

4. To D'ALEMBERT belongs certain priority for deriving the wave equation (251) by 1746 and for obtaining its formal solution (257). These results once derived, however, D'ALEMBERT strove with might and main to prevent their application except subject to limitations which now, at least, seem merely arbitrary. Though some of D'ALEMBERT's criticisms of the work of others were sound, most were merely destructive in the context of the times, devoid of insight into the mechanical principles or analytical concepts whereby the pitfalls may be bridged or outflanked, and had his objections been heeded, they would have but let the progress of mechanics.

5. In this connection EULER approached the concept of function of a real variable. While he failed to justify in all respects his derivation of (257) with Φ and Ψ interpreted as *arbitrary* piecewise smooth functions, this failure resulted partly from his obstinate insistence that solutions with corners are admissible. While this is true[2]), concepts of analysis more general than those received in the eighteenth century are needed to substantiate it. Had EULER been content to compromise by admitting only solutions having continuous curvature[3]), all solid ground would have fallen from D'ALEMBERT's objections. The end points are a more difficult matter. It is now plain, but plain only from accumulated mathematical experience, that it is too much to demand that the solution of a physical problem satisfy the governing differential equations at boundary points. EULER did not state this

1) This work is not anticipated by the inconclusive notes of HUYGENS on the same problem.

2) The justification by CHRISTOFFEL, and EULER's hint toward it, are mentioned above, p. 290.

3) EULER finally found two simple transformations by which he proved absolutely rigorously that (257) is the general solution of (251) as far as solutions of class C^2 are concerned. The first is given in his letter to LAGRANGE of 9 November 1762 and in §§ 4—10 of E 319, "*Recherches sur l'intégration de l'équation* $\left(\frac{ddz}{dt^2}\right) = aa\left(\frac{ddz}{dx^2}\right) + \frac{b}{x}\left(\frac{dz}{dx}\right) + \frac{c}{xx}z$," Misc. Taurin. 3_2 (1762/1765), 60—91 (1766) = Opera

clearly, and had he done so, no one would have been convinced. Determination of the singularities admissible in solutions of partial differential equations, especially hyperbolic ones, lay long in the future. Rather, it is EULER's signal merit to have been led by a most secure intuition to results which the subsequent course of mathematics and rational mechanics has justified in all detail, though he himself lacked the experience and the apparatus to present an adequate argument for them. In every paper, and indeed in every personal letter touching the subject, EULER emphasized that the introduction of "discontinuous" functions and partial differential equations opened "a wholly new part of analysis." While EULER devoted many papers to these subjects, his prophetic remark found little response from other mathematicians until after his death [1]).

6. In EULER's treatment, as he stated emphatically again and again, the question of expansion of functions in trigonometrical series *does not arise;* of course, this purely analytical question is *irrelevant to the mechanical problem* [2]). It is abundantly clear, however, that *no one* in the eighteenth century understood EULER's elegant use of the functional equations to solve the entire problem of the vibrating string, though in every paper he explained his method afresh. (It is the method nowadays often attributed in the special case of initial rest to T. YOUNG.)

7. Everything concerning the uniform string that BERNOULLI or anyone else in the eighteenth century was able to derive from the theory of simple harmonic modes, EULER derived correctly and often in greater generality by the solution in arbitrary functions.

8. As was seen at once, in order to justify BERNOULLI's viewpoint *mathematically* it was necessary to prove that any function $f(x)$ defined in the interval $0 \leqq x \leqq l$ and sufficiently smooth to be an admissible initial shape for the string may be expanded in an infinite trigonometrical series. This and other mechanical problems gave rise to a long series

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omnia I **23**. 42—73. Both are presented in §§ 296—300 of *Institutionum calculi integralis* **3**, Petrop. (1770) = Opera omnia I **13**; *v.* §§ 333, 343, 353—356.

1) The development of the concept of function and integral of a partial differential equation began with the work of ARBOGAST in 1791, soon thereafter to be taken up by the great French mathematicians of the early nineteenth century. See § 13, § 38, *et passim* in BURKHARDT, *op. cit. ante*, p. 11.

Even after the mathematical researches on the propagation of waves had made the identification of function with algebraic expression obviously useless, it lingered on. *Cf.* the remark of STOKES, "*On a difficulty in the theory of sound,*" Phil. Mag **33**, 349—356 (1848) = (abridged) Papers **2**, 51—55: "By the term *continuous function*, I here understand a function whose value does not alter *per saltum*, and not (as the term is sometimes used) a function which preserves the same algebraical expression. Indeed, it seems to me to be of the utmost importance, in considering the application of partial differential equations to physical, and even to geometrical problems, to contemplate functions apart from all idea of algebraical expression."

2) *Cf.* the remark of CHRISTOFFEL, above, p. 290.

of researches in pure mathematics[1]). These investigations brought nothing to the understanding of the motion of the vibrating string; rather, they diverted the disputants from the mechanical problem.

9. To justify BERNOULLI's viewpoint *physically* requires at least some good numerical examples. Neither BERNOULLI nor any one else in the eighteenth century ever attempted to fit a trigonometrical series to a numerical case and to compare the resulting approximate solution with experimental data. In particular, BERNOULLI never gave any solution corresponding to the propagation of a pulse and never explained satisfactorily[2]) the phenomenon of reflection from the ends, while EULER's theory handled these matters easily. BERNOULLI rather sensed than exhibited the strength of his method.

10. The modern reader sees two advantages in BERNOULLI's method:

a) The relative amplitudes of the harmonics may be calculated. Thus, for example, the predominant tone of a composite sound may be determined.

b) For many partial differential equations where a solution in arbitrary functions is not known, BERNOULLI's method leads to a solution in terms of a series of proper functions.

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1) The history of these developments has been written with finality by BURKHARDT, §§ 14—18 of *op. cit. ante*, p. 11, yet it must be admitted that BURKHARDT's presentation is influenced by a prejudice, quaint to the modern reader, in favor of convergence at the expense of all other processes for summation of series.

Most perplexing is that alongside the violent polemics over the vibrating string there was a steady development of the theory of interpolation by trigonometric polynomials and series. What can only be described as a very near miss had been obtained by EULER in 1729 and published by him in 1750. EULER's result, which is *formally* identical with the "FOURIER expansion" of an arbitrary periodic function, including determination of the coefficients by integrals, is printed in footnote 2, p. 278, above. In 1747 EULER obtained results which can now be recognized as interpolatory formulae, to arbitrarily high order, for the coefficients in the trigonometrical series for a certain function arising in the theory of celestial perturbations. D'ALEMBERT, treating the same problem in 1754, by use of a recursion formula obtained the expressions for the first two coefficients by integrals. In a brilliant astronomical work of 1757, CLAIRAUT set up the problem of determining the coefficients in a cosine series for an arbitrary function and solved it, explicitly and generally, by interpolation. Passage to the limit of small intervals yields the integral expression for the coefficients. All this work was in print by 1759, the year of LAGRANGE's memoir deriving (299), from which, granted his presumptions, he could have inferred that any function has a trigonometrical expansion and could have read off the formulae for the coefficients, but he did not do so. Moreover, many special trigonometric series had been determined explicitly by various authors. More revealing investigations came later, after the heat of the controversy was past. BERNOULLI himself in a work published in 1773 first noticed that in special cases the sum of a trigonometrical series may be given by different algebraic expressions in different intervals. Even later came the first determination of the coefficients by use of the orthogonality of the trigonometric functions. This was first achieved by EULER in a work written in 1777 but not published until 1793.

2) *I. e.*, from the trigonometrical solutions, not merely from physical intuition.

Neither of these advantages was stated or illustrated by BERNOULLI. Vague hints of the former may be found in the work of EULER[1]); as we shall see below (pp. 311—212), a step toward the latter was made by D'ALEMBERT; but both were reserved for the next century to exploit.

11. EULER understood BERNOULLI's views and respected them as important for physics. Indeed, as shown by examples in Part II and Part IV of this history, for more complex systems EULER calculated simple modes and proper frequencies with great skill and accuracy, and to him are due most results of this class obtained before 1800. However, he took an inexplicable *dislike* of trigonometric series. Toward the end of his life he came grudgingly to the admission that they *might* be generally valid[2]), but though he had in his own hands all the formal apparatus[3]) needed to exploit BERNOULLI's viewpoint, he never attempted to do so.

12. For the loaded string, EULER obtained the proper frequencies and the general solution[4]) in 1748. In 1759 LAGRANGE obtained the explicit solution of the general initial value problem. His subsequent passage to the limit of the continuous string was gained only by non-trivial fallacies, and his later worked served but to obfuscate the subject by a cloud of calculation as he retreated from his original position. In particular, his reiterated claim to have proved DANIEL BERNOULLI's method wholly false for the continuous string rests only on his own misunderstandings.

13. While the other disputants contented themselves with defending their views by

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1) *E. g.* § 4 of E 213 (cited above, p. 259): "an infinity of sounds, the highest of which will become more and more faint." Also § 30 of E 339, quoted above, p. 285.

2) In the introduction to E 567, cited below, p. 315, EULER wrote, 'Rather often I have warned that in questions of this kind the perfect solution should be distinguished from general solutions, which contain in themselves all possible solutions. Since they consist in an infinite number of terms, they cannot at all be adjusted to those cases when the initial shape of the string is prescribed, unless indeed one were to determine infinitely many constants, which would surpass all the strength of analysis. For the perfect solution . . . is required a finite formula, the application of which to arbitrary initial states can actually be carried out. For strings uniformly thick the most celebrated LA GRANGE and I long ago presented such a solution . . ., by means of which the motion of the string corresponding to an arbitrary initial state is very easily determined, which for other solutions, even if they include all possible motions, is not at all the case. As an example of this sort I proposed long ago the string disturbed initially along only a part of its length, and this case no one so far has been able to handle by means of those formulae of sines progressing to infinity." EULER repeats this view in § 14 of E 576, cited below, p. 317.

D'ALEMBERT and LAGRANGE persisted in categorical denial that trigonometrical series could represent "discontinuous" functions.

3) *Cf.* footnote 1, p. 298.

4) In the context of longitudinal motion; *cf.* especially footnote 1, p. 232 above.

repeating them, EULER to the end of his life *continued to work*, solving new and illuminating cases no one else could approach and proving the fruitfulness of his ideas by extending them to further problems. EULER's papers on the vibrating string, often repetitious and falling short of the level of his other works on mechanics, sometimes display a prejudice close to an *idée fixe;* nevertheless each of them, with the exception of the last one, offers something really new. Polemics aside, there is in them a *positive spirit* which continues to expand the bounds of mechanics and to accumulate *clear and definite results.*

Part IV. Researches subsequent to EULER's "First principles of mechanics", 1752—1788

IV A. The non-uniform vibrating string

43. The earliest researches on non-uniform strings: Simple modes for special cases, and EULER's general inverse method (1752—1765). Dynamical principles sufficient to yield (251) as the differential equation governing small motion of a string of uniform density σ show at once that for small motion of a string of density $\sigma(x)$ we obtain the same equation[1]), with $c^2 = T/\sigma(x)$. In 1753 DANIEL BERNOULLI suggested that since, [as he conjectured,] the proper frequencies of the non-uniform string may be incommensurable, the methods of D'ALEMBERT and EULER are inapplicable[2]). EULER replied[3]) at once that not incommensurability but want of analysis impedes further progress : All that is needed is the general solution of (251) with c^2 being an assigned function of x. BERNOULLI, however, unconvinced, five years later boasted that his method alone is applicable[4]).

Late in 1759 EULER proposed to LAGRANGE the similar problem of determining the small longitudinal motion of air in a wedge; this quickly led both these geometers to consider a class of solutions[5]) of the type

$$(312) \qquad y = \sum_k \mathfrak{A}_k(x)\,[\Phi^{(k)}(x+ct) + \Psi^{(k)}(x-ct)]\ ,$$

where the superscript (k) denotes the k^{th} derivative, and where the sum may be finite or infinite; when the sum is finite, such solutions exist only for certain density functions or certain cross-sectional areas, respectively. LAGRANGE wrote to EULER on 1 March 1760 that he had found solutions of this kind for a class of equations including (251) when $c^2 \propto x^{-n}$, but that such solutions are "exact" only when $\pm \dfrac{2}{2-n} - 1$ is a positive integer; by "exact" he appears to mean that the series (312) is then finite. An assertion of this kind, in

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1) EULER observes this on p. 179 of Notebook EH 6, just following his use of his "first principles of mechanics" to determine the equations of motion of several other continuous systems (above, p. 254). The date of this passage seems to be about 1752.

2) § XXI of *op. cit. ante*, p. 255.

3) § 45 of E 213, cited above, p. 259.

4) On p. 166 of *op. cit. ante*, p. 262, he writes, "What could reveal still more the superiority, the excellence, and the generality of my principles is that they have led me so far as to determine the vibrations, the absolute motion, the properties of the tones, *etc.*, for taut strings that are not uniformly heavy. Mr. EULER has recognized [this subject] to be beyond the sphere of activity of his method. I am very sure, knowing the merit of this illustrious geometer, that he will be able to supply this defect as soon as he learns the thing is possible. In this case, I should be very curious to learn of his results, and I am sure that he would then retract his prejudice against the generality of my method."

5) This development, as far as aerial vibrations are concerned, is traced on pp. XL—XLI, XLVII—L, LXV—LXVII of my Introduction to L. EULERI Opera omnia II **13**.

the form that $n = \dfrac{4\mu - 4}{2\mu - 1}$ where μ is an integer, LAGRANGE soon published[1]), based on transformation of the partial differential equation into that for aerial vibrations. LAGRANGE concludes that unless $n = 0$, "the vibrations will never be isochrone." [This is doubly false : The simple modes, for any vibrating system, are always isochrone, and furthermore there is a certain class of functions c^2, soon to be exhibited by EULER, for which all motions are isochrone.] At about this time D'ALEMBERT published[2]) a few special formal solutions, from which no definite conclusions are drawn.

The first substantial results are obtained in EULER's paper, *On the vibratory motion of*
14 *non-uniformly thick strings*[3]). Since (251) is linear even when $c^2 = \sigma(x)/T$, we may extend also to strings of variable thickness "that splendid physico-musical theorem, put forward by the very famous BERNOULLI, that those sounds which a string may emit separately it may also emit simultaneously."

15—19 Putting $y = v\Phi(u)$, where v and u are functions of both x and t, EULER calculates conditions necessary and sufficient that (251) be satisfied identically in Φ, Φ', and Φ''. One of these yields $u = t \pm \int \dfrac{dx}{c(x)}$, but EULER is not able to solve the remaining equations except in a special case which leads to the line density

$$\sigma = \frac{\sigma_0}{\left(1 + \frac{x}{\alpha}\right)^4} \tag{313}$$

and the corresponding general solution

$$y = \left(1 + \frac{x}{\alpha}\right)\left[\Phi\left(\frac{x}{1 + \frac{x}{\alpha}} + c_0 t\right) + \Psi\left(\frac{x}{1 + \frac{x}{\alpha}} - c_0 t\right)\right], \tag{314}$$

20—36 where $c_0 = \sqrt{T/\sigma_0}$. To this solution, EULER's methods for the uniform string, included as the case when $\alpha = \infty$, are easily extended. In particular, the frequencies of periodic oscillations of a string of length l are given by

$$\nu_k = \frac{k}{2l}\left(1 + \frac{l}{\alpha}\right)\sqrt{\frac{T}{\sigma_0}}\,. \tag{315}$$

The ratio of succeeding tones is thus the same as for a string of uniform thickness, but the fundamental frequency is no longer inversely proportional to the length. [Though EULER shows in detail how to calculate the motion in the case when the initial velocity is zero,

1) § 33 of *op. cit. ante*, p. 273.

2) § III of *op. cit. ante*, p. 274.

3) E 287, "*De motu vibratorio cordarum inaequaliter crassarum*," Novi comm. acad. sci. Petrop. 9 (1762/1763), 246—304 (1764) = Opera omnia II 10, 293—343. Presentation dates: 21 February and 1 December 1760.

he does not mention what is now called dispersion. He does not see how to calculate the speed of propagation of a disturbance down the string.] EULER suggests that (313) may be 37
the only density for which the ratio of frequencies are 1, 2, 3, ... He has studied other laws for σ, but the results are complicated and seem to be useless.

(DANIEL BERNOULLI, meanwhile, had calculated some simple modes for a class of densities included in (313)[1].)

Regarding it particularly illuminating to consider a case when $\sigma(x)$ itself is discon- 38—44
tinuous, EULER finds the motion of a string composed of two uniform portions joined together. The solution for the case of zero initial velocity is

$$y = \begin{cases} \Phi(x+mt) + \Phi(x-mt) & \text{for the left half,} \\ \varphi(x+nt) + \varphi(x-nt) & \text{for the right half,} \end{cases} \tag{316}$$

where x is measured positively from the left end for the left half, from the right end for the right half. The condition that the ends $x = 0$ be fixed shows that Φ and φ are both

1) He considered the case when $\alpha = l$. In an unpublished letter of 7 December 1763 to JOHN III BERNOULLI he gives the corresponding special case of (315) in the form

$$\nu_k = \frac{k}{2l}\sqrt{\frac{7}{6}\,\frac{T}{M/l}}$$

where M is the mass of the string. He gives also the following ratios of the distances of nodes from the denser end to the whole length:

Mode	Nodal ratios
2	$\frac{1}{3}$
3	$\frac{1}{5}$, $\frac{3}{10}$

"It is thus very remarkable that the times for the vibrations of each order are always multiple sounds of the times of fundamental vibration ... as in strings that are uniformly thick. Thus one can apply to this string, too, the theorem of Mr. D'ALEMBERT. But calculation has shown me that almost all other strings fail to have this property. Since I respect very much the lights and the color [candor?] of Mr. EULER, he would give me much pleasure were he so kind as to try his method on strings of non-uniform thickness."

A fragment of EULER's reply, late in December, to JOHN III BERNOULLI is preserved in the Gotha University Library. It summarizes the contents of the first half of E 287, which had been complete for at least three years.

An undated letter of 1764 from DANIEL BERNOULLI to JOHN III BERNOULLI states that "... the strings with thickness proportional to $(\alpha + x)^{-4}$ are not the only ones, as Mr. EULER believes, that can produce regular vibrations. I can give infinitely many more. I have examined also what happens when the string is composed of two parts, each one uniformly thick, but of thickness unequal to one another's."

Letters of DANIEL BERNOULLI to CLAIRAUT of 27 December 1763 and 15 January 1764 show only that the former was studying non-uniform strings and that the latter was not *au courant* in the controversy over the uniform string.

odd. At the junction, the two displacements are equal and *the two parts have a common tangent;* hence

$$\begin{aligned}\Phi(a+mt)+\Phi(a-mt)&=\varphi(b+nt)+\varphi(b-nt)\ ,\\ \Phi'(a+mt)+\Phi'(a-mt)&=-\varphi'(b+nt)-\varphi'(b-nt)\ .\end{aligned}\tag{317}$$

Since these conditions are to hold for all t, we may integrate the latter and obtain

$$n\Phi(a+mt)-n\Phi(a-mt)=-m\varphi(b+nt)+m\varphi(b-nt)\ .\tag{318}$$

Hence

$$\begin{aligned}\Phi(a+mt)&=\frac{2m}{m+n}\varphi(b-nt)-\frac{n-m}{m+n}\Phi(a-mt)\,,\\ \varphi(b+nt)&=\frac{2n}{m+n}\Phi(a-mt)+\frac{m-n}{m+n}\varphi(b-nt)\ .\end{aligned}\tag{319}$$

The initial shape of the curve is defined by $\Phi(x)$ for $0\leqq x\leqq a$ and by $\varphi(x)$ for $0\leqq x\leqq b$, and we have seen that Φ and φ are odd. This information put into (319) suffices to determine Φ and φ for all values of x, whence the general motion follows immediately by (316).

45—46 From this solution EULER shows that the two parts do not generally reach their maximum displacement simultaneously, "and thus there can be no question of the vibra-
47—49 tions made in a given time, or of the sound that a string of this kind emits. We get a regular and isochronous motion not only in the case of uniform thickness, $n=m$, but
50—53 also when $a/b=m/n$; equivalently, when the masses are inversely as the lengths. In this case the fundamental frequency is

$$\nu_1=\frac{1}{4a}\sqrt{\frac{Ta}{M_a}}=\frac{1}{4b}\sqrt{\frac{Tb}{M_b}}\ ,\tag{320}$$

where M_a and M_b are the masses of the two parts. This is one half of the fundamental frequency of a string of length a and density M_a/a, stretched by the same weight. EULER gives also an interpretation equivalent to this: Set $M=M_a+M_b$, $M_a=\beta M$, $M_b=\gamma M$, with $\beta+\gamma=1$. Then the mass P which a uniform string must have in order to emit the fundamental frequency (320) when stretched by the same force T is $P=4\beta\gamma M$.

54 EULER then considers, in the case when $m/n=\frac{1}{2}$, the illuminating example occa-
55—56 sioned by an initial shape which is an equilateral triangle. By using (319), he constructs the curves representing Φ and φ, shown as the upper and lower polygons in Figure 88. The initial position of the midpoint junction, in both curves, is D, and the two ends are A and
57 B. EULER does not sketch any of the successive shapes of the string which follow from these curves, but he calculates and illustrates the position of the midpoint at twelve equidistant times. "Hence it appears that the vibrations occur alternately slower and swifter, nor do they yield any law . . ."

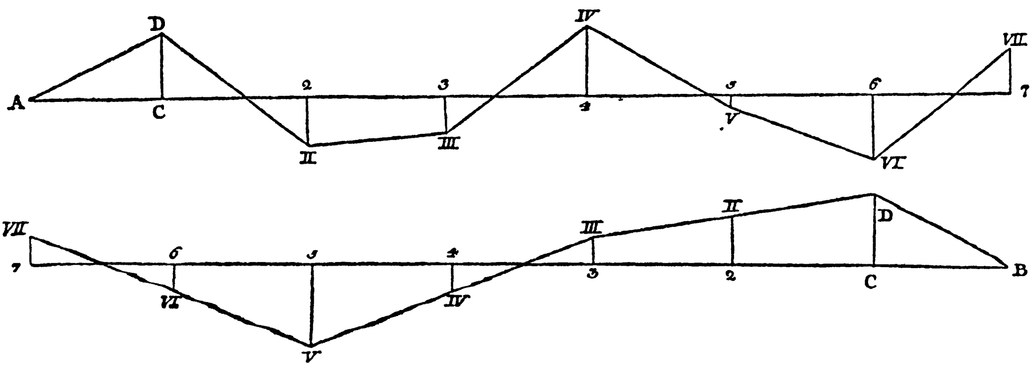

Figure 88. EULER's construction for the motion of a compound string plucked into triangular form (1760)

[Since EULER himself in later years is to forget if not to repudiate this remarkable analysis, we explain precisely what he has done. The primes on the left-hand side of $(317)_2$ denote left derivatives; those on the right, right derivatives. Thus the integration leading from $(317)_2$ to (318) was not justified except at points where Φ and φ are differentiable. Since the initial shape is triangular, EULER's assumed condition $(317)_2$, expressing the continuity of the slope at the junction, *cannot hold* in the initial instant. But EULER uses *the integrated form* (318). This condition, which suffices to yield a unique solution, has been adjusted so as to be satisfied trivially at $t = 0$ and thus permits an arbitrary initial discontinuity. Conversely, it implies $(317)_2$ at places and times such that Φ and φ are differentiable, but this latter requirement is difficult to interpret mechanically since it refers to the constructed functions Φ and φ rather than to the displacement y itself. The use of (318) even when $(317)_2$ does not hold furnishes the first example of a *generalized solution* of a boundary value problem. It is exemplary of EULER's customary approach to problems of divergence or irregularity[1]) and is entirely legitimate.]

To see if "regular" vibrations exist, EULER tries for a special solution 58—60

$$(320) \qquad y = \begin{cases} \alpha \sin \dfrac{\omega}{m} x \cos \omega t & \text{for } 0 \leqq x \leqq a\,, \\[2ex] \beta \sin \dfrac{\omega}{n} x \cos \omega t & \text{for } 0 \leqq x \leqq b\,. \end{cases}$$

The end conditions are satisfied. The condition $(317)_1$ is now equivalent to

$$(321) \qquad \alpha = \frac{C}{\sin \dfrac{\omega a}{n}}\,, \quad \beta = \frac{C}{\sin \dfrac{\omega b}{m}}\,;$$

1) *Cf.* p. XLIII of my Introduction to L. EULERI Opera omnia II 13.

the condition $(317)_2$, to

$$m \tan \frac{\omega a}{n} + n \tan \frac{\omega b}{m} = 0 \; . \tag{322}$$

The infinitely many roots correspond to infinitely many isochronous vibrations at fre-
61—64 quencies asserted to be incommensurable[1]). When $n/m = \frac{1}{2}$, the proper frequencies are given by

$$\tfrac{1}{2}\omega_k \cdot \frac{a}{m} = k\pi \pm \operatorname{Arctan} \sqrt{2}\,, \qquad k = 1, 2, 3, \ldots \tag{323}$$

Nodes are possible. In the second mode, there is one node in the left-hand section, none in the right. In the third mode, there are two on the left and one on the right, *etc.*

65—67 EULER derives the equation for the proper frequencies in a case when m/n is irrational, namely $n = m\sqrt{2}$, and calculates the lowest frequency numerically.

68—69 Returning to the general equation (251), EULER seeks all shapes for which isochronous vibrations are possible[2]). For such a vibration we must have

$$\frac{\partial^2 y}{\partial t^2} = -\,\omega^2 y\,, \quad \omega = \text{const.}\;; \tag{324}$$

hence for the case of zero initial velocity $y = p(x)\cos \omega t$. Therefore from (251) follows

$$c^2 p'' + \omega^2 p = 0 \; . \tag{325}$$

Putting $p = e^{\int q\,dx}$, we get

$$q' + q^2 + \frac{\omega^2}{c^2} = 0 \; . \tag{326}$$

71—80 EULER is unable to solve this equation in general, but he notes that when $c = (\alpha + \beta x)^{\frac{1}{n}+1}$ it becomes a RICCATI equation. Since $c^2 > 0$, known solutions are imaginary for the cases appropriate here. Put $\xi \equiv \alpha + \beta x$ and $C \equiv \omega/\beta$; then the real solution given without derivation[3]) by EULER is

$$p = B\xi^{\frac{r+1}{2r}}\Big[\sin\big(rC\xi^{-\frac{1}{r}} + \theta\big)\sum_{k=0}^{\infty}(-1)^k A_{2k}\xi^{\frac{2k}{r}} + \cos\big(rC\xi^{-\frac{1}{r}} + \theta\big)\sum_{k-1}^{\infty}(-1)^k A_{2k+1}\xi^{\frac{2k-1}{r}}\Big]\,, \tag{327}$$

1) Experiments on the overtones of a string of this type were to be published in 1784 by M. YOUNG, §§ 67—70 of *op. cit. ante*, p. 294.

2) Some of this material appears on pp. 179—182 of Notebook EH 6, written *c.* 1752, immediately after the first appearance of (251) for the non-uniform string; thus, naturally enough, EULER *first* attempted to find simple modes. That he held this material ten years unpublished doubtless reflects his opinion, many times expressed, that this approach is insufficient.

3) These solutions are not included in EULER's papers on the RICCATI equation, *viz* §§ 68—88 of E 269, "*De integratione aequationum differentialium,*" Novi comm. acad. sci. Petrop. 8 (1760/1761), 3—63 (1763) = Opera omnia I 22, 334—394 and E 284, "*De resolutione aequationis* $dy + ayydx = bx^m dx$," Novi. comm. acad. sci. Petrop. 9 (1762/1763), 154—169 (1764) = Opera omnia I 22, 403—420.

where B and θ are the arbitrary constants and where

(328) $$A_0 = 1, A_k = \frac{(r^2-1)(r^2-9)\dots(r^2-[2k-1]^2)}{k!(8Cr)^k} \quad \text{for} \quad k \leqq 1 .$$

[When $c = (\alpha + \beta x)^{\frac{1}{r}+1}$, (325) is easily transformed into BESSEL's equation of order $\frac{1}{2}r$ in the argument $-r\sqrt{C}\xi^{-\frac{1}{r}}$. Thus EULER's result (327) is equivalent to the general formal asymptotic solution of BESSEL's equation for large argument[1]).] The series terminates when r is an odd integer. EULER writes out the entire solution for several such cases.

BERNOULLI's *Memoir on the vibration of strings on non-uniform thickness*[2]) concerns the isochrone oscillations $y = p(x)\cos\omega t$, leading to (325). BERNOULLI tries a solution 3, 4—5
of the type

(329) $$p = \mathfrak{A} q(x) \sin \int_0^x \frac{d\xi}{a[q(\xi)]^2} .$$

For this formula to furnish a solution it is necessary that

(330) $$-\frac{p''}{p} = \frac{1}{a^2 q^4} - \frac{q''}{q} = \frac{\omega^2}{c^2} .$$

Therefore, for a given frequency ω, any function q yields a density $\sigma = T/c^2$. The end 6
condition $y(0, t) = 0$ is satisfied; to satisfy also $y(l, t) = 0$, we have

(331) $$\frac{1}{a}\int_0^l \frac{d\xi}{[q(\xi)]^2} = r\pi , \quad r = 1, 2, 3, \dots ,$$

which determines a. Thus (329) becomes

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1) WATSON, § 7.1 of *op. cit. ante*, p. 159, attributes the formal asymptotic series for J_0 to POISSON (1823) and those for J_n and Y_n to much later authors; in § 4.13 WATSON mentions the two papers of EULER cited in the preceding footnote, but he fails to notice the more general results obtained here.

2) "*Mémoire sur les vibrations des cordes d'une épaisseur inégale,*" Hist. acad. sci. Berlin [21] (1765), 281—306 (1767). A footnote informs us that this paper was completed at the beginning of 1765, before BERNOULLI had seen EULER's papers E 286 and E 287 (described above, p. 271—273, 302—307) or LAGRANGE's second memoir, cited above, p. 273.

Indeed, on 22 December 1764 in a letter to JOHN III BERNOULLI, DANIEL BERNOULLI proposes to EULER the problem of determining the frequencies of a string having the line density (335), and on 7 May 1765 he writes that the memoir has already been sent to Berlin. "In my memoir I have not been able to dispense with some passages that are a little personal, but I should not wish nevertheless that there were the least word that could displease this great man [EULER]. Therefore, my dear Nephew, I ask you before all else to communicate it to Mr. EULER and to ask him to read it and even to examine it . . . I value his rectitude as much as I hate the low ways of Mr. D'ALEMBERT, whom, because of his rare merit and his tastless demerit, I should like to call *an ox half man and a man half ox.*"

$$(332) \qquad p = \mathfrak{A}_r q(x) \sin\left[\frac{\int_0^x \frac{d\xi}{[q(\xi)]^2}}{\int_0^l \frac{d\eta}{[q(\eta)]^2}} r\pi\right].$$

7, 8—9 The number of nodes is $r-1$. For given r, by (330) the product $\omega^2\sigma(x)$ is determined by

$$(333) \qquad \frac{\omega^2}{c^2} = \frac{\omega^2\sigma}{T} = \frac{r^2\pi^2}{\left(\int_0^l \frac{d\xi}{[q(\xi)]^2}\right)^2 q^4} - \frac{q''}{q}.$$

Thus for a given q and r we get for *some* string a mode with $r-1$ nodes, but in general we do not get all the modes of any one string in this way.

10—12 If we take $q(x)$ as a linear function, we may satisfy (333) by (313), and (315) follows. In this special case, we obtain infinitely many modes for one and the same string. [This part of BERNOULLI's paper contains counterparts, obtained by his methods, of results
13 already published by EULER 1).] Superposition of these modes yields the general solution for these strings, writes BERNOULLI, because this has already been proved for the subcase
14—18 of the uniform string. [The double falsity of this inference is obvious.] There follows a detailed study of the forms of the modes.

19—21 From (333) BERNOULLI sees that in order for his method to yield an infinite sequence of modes for the same string it is sufficient that $q''/q = K/q^4$. The density of the string and the frequency of the r^{th} mode are then given by

$$(334) \qquad \sigma = \frac{\sigma_0}{q^4}, \quad \omega^2 = \frac{T}{\sigma_0}\left(\frac{r^2\pi^2}{\left(\int_0^l \frac{d\xi}{[q(\xi)]^2}\right)^2} - K\right).$$

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1) Upon learning of DANIEL BERNOULLI's work (*cf.* the foregoing footnote), on 6 July 1765 EULER writes to JOHN III BERNOULLI, "In begging you to assure your uncle of my very humble respects, I have the honor to tell you that the memoir you read to us last Thursday is very excellent in all respects, and although I too have treated strings of non-uniform thickness [in E 287, above, pp. 302—307], I must agree that your uncle has noticed some interesting cases that escaped me. Nevertheless, I cannot yet persuade myself that the method itself suffices to solve the problem ... of determining the motion of a string *when its initial shape is given.*"

On 25 July 1765 DANIEL BERNOULLI writes to JOHN III BERNOULLI that he has not received gratis any of the Petersburg Memoirs since Volume 6, therefore he has not seen EULER's paper! "I was in fact astonished that a subject of this kind could have escaped the piercing eyes of the greatest geometer of our century." *etc. etc.*

In June 1766 DANIEL BERNOULLI again writes to JOHN III BERNOULLI that his own work on the non-uniform string goes beyond that of EULER and LAGRANGE.

Such a function is $q = \sqrt{1 + x^2/b^2}$. Then

(335) $$\sigma = \frac{\sigma_0}{\left(1 + \frac{x^2}{b^2}\right)^2} ,$$

and the corresponding amplitudes and frequencies are given by

$$y_r = A_r \sqrt{1 + \frac{x^2}{b^2}} \sin\left(\frac{\operatorname{Arc\,tan}\frac{x}{b}}{\operatorname{Arc\,tan}\frac{l}{b}} r\pi\right) ,$$

(336) $$\omega_0 = \frac{\pi}{b \operatorname{Arc\,tan}\frac{l}{b}} \sqrt{\frac{T}{\sigma_0}} ,$$

$$\omega_r = \sqrt{r^2 - g^2}\, \omega_0 , \quad \text{where} \quad g\pi = \operatorname{Arc\,tan}\frac{l}{b} , \ r = 1, 2, 3, \ldots$$

The general solution, obtained by superposition of these modes, is not periodic in general. The paper ends with special cases and much discussion regarding "regular" vibrations. 22—23 ["Regular" vibrations are often mentioned but never clearly defined in the literature of the period; since this concept has not proved useful, there is no point in our following the polemics concerning it.]

While LAGRANGE had treated the non-uniform string only by transformation to the equation of aerial vibrations, so that (312) becomes applicable, EULER soon replaced it by another form suited for direct solution:

(337) $$y = \sum_{k=0}^{N} [C_k(x) f^{(k)}(\int w dx + t) + D_k(x) g^{(k)}(\int w dx - t)] .$$

In his *Researches on the motion of strings of unequal thickness*[1]), EULER asserts that he has found all functions $c(x)$ such that (337) furnishes a solution of (251) with suitably chosen functions C_k, D_k, w and for some constant N, identically in $f, f', \ldots, g, g', \ldots$. While he presents only the cases $N = 0, 1, 2$, his method is general, and we organize it in general terms[2]). As he observes, it suffices to consider the case when $D_k = 0$, since the D_k are determined by the same rule as the C_k. Substitution of (337) into (251) and equating the coefficients of $f^{(k)}$ yields the following recursive system:

(338) $$C''_{k+2} + 2C'_{k+1} w + C_{k+1} w' + \left(w^2 - \frac{1}{c^2}\right) C_k = 0 ,$$

1) E 318. "*Recherches sur le mouvement des cordes inégalement grosses,*" Misc. Taurin. 3 (1762/1765), 25—59 (1766) = Opera omnia II 10, 397—425. Presentation date: 16 February 1765.

2) As did EULER himself in § 12 of E 442, described below, pp. 314—315.

where $C_k \equiv 0$ if $k < 0$ or if $k > N$. Putting $k = N$ yields $w^2 = 1/c^2$. Therefore (338) becomes

$$C_k C''_{k+1} = -\left(\frac{C_k^2}{c}\right)' . \tag{339}$$

Thus we have the alternative forms

$$\begin{aligned} C_{k+1} &= -\int\int \frac{1}{C_k}\left(\frac{C_k^2}{c}\right)' dx \, , \\ \frac{C_k^2}{c} &+ \int_0^x C_k C''_{k+1} dx = \gamma_k^2 \, , \\ C_k &= -\sqrt{c}\,[\tfrac{1}{2}\int_0^x \sqrt{c}\, C''_{k+1} dx + \gamma_k] \, , \end{aligned} \tag{340}$$

where γ_k is a constant of integration. Putting $k = N$ in $(340)_2$ yields

$$C_N^2 = \gamma_N^2 c \; . \tag{341}$$

Substitution of this result into $(340)_3$ then determines C_0 in terms of c and of N arbitrary constants. However, from (338) follows $C_0''' = 0$; hence

$$C_0 = \alpha x + \beta \; , \tag{342}$$

where α and β are arbitrary constants. Comparing these two expressions for C_0 then yields, for each fixed N, *a condition on* c as necessary and sufficient for there to be a solution of the type (337) with $C_N \neq 0$.

E.g., for $N = 0$ comparison of (341) with (342) yields a result equivalent to $c = (\alpha x + \beta)^2$; hence (313) holds, defining the class of strings EULER had studied previously (above, pp. 302—303). For $N = 1$, we have from $(340)_2$

$$\begin{aligned} \gamma_0 &= \frac{C_0^2}{c} + \int C_0 C_1'' dx \, , \\ &= \frac{C_0^2}{c} + C_0 C_1' - \int C_0' C_1' dx \, , \\ &= \frac{(\alpha x + \beta)^2}{c} + (\alpha x + \beta) C_1' - \alpha C_1 \, , \end{aligned} \tag{343}$$

by (342). Putting $C_1 = \gamma_1 \sqrt{c} = (\alpha x + \beta) z$ yields

$$\frac{dx}{(\alpha x + \beta)^2} = \frac{dz}{\gamma_0 - \frac{\gamma_1^2}{z^2}} \; . \tag{344}$$

This is EULER's result, showing that there is a five-parameter family of functions $c = c(x)$, determined by quadrature from (344) and the condition $c = \alpha x + \beta)^2 z^2/\gamma_1^2$, such that (337) with $N = 1$ is a solution of (251).

For $N = 2$, EULER considers only special cases in which various of the functions are proportional to a power of x.

[Thus by 1765 EULER and DANIEL BERNOULLI had contrived inverse methods whereby solutions for a great variety of density functions could be obtained. That neither of these methods has found its way into the literature on vibration can indicate only a lack of modern interest in the mechanical problem[1]).]

44. Polemics, errors, and further special cases (1770—1788). In a work[2]) published in 1770 and already discussed in connection with the uniform string, D'ALEMBERT, after VIII accusing BERNOULLI of obtaining only special solutions by his method of simple modes, brazenly proceeds to use that method himself, [but he makes a valuable addition]. In IX—X (326) set $u = -q$, so that $p'/p = -u$ and

$$\frac{dx}{du} = \frac{1}{\frac{\omega^2}{c^2} + u^2}. \tag{345}$$

If we are to have $p(0) = 0$, then $u(0) = -\infty$ [in the case $p'(0) > 0$; the case $p'(0) < 0$ can be handled similarly]. Assume that $c^2 \geqq \frac{1}{C^2}$, where C^2 is a constant. Define X by the conditions

$$\frac{dX}{du} = \frac{1}{\omega^2 C^2 + u^2}, \quad X(-\infty) = 0, \tag{346}$$

then $\frac{dx}{du} \leqq \frac{dX}{du}$. Hence

$$x(u) \leqq X(u) = -\frac{1}{\omega C} \operatorname{Arc\,cot} \frac{u}{\omega c}. \tag{347}$$

Thus as u goes from $-\infty$ to $+\infty$, $x(u)$ goes from 0 to a certain number A satisfying

1) Apart from presenting the STURM-LIOUVILLE theory, RAYLEIGH in treating the string of non-uniform density gives only crude approximations and some brief remarks on the case of constant but discontinuous σ and the case $\sigma \propto x^{-2}$ (a special case of (335)). *Cf.* §§ 91, 140, 142, 142a, b of *The Theory of Sound*, 2nd ed., Cambridge, 1894.

2) Cited above, p. 274.

Earlier D'ALEMBERT had attacked EULER's work on the subject. In the article "*Cordes Vibrantes*," Ency. Suppl. 2 (1776), after citing "our researches and those of Messrs. DE LA GRANGE, EULER, and DAVID (sic) BERNOULLI", he writes, "A clever geometer having consulted me regarding ... the vibrations of a string of non-uniform thickness, the fallacy of his solution seems to me sufficiently subtle that I should show where it lies." The "fallacy" is EULER's [correct] condition that the slope be continuous at a junction. D'ALEMBERT, even in this article for the common reader, supplies his usual thicket of calculations. He claims also that if EULER's ideas were correct, we should have

$$y = \varphi(t + \int ds/c) + \Psi(t - \int ds/c)$$

for all functional forms of $c(x)$, but of course this is explicitly contradicted by EULER's analysis in E318 (described above, pp. 309—311).

$A \leqq \frac{\pi}{\omega C}$. Hence $\int u\,dx$ falls from the value $+\infty$ at $x=0$ to a certain finite value and rises again to $+\infty$ at $x=A$. Since $p=e^{-\int u\,dx}$, p rises from the value 0 at $x=0$ to a certain maximum and falls again to 0 at $x=A$. D'ALEMBERT concludes that "one may choose ω in such a way that this vanishing occurs at any prescribed point on the x-axis," [but this he does not prove. The foregoing analysis we recognize as the *beginning of the* STURM-LIOUVILLE *theory of proper functions* for strings of arbitrary uniformly bounded density; D'ALEMBERT has gone some way toward proving the existence of the fundamental mode,] but he makes no use of these results, [which indeed rest upon BERNOULLI's approach rather than his own].

In a work published in 1764 EULER had solved in general the problem of the string composed of two different uniform parts joined together and had obtained the proper frequencies and simple modes (above, pp. 303—306). In a paper appearing in 1772, *On the vibration of strings composed of two parts of differing length and thickness*[1]), DANIEL BERNOULLI
2 finally calculates some solutions by his method. This is the first work in which BERNOULLI
3 uses SAUVEUR's acoustical terminology, "fundamental" and "harmonics". For a simple mode proportional to $\sin\frac{\omega}{c}x$, the length of the subtangent at x is $y/y' = \frac{c}{\omega}\tan\frac{\omega}{c}x$. At a junction, the displacement and slope are continuous; equivalently, the displacement
4 and subtangent continuous. If one part of the string is massless, it will remain a straight line of length, say, a and this will be the length of the subtangent at the junction, $x=l$, with the part of line density $\sigma = T/c^2$. Hence

$$a = -\frac{c}{\omega}\tan\frac{\omega l}{c}, \tag{348}$$

13 where the minus sign is appropriate because x is here measured from the fixed end. When both parts of the string have mass, BERNOULLI's formula for the subtangent enables him to write down [EULER's] equation (322) for the proper frequencies. [BERNOULLI ignores EULER's work on the same problem.] The rest of the paper consists in numerical determination of proper frequencies satisfying (348) and (322) in special cases and in discussion
22 of the wave forms. At the end BERNOULLI proposes the problem of the continuous string loaded by a single mass; he remarks that the slope is not continuous at the point where the mass is attached.

5 EULER's paper, *Remarks on* BERNOULLI'*s solution for the motion of a string composed of two parts of differing thickness*[2]), begins by objecting to BERNOULLI's condition that the

1) "*De vibratione chordarum, ex duabus partibus, tam longitudine quam crassitie, ab invicem diversis, compositarum,*" Novi. comm. acad. sci. Petrop. **16** (1771), 257—280 (1772).

2) E 440, "*Animadversiones in solutionen Bernoullianum de motu chordarum ex duabus partibus diversae crassitiei compositarum,*" Novi. comm. acad. sci. Petrop. **17** (1772), 410—421 (1772) = Opera omnia II **11**, 81—97. Presentation date: 2 July 1772.

two parts have the same slope at the junction. [Evidently EULER has forgotten that he himself proposed and used this condition in the general form $(317)_2$ and also in the specific form (322) obtained by BERNOULLI;] also EULER says in the opening paragraph that BERNOULLI's solution "disagrees very markedly from that I gave some time ago for the same problem," [but in fact their solutions for the simple modes and proper frequencies are exactly the same! To explain this inconsistency on EULER's part, we must recall that he has insisted that for the uniform string *corners may occur;* thus, to demand a continuous slope for a discontinuous string would be a retreat: His earlier solution is in fact correct, corners and all, but we may sympathize with his current doubts, since a firm stand was scarcely possible without a better grasp of the concept of integral than anyone could have had in the eighteenth century. It seems impossible, however, to reconcile EULER's statements here with the truth. He suspects that a further mechanical condition is required] and proposes first that the acceleration be continuous, but this condition cannot be satisfied unless the two parts have the same density. Next he proposes, without giving a reason, 7
that the phase of the two oscillations be the same; continuity of the displacement then implies that the amplitudes are equal. In this case, he says, BERNOULLI's results coincide with his own.

EULER repeats his old analysis in a different notation and concludes that the period of 13
a string compounded of two parts of lengths a, b and wave speeds $c = \alpha, \beta$ is

$$T = \frac{2a}{\alpha} + \frac{2b}{\beta}\,, \tag{349}$$

[but this is false[1])].

In the paper, *On the vibratory motion of strings composed of any number of parts having different thicknesses*[2]), EULER applies the same method to a string composed of three sec-

1) EULER asserts that the solution is

$$y = \begin{cases} \varphi\left(t + \dfrac{x}{\alpha}\right) - \psi\left(t - \dfrac{x}{\alpha}\right), & 0 \leqq x \leqq a\,, \\ \varphi\left(t + \dfrac{a}{\alpha} + \dfrac{x'}{\beta}\right) - \psi\left(t - \dfrac{a}{\alpha} - \dfrac{x'}{\beta}\right), & 0 \leqq x' \leqq \beta\,, \end{cases}$$

where $x = a$ and $x' = 0$ is the point of junction, but this solution is not general, since φ, ψ in the second line should be replaced by different functions Φ, Ψ. The end conditions and the condition of continuity of displacement at the junction yield

$$\varphi = \psi,\; \Phi\left(\zeta + \frac{a}{\alpha} + \frac{b}{\beta}\right) = \Psi\left(\zeta - \frac{a}{\alpha} - \frac{b}{\beta}\right),\; \varphi - \psi = \Phi - \Psi\,,$$

but without some further condition (such as continuity of the slope), the problem remains indeterminate, and the motion is not generally periodic.

2) E 441, "*De motu vibratorio chordarum ex partibus quotcunque diversae crassitiei compositarum,*" Novi comm. acad. sci. Petrop. **17** (1772), 422—431 (1773) = Opera omnia II **11**, 90—97. Presentation date: 24 August 1772.

tions having different thicknesses. [The result, like that in the preceding papers, is faulty; it shows only that solutions continuous at the junctions and having period $\frac{2a}{\alpha}+\frac{2b}{\beta}+\frac{2c}{\gamma}$ exist. Such solutions generally fail to have continuous slope at the junctions.] By passing to the limit as the number of segments becomes infinite, EULER derives a formula for the frequency of a string of arbitrarily variable thickness:

$$\nu = \frac{\frac{1}{2}}{\int\limits_0^a \frac{dx}{c}}\,; \tag{350}$$

[of course this result, also, is false.

These last are EULER's two weakest papers in the mechanics of continua.]

EULER's paper, *On the vibratory motion of strings of arbitrarily variable thickness*[1]),
4 attempts to provide a direct proof of (350). After a fruitless attempt at a solution in
5 infinite series, EULER returns to the inverse method based on (337). He derives afresh the solutions corresponding to (313) and verifies that ν_1, as given by (315), is in accord
12—15 with (350). More generally, he sets up the formal series (337) with $N=\infty$, $w=1/c$ and $C_k=D_k$. [He does not justify setting $C_k=D_k$, which is in fact a sufficient condition that the initial velocity be zero,] but he organizes the method, [much as we have presented it above, pp. 309—310]. However, he asserts that there is no loss in generality in taking the two constants of integration in $(340)_1$ in such a way that $C_k(0)=C_k(l)$ for $k\geqq 1$, [but in fact this yields only a special case. For such a solution,] the end conditions reduce to

$$\begin{gathered} f(t)+g(-t)=0\,, \\ f\left(\int\limits_0^l \frac{d\xi}{c(\xi)}+t\right)+g\left(\int\limits_0^l \frac{d\xi}{c(\xi)}-t\right)=0\,. \end{gathered} \tag{351}$$

Hence f and g are periodic of period $2\int\limits_0^l \frac{d\xi}{c(\xi)}$, and this establishes (350).

[The failings of this analysis are obvious. However, a positive result emerges: For any string, there are *special* solutions having the frequency (350). From BERNOULLI's theory of harmonic oscillations it is clear that the general motion of a string of varying thickness is not periodic. EULER now apparently fails to grasp this fact[2]), although he himself had asserted it unequivocally and proved it in an earlier work (above, p. 304).

1) E 442, "*De motu vibratorio chordarum crassitie utcunque variabili praeditarum,*" Novi comm. acad. sci. Petrop. **17** (1772), 432—448 (1773) = Opera omnia II **11**, 98—111. Presentation date: 24 August 1772.

2) There can be no question of EULER's meaning, as he says his "Problema generalissimum" (§ 15) "includes the most general motion of all strings."

This contradiction remains one of the mysteries of the problem of the vibrating string, which seduced all its attackers into a most deplorable prostitution of reason to special pleading for results conjectured from flimsy beliefs.]

Two years later EULER reconsidered the matter, especially in respect to the results in DANIEL BERNOULLI's paper[1]) of 1767. EULER begins his *Enlightenment on the motion of strings of non-uniform thickness*[2]) with the words, "When . . . the extraordinary paper of the most illustrious DANIEL BERNOULLI . . . on the motion of strings of non-uniform thickness was read to me recently, I was wonderfully pleased by the general formula which he gave for the variable thickness of the string in order that a regular motion according to the sine of some angle be possible. Therefore at once there came into my mind the question whether for these cases the motion may be defined in general and the perfect solution obtained." First EULER derives (333) afresh[3]). If there are to be infinitely many possible 7—15
modes corresponding to a single function q, then ω must be made to depend upon r in such a way as to satisfy (333) for all r. [EULER does not treat the general problem clearly from this point on, though later he asserts its solution correctly. We are to determine all functions q such that for some function $\omega = \omega(r)$ we may render $\frac{1}{\omega^2}(k^2\pi^2 - q^3 q'')$ independent of r. Hence $q^3 q'' = \text{const.}$ Hence] 17

$$q = \sqrt{\lambda x^2 + 2\mu x + \nu}\,, \tag{352}$$

where λ, μ, $\nu = \text{const.}$ "In all other cases . . . the thickness . . . will be dependent upon the number r, so that the law of the thickness will continually change for the several simple sounds." [Thus EULER delimits the cases to which BERNOULLI's method[4]) could be applied.] For other laws of thickness than that resulting from (352), there is indeed one simple mode, but this method fails to reveal anything regarding the other possible motions.

For q as given by (352), we find that 14

$$c = \sqrt{\frac{T}{\sigma}} = A\beta(\lambda x^2 + 2\mu x + \nu)\,, \quad \omega_r = \beta\sqrt{r^2\pi^2 - A^2(\mu^2 - \lambda\nu)}\,, \tag{353}$$

where $\beta = \text{const.}$ The ratio of frequencies is irrational in general. However, in order that the ratio of frequencies be 1, 2, 3, 4, . . ., it is necessary and sufficient that $\mu^2 = \lambda\nu$, whence follows a result equivalent to (313).

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1) Cited above, p. 307.

2) E 567, "*Dilucidationes de motu chordarum inaequaliter crassarum,*" Acta acad. sci. Petrop. 1780: II, 99—132 (1784) = Opera omnia II 11, 280—306. Presentation date: 1 December 1774. §§ 6—13 and 19—39 consist in examples and repetitions of EULER's earlier results.

3) This is not so repetitious as might appear, since EULER begins from the partial differential equation, while BERNOULLI had used the old, directly postulated, theory of simple modes.

4) *I. e.*, BERNOULLI's attempt to find modes that are of the type (329).

The unsatisfactory state of the theory of the non-uniform string is summarized in a paper published in 1775 by BERNOULLI, where, referring first to the uniform string, he writes[1]), "... I did not wish to further the controversy as to where lay the heart of the riddle, but I preferred to take refuge in the arguments called *ad hominem*. Therefore, [in 1753] I proposed the problem of the vibrations of a non-uniformly thick string ... so as to see which of our methods would find the more fertile field, but in fact, each using his own method, we came up against the same barriers, except that my method, synthetic rather than purely analytical, yielded some very special corollaries that escaped the methods considered most fertile by the others."

[Indeed, as BERNOULLI observed, the problem of the non-uniform string revealed the impotence of all methods proposed in the eighteenth century. EULER's insistence on using the general solution in arbitrary functions, which had brought him complete though unacknowledged success with the uniform string, for the non-uniform string left him powerless except in special cases. BERNOULLI had some glimmering of the generality of his method of proper functions.] In 1775 he wrote to N. FUSS that his method "can be applied to any finite number of bodies, even when in the system there is no ... period ..." [His retreat to the finite, combined with his baseless insistence on *sinusoidal* modes, cost him all but the simplest fruits of his theory, which he lacked the analytical and mechanical concepts to substantiate even on heuristic grounds. It is most strange that the non-sinusoidal modes he himself had long ago discovered for other systems such as rods and heavy ropes did not suggest the nature of the right answer to him. Only D'ALEMBERT made any step toward the general theory of proper functions, but, since they did not fall in with his preconceived ideas, he abandoned the analysis.]

IV B. Plane vibrations of a heavy cord hung from one end

45. Plane vibrations of a heavy cord hung from one end. The partial differential equation (157 F) for small transverse oscillation of a heavy continuous vertical cord, sometimes called a hanging chain, for the case of uniform density had been published in 1743 by D'ALEMBERT; for arbitrary line density σ, it appears in a work of LAGRANGE of 1762[2]):

$$\frac{1}{g}\,\frac{\partial^2 y}{\partial t^2}=\frac{\partial y}{\partial x}+\frac{1}{\sigma}\int_0^x \sigma\,dx\cdot\frac{\partial^2 y}{\partial x^2}\,. \tag{354}$$

Beyond stating the analogy to the problem of the vibrating string and of the oscillations of air in a tube in the special case when $\sigma \propto x^n$, LAGRANGE does not develop any conse-

1) § 1 of *op. cit. ante*, p. 291.

2) § 34 of *op. cit. ante* p. 273. For the case $\sigma=$ const. it is obtained on p. 81 of EULER's notebook EH 8, written in 1759—1760; EULER uses the method of moments, taking into account the moment of the inertial force.

quences of importance. He remarks on terminating series of the type (312) and proposes the condition $\partial^2 y/\partial x^2 = 0$ at the free end.

[We have remarked upon EULER's program of reconsidering all the mechanical problems treated prior to his "first principles of mechanics" (above, pp. 253—254).] In 1774 he turned his attention to our present problem in the paper, *On the very small oscillations of a freely suspended rope*[1]), [in which the modern, systematic theory first appears]. First 1—3
there is a derivation of (354) from the balance of forces acting on an element of the cord[2])
in the case when $\sigma = \text{const.}$:

$$g\frac{\partial}{\partial x}\left(x\frac{\partial y}{\partial x}\right) = \frac{\partial^2 y}{\partial t^2}\,. \tag{355}$$

For want of the general solution, EULER is driven to apply DANIEL BERNOULLI's method of 4—5
simple modes. The equation to be satisfied is $(108)_2$ with $\alpha = g/\omega^2$; [BERNOULLI's old 6—12
results (99) and (100),] viewed from the present standpoint, yield the solution

$$y = \Sigma\, \mathfrak{A}_r J_0\left(2\,\omega_r \sqrt{\frac{x}{g}}\right)\sin\,(\omega_r t + \zeta_r)\,,\quad J_0\left(2\,\omega_r\sqrt{\frac{l}{g}}\right) = 0\,. \tag{356}$$

But the equation $(108)_2$ for the proper functions is of second order, so the simple modes 16
must contain a second arbitrary constant in addition to $\mathfrak{A}_r$. To determine the second solu- 17—18
tion, EULER has recourse to [BERNOULLI's old] theorem on the subtangent at the free end
(above, p. 158); thence follows[3]) $-\dfrac{1}{y}\dfrac{dy}{dx} = \dfrac{g}{\omega^2}$. The general solution of $g\,(xy')' = -\,\omega^2 y$ is 20—23

$$y = \pi(u)\left[D + C\int\frac{du}{[\pi(u)]^2}\right], \tag{357}$$

---

1) E 576, "*De oscillationibus minimis funis libere suspensi,*" Acta acad. sci. Petrop. 1781: I, 157—177 (1784) = Opera omnia II 11, 307—323. Presentation date: 31 October 1774.

2) EULER's mastery of the method of balancing the momentum of a continuous line dates from 1750. The idea of the derivation he gives here is more easily grasped if we first replace (227) and (39) by the more general equations

$$\frac{\partial}{\partial s}\left(T\frac{\partial x}{\partial s}\right) = -F_x + \sigma\frac{\partial^2 x}{\partial t^2}\,,\qquad \frac{\partial}{\partial s}\left(T\frac{\partial y}{\partial s}\right) = -F_y + \sigma\frac{\partial^2 y}{\partial t^2}\,.$$

For the present problem, assume $x \approx s$, $F_x = -\,\sigma g$, $F_y = 0$. The first of the above equations yields $T = g\int_0^x \sigma\,dx + T_0$, where $T_0 = 0$ when the end of the cord is not weighted. The second equation then becomes

$$\sigma\frac{\partial^2 y}{\partial t^2} = g\frac{\partial}{\partial x}\left[\left(\int_0^x \sigma\,dx\right)\frac{\partial y}{\partial x}\right],$$

which is equivalent to (354).

3) This is not very convincing. It is better to put $y \propto \cos\omega t$ in (354); putting $x = 0$ then yields the desired result, with the proviso that $\sigma(0) \neq 0$ and $\left.\dfrac{\partial^2 y}{\partial x^2}\right|_{x=0} \neq \infty$.

where $\pi(u) = J_0(2\sqrt{u})$ and $u = \omega^2 x/g$. From the series expansion for J_0 it appears that (357) does not satisfy the end condition, which assumes the form $\pi(0) = -\pi'(0)$, unless $C = 0$.

24—25 The problem is now to solve (100), [and to this end EULER introduces a new method in the theory of transcendental equations]. Setting $n = 2\sqrt{u}$, he considers the identity

(358) $$J_0(n) = \sum_{p=0}^{\infty} (-1)^p \frac{n^p}{(p!)^2} = \prod_{q=1}^{\infty} (1 - \alpha_q n) ,$$

so that the roots, in increasing order, are $\frac{1}{\alpha_q}$; then he sets

(359) $$A_r \equiv \sum_{q=1}^{\infty} (\alpha_q)^r$$

and by equating coefficients of like powers of n in (358) calculates

(360) $$A_1 = 1,\ A_2 = \frac{1}{2},\quad A_3 = \frac{1}{3},\quad A_4 = \frac{11}{48},\quad A_5 = \frac{19}{120},\quad A_6 = \frac{473}{4320} .$$

26 Since $\alpha_1^q < A_q$, we have

(361) $$n_1 \equiv \frac{1}{\alpha_1} > (A_q)^{\frac{1}{q}} .$$

Taking $q = 6$ yields $n_1 < 1{,}445785$, and extrapolating by the rapidly decreasing differences suggests

(362) $$n_1 = 1{,}445795 .$$

28 Since

(363) $$\alpha_1 A_q - A_{q+1} = (\alpha_1 - \alpha_2)\alpha_2^q + (\alpha_1 - \alpha_3)\,\alpha_3^q + \cdots ,$$

and since $\alpha_1 > \alpha_2$, we have $\alpha_1 A_q - A_{q+1} > 0$, or

(364) $$n_1 < \frac{A_q}{A_{q+1}} .$$

Taking $q = 5$ yields

(365) $$n_1 < 1{,}446089 .$$

29 Substitution of (362) into (359) enables one to calculate the sums $\sum_{q=1}^{\infty} (\alpha_q)^r$, and to these

30—31 the same process is applied to calculate α_2. The convergence is much worse; EULER concludes with some hesitation that[1])

(366) $$n_2 = 7{,}6658,\ n_3 = 18{,}63 .$$

1) More accurate values are $n_1 = 1{,}4457965$, $n_2 = 7{,}6178156$, $n_3 = 18{,}7217517$. *Cf.* the approximations (101) and $(104)_1$ obtained by DANIEL BERNOULLI.

EULER's paper, *On the disturbing effect of their own weight on the motion of strings*[1]), 2—3
begins by considering the differential equation for small transverse oscillation of a taut heavy horizontal cord :

$$\frac{1}{c^2}\frac{\partial^2 y}{\partial t^2} = \frac{g}{c^2} + \frac{\partial^2 y}{\partial x^2} \; . \tag{367}$$

[When $c = \text{const.}$], the general solution for the string with fixed ends $x = 0$ and $x = l$ 4—5
is easily shown to be

$$y = -\frac{\frac{1}{2} g x (x - l)}{c^2} + \Phi(ct + x) + \Psi(ct - x) \, . \tag{368}$$

Thus the results are the same as for the weightless string, except that the oscillation takes 6—7
place about the [parabolic] figure of equilibrium $y = -\dfrac{\frac{1}{2} g x (x - l)}{c^2}$.

When the string is vertical, (355) is to be replaced by[2]) 8—9

$$\frac{\partial^2 y}{\partial x^2} = g \frac{\partial}{\partial x}\left[\left(x + \frac{T_0}{\sigma g}\right)\frac{\partial y}{\partial x}\right] \tag{369}$$

(*cf.* (148)). Taking $x + \dfrac{T_0}{\sigma g}$ in place of x as an independent variable reduces (369) to (355), 10—14
but now, putting $u = \dfrac{\omega^2}{g}\left(x + \dfrac{T_0}{\sigma g}\right)$, we require solutions $y(u)$ vanishing at $u = u_1 \equiv \dfrac{\omega^2}{g^2} \cdot \dfrac{T_0}{\sigma}$ and at $u = u_2 \equiv \dfrac{\omega^2}{g}\left(l + \dfrac{T_0}{\sigma g}\right)$. Thus in the simple modes we can no longer take $y = J_0(2\sqrt{u})$; the second solution is required as well. [Abandoning (357),] EULER follows 15
a suggestion he had made in his *Integral Calculus*[3]) : He puts $y = p + q \log u$ and finds that in order for the coefficient of $\log u$ to vanish, q must be a solution of the original equation, so that we may take $q = J_0(2\sqrt{u})$. The coefficients in the power series for p 16—18
are then determined uniquely to within two arbitrary constants ; the solution which EULER gives in series form we should now write as

$$y = A J_0(2\sqrt{u}) + B Y_0(2\sqrt{u}) \; . \tag{370}$$

To satisfy the end conditions we must therefore determine the arbitrary constants A 19
and B in such a way that

$$\begin{aligned} A J_0(2\sqrt{u_1}) + B Y_0(2\sqrt{u_1}) &= 0 \; , \\ A J_0(2\sqrt{u_2}) + B Y_0(2\sqrt{u_2}) &= 0 \; . \end{aligned} \tag{371}$$

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1) E 577, "*De perturbatione motus chordarum ab earum pondere oriunda,*" Acta acad. sci. Petrop. 1781: I, 178—190 (1784) = Opera omnia II 11, 324—334. Presentation date: 7 November 1774.

2) See the derivation in footnote 2, p. 317.

3) E 366, *Institutiones calculi integralis* 2, Petropoli, 1769 = Opera omnia I 12, 1—413. In § 977 the above solution had been obtained somewhat less explicitly.

EULER says we are to solve the first of these for A, substitute into the second, then divide out by B. *I. e.*, he describes the characteristic equation

$$J_0(\zeta)\,Y_0(C\zeta) - J_0(C\zeta)\,Y_0(\zeta) = 0 \ , \tag{372}$$

where $\zeta \equiv 2\,\dfrac{\omega}{g}\sqrt{\dfrac{T_0}{\sigma}}$ and $C \equiv 1 + \dfrac{l\sigma g}{T_0}$, an arbitrary constant not less than 1. He says there is no doubt that there are an infinite number of real frequencies ω satisfying this equation, [but he makes no attempt to calculate them.

Thus the theory of the hanging heavy cord, which at the beginning of the century had led to the first ideas of proper frequencies and simple modes, and which in 1743 had led to the first partial differential equation of mathematical physics, by the end of the century had brought no further light to the principles and methods of mechanics. Rather, it performed the minor but not uninteresting service of bringing into existence "BESSEL functions" of all orders, of both kinds, and of real or imaginary argument, and of revealing some of their properties[1]).]

IV C. Plane vibrations of straight or curved rods

46. EULER's faulty theories of the vibrations of curved rods (1760, 1774). EULER's *Essay on the sound of bells*[2]) is essentially a revision of his earliest work on the subject, E 831
6—7 (above, § 20). To consider small flexural vibrations of a circular ring, EULER takes x as arc-length along the middle line and assumes that there is a small displacement y along the radius, [and no other. For an inextensible ring, we know now that this is not possible, since there must be a tangential displacement w such that

$$a\frac{\partial w}{\partial x} = -\,y\,. \tag{373}$$

7—10 EULER neglects this displacement w when he calculates the reaction of inertia and the change of curvature.] Thus he obtains the partial differential equation

$$\frac{1}{c^2 f^2}\frac{\partial^2 y}{\partial t^2} + \frac{1}{a^2}\frac{\partial^2 y}{\partial x^2} + \frac{\partial^4 y}{\partial x^4} = 0\,, \tag{374}$$

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1) Since WATSON does not mention a number of the major papers on mechanical subjects where "BESSEL functions" are first introduced and studied, his history (*op. cit. ante*, p. 159) fails to give a just idea of how much of the early theory of these functions is due to EULER.

2) E 303, "*Tentamen de sono campanarum*," Novi comm. acad. sci. Petrop. **10** (1764), 261—281 (1766) = Opera omnia II **10**, 360—376. Presentation dates: 25 September 1760, 17 May 1762. Most of the results we describe here are given on p. 84 of Notebook EH 8. On p. 184 of Notebook EH 7 it is stated that this paper and E 302 were sent to the Petersburg Academy on 26 April 1762. *V.* below, footnote 3, p. 321.

where $f^2 = E/\varrho$, [instead of the correct equation, which is of sixth order[1]). The paper is nevertheless an extraordinary performance, both for its otherwise correct argument in deriving (374) and] for its determination of the simple modes for the complete ring by the condition that they shall be periodic of period $2\pi a$ in x. The proper frequencies given by 11—17
EULER are

$$(375) \qquad \nu_{r-1} = \frac{fc}{2\pi a^2}\, r\sqrt{r^2-1}\,, \quad r = 2, 3, 4, \ldots$$

[and of course are not correct]; he shows that [under his assumptions] there are $2r$ nodes 18—19
in the $r - 1^{\text{st}}$ mode.

In applying these results to bells EULER is not able to go beyond the remarks in E 831. 20
He suggests that the rings should be regarded as cut out by sections normal to the bell 21—23
rather than normal to the axis, but the same partial differential equation results. He doubts 24
whether this theory is sound, "since the trembling may be regarded as obeying a very different rule from that we have supposed here. What is needed is a method for determining the trembling motion of a body of arbitrary form. The methods used so far are restricted to certain kinds of bodies, such as strings or very thin sheets, and therefore it would not be right to attribute to the results in this memoir a greater validity than belongs to the hypothesis expressly set down."

A second, and different, theory of the vibrations of curved rods is proposed in a paper[2]) published by EULER in 1782. There he asserts that all results derived for straight rods remain valid for curved rods if x is interpreted as arc-length and y as the normal displacement. [That this contradicts his own earlier contention that (374) rather than (273) governs the transverse oscillation of a circular rod[3]) matters less than that it is in principle

1) *Cf.*, *e. g.*, § 293 of A. E. H. LOVE, *A treatise on the mathematical theory of elasticity*, 4th ed., Cambridge, 1927.

In §§ 290 and 294 of *op. cit. ante*, p. 11, TODHUNTER writes that SOPHIE GERMAIN in her celebrated prize essay published in 1821 asserted that there was an error in sign in EULER's equation (374), but TODHUNTER finds that the difference arises from one of Mlle GERMAIN's numerous slips in calculation. Judging by TODHUNTER's far from gallant account of the lady's researches on vibrations of elastic rings, her work if corrected in detail would coincide almost completely with EULER's.

2) §§ 52—55 of E 526, completed by 1774, cited below, p. 326.

§§ XXIV—XXV of LAMBERT's paper of 1777, cited below on p. 325, contain inconclusive remarks about the vibrations of elastic rings, bells, and tubes.

3) That EULER was aware of this contradiction is shown by the paper of his student M. GOLOVIN, "*Applicatio tentaminis de sono campanarum auctore L. EULERO novor. commentor. tomo X inserti ad sonos scyphorum vitreorum, qui sub nomine instrumenti harmonici sunt cogniti*," Acta acad. sci. Petrop. 1781_2, 176—184 (1784). In § 3 GOLOVIN claims that the term $\frac{1}{a^2}\frac{\partial^2 y}{\partial x^2}$ in (374) arises from "elementary forces", which he is supposing absent. What this means is not clear. GOLOVIN then derives afresh the [false] results in §§ 53—55 of E 526 and applies them to explain the sounds of the glass harmonica.

false : The initial curvature of a rod cannot be disregarded in calculating its vibratory motion. Perhaps EULER was misled by the one-dimensional theory of flow of a fluid in a
53 tube, where the curvature of the directrix has no effect[1]).] For example, for a rod in the form of a circle or any other closed curve, the solution must be periodic, and this leads at once to the same sequence of sounds as for a rod pinned at both ends, *i. e.*, overtones in the ratios 4, 9, 16, . . ., "which quickly become too high to be heard, so that besides the fundamental only the double octave will be perceived, by which a most pleasing harmony will be experienced. Therefore such elastic rings . . . enjoy this remarkable property of giving out much purer sounds, and it seems the same should occur also for whole discs and bell-shaped bowls, the sounds of which are considered to affect the hearing with an extraordinary sweetness." [While this is false in detail, it is true in principle, since an argument of this kind, when applied to the correct governing partial differential equation, serves to exclude for the full circle many of the modes possible for a circular segment.]

In a paper called *Thoughts on the formula by which is expressed the motion of elastic bands curved into circular rings*[2]), A. J. LEXELL attempts to establish the equations of motion for a rod by specializing and approximating EULER's general exact equations (572),
4 to be discussed below. For a straight rod LEXELL's equation has the coefficient $\frac{3}{2}$ instead of $\frac{1}{2}$
5 for the non-linear term in (575). For a circular rod, LEXELL obtains an equation having
6 $\frac{2}{a^2}\frac{\partial^2 y}{\partial x^2}$ in place of the second term in (374), and he gives arguments to indicate that his
7—8 result, rather than EULER's, is correct. Also, LEXELL explains that terms of this kind arise as the result of the tension in the rod and cannot in general be neglected, despite EULER's
9—10 claim to the contrary. There follows a derivation of a corresponding equation for a rod having the form of an arbitrary plane curve. [These results, like EULER's, are wrong from failure to realize that for a rod which is not straight it is necessary to impose the condition of inextensibility or some definite elastic law of extension. However, they have a certain importance in that while based on equally plausible reasoning, they differ from EULER's results and thus indicate need for a more systematic linearization of the equations of finite motion. A correct theory of the vibrations of circular rods was not to be obtained until nearly a century after the end of our period of study.]

47. EULER's definitive work on the six kinds of transverse vibrations of straight rods (1772—1774), and its experimental verification by JORDAN RICCATI (1782) and by CHLADNI (1787). In § XLVI of his first paper on the vibrating string, D'ALEMBERT had written

1) *Cf.* Introduction to L. EULERI Opera omnia II **13**, p. XVI.

2) "*Meditationes de formula qua motu laminarum elasticarum in annulos circulares incurvatarum exprimitur,*" Acta acad. sci. Petrop 1781_2, 185—218 (1784).

that "if . . . the string makes longitudinal vibrations," the same partial differential equation holds, *viz* (251), but he had not stated what c is or what elastic hypothesis he used. [The subsequent history of this interpretation of (251) belongs to the theory of aerial propagation of sound[1]).] In § XLVII, D'ALEMBERT had shown himself familiar with (125), not raising any objection against it. However, in a work of 1761[2]), he claims that the equation governing the vibrations of a spring should be

$$\frac{\partial^2 y}{\partial t^2} + \frac{\partial^2 y}{\partial x^2} = 0 . \tag{376}$$

He remarks that DANIEL BERNOULLI had derived (125), citing a publication[3]) of 1751; he criticizes BERNOULLI's assumptions; and he leaves the reader with the implication that BERNOULLI is wrong. [BERNOULLI's result (125) is correct and D'ALEMBERT's proposal (376) is false[4]).

This paragraph leaves the reader with a low estimate of D'ALEMBERT's grasp of mechanical principles.]

In 1771 appeared DANIEL BERNOULLI's *Physico-mechanical investigation of the mixed motion caused by striking an elastic band*[5]). In attempting to solve the difficult problem stated in the title, BERNOULLI has to take refuge in approximation of the form of the vibrating rod by a parabola.

EULER's paper *On the vibratory motion of elastic bands, where are developed several new kinds of vibration not hitherto considered*[6]), begins by applying the balance of moments in 1—9
the form (91) to the case when the only force acting is the inertial force $F_y = -\sigma \frac{\partial^2 y}{\partial t^2}$. Hence for small motions (91) becomes

$$-\int_0^x x \int_0^x \sigma \frac{d^2 y}{\partial t^2}\, dx = \mathcal{B} \frac{\partial^2 y}{\partial x^2} . \tag{377}$$

1) Part II of Introduction to L. EULERI *Opera omnia* II **13**.

2) § IV of *op. cit. ante*, p. 274.

3) The result, as we have seen, was obtained by BERNOULLI in 1734 and was published by EULER in 1735. D'ALEMBERT was not given to just citation except for deceased authors.

4) That (376) is not a misprint for the equation of longitudinal vibration is shown by D'ALEMBERT's proceding to solve it by complex functions, known to him from his hydrodynamical researches.

5) "*Examen physico-mechanicum de motu mixto qui laminis elasticis a percussione simul imprimitur*," Novi comm. acad. sci. Petrop. **15** (1770), 361—380 (1771).

6) E 443, "*De motu vibratorio laminarum elasticarum ubi plures novae vibrationum species hactenus non pertractatae evolvuntur*," Novi. comm. acad. sci. Petrop. **17** (1772), 449—487 (1773) = Opera omnia II **11**, 112—141. Presentation date: 21 September 1772.

Between 1743 and 1772 EULER seems to have given but slight attention to the subject. On p. 80 of Notebook EH 8 (1759—1760), immediately after deriving (273), EULER obtains a few special solutions, and on p. 83 he reviews the analysis of free-free modes.

[This will be recognized as the extension of (130) to general motion, yielding as corollaries not only the differential equation (273), with $c^4 = \mathcal{B}/\sigma$, a result EULER had had in his notebooks for twenty years, but also the end conditions (132) for a free end. Thus EULER has synthesized his old treatment in E 40 with his general principles of mechanics. Here also is the first occurrence of a partial integro-differential equation as a statement of a physical problem.]

EULER writes, "I am driven to say that so far I have in no way been able to find the
11—12 complete integral" of (273), which should contain four arbitrary functions; while he gives such a solution in series form

$$y = \sum_{n=0}^{\infty} (-1)^n \left[\frac{P^{(2n)}(t)}{(4n)!} \left(\frac{x}{c}\right)^{4n} + \frac{Q^{(2n)}(t)}{(4n+1)!} \left(\frac{x}{c}\right)^{4n+1} + \frac{R^{(2n)}(t)}{(4n+2)!} \left(\frac{x}{c}\right)^{4n+2} + \frac{S^{(2n)}(t)}{(4n+3)!} \left(\frac{x}{c}\right)^{4n+3} \right], \tag{378}$$

he considers it of no use. He decides to give a "clearer and more definite" explanation of the properties of the special solution

$$y = \cos \omega t \left(A \cos \frac{\sqrt{\omega}\, x}{c} + B \sin \frac{\sqrt{\omega}\, x}{c} + C e^{\frac{\sqrt{\omega}\, x}{c}} + D e^{-\frac{\sqrt{\omega}\, x}{c}} \right) \tag{379}$$

13—27 (*cf.* (147)). Most of the remainder of the paper presents in the way now grown customary the material derived more crudely in the second part of E 65 (above, pp. 219—222). To the four kinds of vibration considered there, EULER adds[1])

	The end $x = 0$ is	The end $x = l$ is
Case V (§§ 24—25)	free	pinned
Case VI (§ 27)	pinned	clamped,

exhausting all combinations of the end conditions considered. The corresponding frequency equations are

$$\begin{aligned} &\text{Case V:} && \tan \zeta = \tanh \zeta \\ &\text{Case VI:} && \text{the same.} \end{aligned} \tag{380}$$

He now handles the solution of all the frequency equations (134), (199), and (380) with masterful ease, giving exact transformations, bounds, and the following numerical values[2]):

1) Case I is treated in §§ 13—22; Case II is EULER's Problem 1; Case III, Problem 3; Case IV, Problem 5; Case V, Problem 2; Case VI, Problem 4.

2) EULER explains his numerical procedure as yielding $\frac{5}{2}\pi$, $\frac{7}{2}\pi$, $\frac{9}{2}\pi$, ... for the higher modes of clamped-free vibration; in the reprint on p. 123 of the Opera omnia II 11_1, Professor TROST has replaced EULER's values by correct decimals for the above multiples of $\frac{1}{2}\pi$. However, the numerical values printed by EULER must derive from some more accurate but undescribed numerical procedure, since (except for the last figure, where the final 1 should be 7), they are closer to the correct solutions of the transcendental frequency equation. *Cf.* the more accurate results of LAMBERT and RICCATI, given on pp. 326 and 328.

Value of ζ

Mode	Clamped-Free	Free-Free Clamped-Clamped	Pinned-Free Pinned-Clamped	Pinned- Pinned
1	1,87514	4,73007	3,92660	π
2	4,69408	$\frac{5}{2}\pi - 0{,}01765$	7,06858	2π
3	7,85473	$\frac{7}{2}\pi + 0{,}01765$	10,21017	3π
4	10,99553	$\frac{9}{2}\pi - 0{,}01765$	13,35176	4π
5	14,13711	$\frac{11}{2}\pi + 0{,}01765$	$\frac{21}{4}\pi$	5π

To conclude the paper (Problem 6), EULER considers a band pinned at an interior point C and free at each end. The slope at C is continuous, and thus the frequency of each side must be the same, though the amplitudes may be different. EULER demands also that the curvature be continuous at C. There thus result eight homogeneous equations for the eight amplitudes. EULER seems unable to derive the general form of the characteristic equation. Restricting attention to the case when C is the midpoint, he shows that there are then two sequences of proper frequencies, one of which is given by the result of Case I, the other by the result of Case V. That is, he shows that a band pinned at its midpoint has the same proper frequencies as would either half, either pinned or clamped at C and vibrating independently of the other.

[In respect to mechanics, this paper's main interest lies in its calculation of the force and moment exerted by the band on the support.] Considering only Case I, from (377) we have at once

$$(381) \qquad F(l) = \mathcal{B} \left.\frac{\partial^3 y}{\partial x^3}\right|_{x=l}, \quad M(l) = \mathcal{B} \left.\frac{\partial^2 y}{\partial x^2}\right|_{x=l}.$$

From the solution (379), with the constants A, B, C, D evaluated in terms of the root ζ corresponding to the mode being considered, EULER obtains

$$(382) \qquad \begin{aligned} F(l) &= \mathcal{B} \frac{\zeta^3}{l^3} \cos\left(\frac{\zeta^2 c^2}{l^2} t\right) \sin\zeta \sinh\zeta\,, \\ M(l) &= \mathcal{B} \frac{\zeta^2}{l^2} \cos\left(\frac{\zeta^2 c^2}{l^2} t\right) (\sin\zeta \cosh\zeta - \cos\zeta \sinh\zeta)\,. \end{aligned}$$

[In fact the right-hand sides should be multiplied by a length A specifying the amplitude of vibration; EULER has set $A = 1$.] The force and moment which the wall exerts upon the band are the negatives of these.

LAMBERT's posthumous paper, *On the sound of elastic bodies*[1]), [is scarcely more than

1) "*Sur le son des corps élastiques*" (January 1777), Acta Helv. 9 = Nova acta Helv. 1, 42—75 (1787).

XXXVIII an exposition[1]) of some parts EULER's work of 1742.] He obtains the following numerical values of the frequency parameter ζ for the clamped-free modes[2]) :

Mode	Value
1	1,8751048
2	4,69409108
3	7,8547575
4	10,99554073
5	14,13716839
6	17,27875952
7	20,42035225
8	23,56194490
9	26,70353755

XLVI LAMBERT describes a curious experiment for confirming these values[3]).

EULER's *Investigation of the trembling of elastic bands and rods*[4]) begins, "Although this subject was treated at length some time ago both by the most illustrious DANIEL BERNOULLI and by me, nevertheless since at that time neither were the principles by which such motions are to be determined sufficiently refined, nor was that part of analysis concerning functions of two variables sufficiently explored, it does not seem amiss if I now investigate this same subject more closely."

[This paper is EULER's definitive treatise on the small vibrations of what he now calls a rod.] While the exposition is more certain and the results are more complete, the contents
3 are almost entirely a repetition of E 443. The basic principle is the integro-differential equation (377), generalized to allow for a tension T and a normal force F applied at the end $x = 0$:

$$(383)\qquad -Ty + Fx + \int_0^x dx \int_0^x dx \cdot \sigma \frac{\partial^2 y}{\partial t^2} = -\mathcal{B} \frac{\partial^2 y}{\partial x^2} .$$

4 Hence

1) *Cf.* LAMBERT's explanation in § XLIII. The simple theory given by LAMBERT in §§ X—XXIII I do not understand at all, but he begins over again at § XXXI.

2) The six-place table for the first six modes given by RAYLEIGH in § 174 of his ***Theory of Sound*** agrees with LAMBERT's, as does the briefer table given by R. E. D. BISHOP & D. C. JOHNSON, "*Vibration Analysis Tables,*" Cambridge, 1956.

3) This experiment involves holding a vice in ones teeth so as to sense a sound which is not audible. LAMBERT goes on to project an instrument based on this idea and called "Musique solitaire" (§ L) whereby a person may enjoy music through his teeth without awakening sleepers.

4) E 526, "*Investigatio motuum quibus laminae et virgae elasticae contremiscunt,*" Acta acad. sci. Petrop. 1779: I, 103—161 (1782) = Opera omnia II 11, 223—268. Presentation date: 28 November 1774.

$$(384)\qquad \begin{aligned} -T\frac{\partial y}{\partial x} - F + \int_0^x dx\cdot\sigma\frac{\partial^2 y}{\partial t^2} &= -\mathcal{B}\frac{\partial^3 y}{\partial x^3}\,, \\ -T\frac{\partial^2 y}{\partial x^2} + \sigma\frac{\partial^2 y}{\partial t^2} &= -\mathcal{B}\frac{\partial^4 y}{\partial x^4} \end{aligned}$$

(*cf.* $(573)_2$). EULER remarks upon the effect of compression or extension but does not attempt 6
to include them in the theory; he remarks also that (384) includes both the perfectly flexible 7
case, $\mathcal{B} = 0$, leading to (251), and that of the "proper sound of an elastic rod", $T = 0$, leading to (273), while in general there is "a case mixed from both", but he considers only the purely flexural vibrations. [In fact (383) wants a term $-L$ to allow for a couple.] Putting $x = 0$ in (383) and $(384)_1$ then yields

$$(385)\qquad \left[L = \mathcal{B}\frac{\partial^2 y}{\partial x^2}\bigg|_{x=0} \text{ and}\right] \quad F = \mathcal{B}\frac{\partial^3 y}{\partial x^3}\bigg|_{x=0}\,.$$

At a pinned end we have the conditions $[L = 0$, and hence$]$ $\dfrac{\partial^2 y}{\partial x^2} = 0$, which EULER thus derives from his [incomplete] equation (383); at a free end, the additional condition $F = 0$; at a clamped end, the kinematical condition $\partial y/\partial x = 0$; the definition of pinned and clamped ends is completed by the kinematical condition $y = 0$. Most of the rest of the paper consists in a systematic exploration of the simple modes satisfying the six sets of end conditions previously considered[1]) in E 443. The terms "fundamental sound" and "node" EULER now uses for the first time. He gives less attention to the values of the frequencies but draws a few figures from calculated points. While he mentions the nodes, he does not determine their locations accurately.

EULER studies again the simple modes of a rod pinned at one point L as well as at 41
both ends. This time, with a better choice of notations and a more systematic elimination, he derives the characteristic equation

$$(386)\qquad 0 = 2 - 2e^{2\zeta} - (e^{\lambda\zeta} - e^{\zeta(2-\lambda)})\,(e^{\lambda\zeta} - e^{-\lambda\zeta})\,\frac{\sin\zeta}{\sin\lambda\zeta\,\sin(1-\lambda)\zeta}$$

for the dimensionless frequency ζ, where λ is the proportional distance from one end to L.
The case $\lambda = \frac{1}{2}$ is then treated in detail, with results amplifying those in E 443. 48

1) The following table defines and interconverts the numbers assigned to the types by EULER in his three treatments:

type	E 65	E 443	E 526
free-free	II	1	I
free-pinned	—	2 (V)	II
free-clamped	I	(0)	III
pinned-pinned	III	3	IV
pinned-clamped	—	4 (VI)	V
clamped-clamped	IV	5	VI

Published in the same year was JORDAN RICCATI's long treatise, *On the sounding vibrations of cylinders*[1]), which takes EULER's work of forty years before as its starting point[2]). [The general theory does not advance beyond the work of DANIEL BERNOULLI and
XIII—XXX EULER;] the body of the paper presents in all numerical detail the calculation of the proper frequencies and nodal distances for the first six free-free modes. [While EULER in his later papers E 443 and E 526 had considered rough approximations sufficient, RICCATI, following the spirit of E 65,] takes "lengthy pains" to get very accurate values for the roots ζ of (199) and for the nodal distances $z/\frac{1}{2}l$, where z is measured from the center to the node[3]):

Mode	ζ	$z/\frac{1}{2}l$
I	4,7300408	0,5516864
II	7,8532046	0,7357831, 0
III	10,9956079	0,2831042, 0,8111144
IV	14,1371655	0,4425042, 0,8530974, 0
V	17,2787596	0,1817456, 0,5438638, 0,8798045
VI	20,4203522	0,3076335, 0,6140386, 0,8982961, 0

[While DANIEL BERNOULLI had made a sufficient number of rough experiments to convince himself that the forms and frequencies of some of the modes of a rod are in fact as theory predicts,] RICCATI is the first to undertake a systematic and precise experimental
XXXI—XXXII program designed to check the predictions of theory. For brass and steel rods for which the fundamental is inaudible but the next five modes are not, he finds very good agreement with the above calculated values.

XXV Recalling his father's suggestion that the elastic properties of a material be determined from the sounds it emits (above, p. 115), [as indeed had been suggested earlier by LEIBNIZ
XXXIII and repeatedly thereafter by EULER in this very context,] RICCATI from the experimental

1) "*Delle vibrazioni sonore dei cilindri,*" Mem. mat. fis. soc. Italiana 1, 444—525 (1782).

I have not been able to see the work of LINDQUIST, "*De inflexionibus laminarum elasticarum,*" Aboae, 1777.

2) Evidently RICCATI had not seen any of EULER's later work on this subject. *E.g.*, in §§ XI and XXVII he corrects the error that EULER had himself corrected in E 84 (above, p. 221); in §§ III—IV he seems to be unaware of EULER's determination of $\mathcal{B}$ in E 303 (below, p. 388); and he does not refer to E 443 (above, pp. 323—325), where there is a treatment of the major problem discussed here.

3) These values agree closely with such as are cited from later authors or derived by RAYLEIGH in §§ 174 and 178 of his *Theory of Sound*, Cambridge, 1877. The only difference not in the last figure occurs for the smaller nodal distance for the third mode, for which RAYLEIGH gives 0,288394. The table given by BISHOP & JOHNSON, *op. cit. ante*, p. 326, is not sufficient to determine nodal distances with this accuracy.

results concludes that [1])

(387) $$\frac{E_{\text{steel}}}{E_{\text{brass}}} \approx 2{,}06\,.$$

[The values for these alloys as manufactured today yield ratios from 2,13 to 2,22.]

Using his criterion of equal kinetic energy (above, pp. 280—281), RICCATI infers that equable tone from an instrument whose sources of sound are cylinders of like material can be produced if XXXIV—XXV

(388) $$d \propto \nu^{-\frac{2}{5}},\ l \propto \nu^{-\frac{7}{10}},$$

and he gives a table constructed from these formulae over a range of two octaves. [I do not follow his dimensional argument, nor can I justify the results on other grounds.]

A fuller program of experiment was published in 1787 by CHLADNI [2]). Although he considers the BERNOULLI-EULER theory of straight prismatic rods so perfect as to leave nothing further to be learned [3]), CHLADNI takes the pains to verify by experiment the tonal sequences and nodal patterns of the first few modes for all six kinds of end conditions [4]). With minor reservations, he confirms the prediction of the theory in precise detail.

For the vibrations of circular rods it is a different matter [5]). Of EULER's two theories (above, § 46), which predicted frequencies in the ratios $(r+1)\sqrt{r^2+2r}$ and r^2, respectively, CHLADNI writes that while the displacement was assumed to lie in the plane of the ring, "experience teaches that the parts of an elastic ring that is not too thick are more inclined to vibrate up and down rather than in and out" and that in both cases the sequence of tones is entirely different from EULER's predictions. CHLADNI's experiments show that the k^{th} mode has $2k+2$ nodes and that the progression of frequencies is $(2k+1)^2$. [Subsequent work has not borne out the latter result.] The dependence of frequency on length and thickness he finds to be the same as for straight rods.

CHLADNI states finally [6]) that if a long, thin string is stroked with a violin bow at a

1) We do not remark upon RICCATI's use of "YOUNG's modulus", since it had been defined and explained earlier by EULER (see § 60, below).

2) *Entdeckungen über die Theorie des Klanges*, Leipzig, Weidmanns Erben und Reich, 1787.

3) *Op. cit.*, p. 1. He regards the theory as initiated by DANIEL BERNOULLI and perfected by EULER.

The work of C. B. FUNK, "*Versuch über die Lehre vom Schall und Ton,*" Leipziger Magazin zur Naturkunde, Mathematik, und Ökonomie 1781, 88—96, 210—227, 463—471, is merely descriptive and contains nothing of value.

Most of the theoretical and experimental studies by other minor writers whom CHLADNI criticizes harshly I have been unable to see.

4) CHLADNI employs the theoretical values given by EULER in his last paper on the subject (E 526, above, p. 326) and by RICCATI (above, p. 328). CHLADNI's experimental results are described in *op. cit.*, pp. 2—15, and in §§ 79—87 of *Die Akustik*, Leipzig, Breitkopf und Härtel, 1802.

5) *Entdeckungen*, pp. 16—17. *Akustik*, § 100.

6) *Entdeckungen*, p. 76.

very acute angle, there may be produced a sequence of tones in the ratios 1, 2, 3, 4, . . . but three to five octaves higher than the usual tones of transverse oscillation. "All these tones sound rather unpleasant . . . They have no definite relation to the tones obtainable by rectangular strokes, in that the tension of the string has very little influence, so that when the usual tones are raised an octave by increasing the tension, these newly observed tones are increased by scarcely a semitone." These tones CHLADNI later recognizes as arising from longitudinal elastic oscillation[1]).

[That such tones are much higher than those of transverse vibration is immediate from the fact that $c^2 = EA/\sigma$ for the former, $c^2 = T/\sigma$ for the latter : In order for the speeds of propagation, and hence the frequencies of a wave of a fixed wave-length, to be equal for the two kinds of oscillation, we should have to have $T = EA$, the tension theoretically sufficient to double the length of the string[2]).

With the perspective of more than a century, we easily account for the complete success of the theory for small vibrations of straight rods and complete failure for curved rods. Not only dynamical principles but also, for the simple modes, even mathematical analysis was sufficient, but lacking was an adequate description of the *strain* of a rod, a necessary preliminary to a correct theory.]

IV D. Vibrations of membranes and plates

48. EULER's theory of the vibrating membrane (1759). EULER's paper of 1759, *On the vibration of drums*[3]), gives the first successful attempt to describe the deformation of a body of more than one dimension. The summary of this paper and of EULER's paper E 303 on rings and bells (above, pp. 320—322) tells us, "Here are undertaken two investigations pertaining to acoustics, so difficult that he who has succeeded in some measure in reducing those sounds to calculation must be regarded as extraordinarily superior[4]). It is now abundantly clear what difficulties were involved in the question of vibratory motion of strings until a virtually new part of integral calculus was undertaken. Since in questions . . . regarding the sound or vibration of drums and bells . . . the disturbance of an entire surface or even of a body is investigated, it is easily seen that much deeper mysteries of cal-

Summarium

1) *Akustik*, §§ 60—62.

2) *Cf.* § 151 of RAYLEIGH's *Theory of Sound.*

3) E 302, "*De motu vibratorio tympanorum,*" Novi comm. acad. sci. Petrop. **10** (1764), 243—260 (1766) = Opera omnia II **10**, 344—359. Presentation dates: 22 January 1761, 17 May 1762. In his letter of 1 January 1760 to LAGRANGE, EULER tells of just having read this paper and E 303 to the Academy of Berlin. *Cf.* footnote 2, p. 320, above. Most of the contents are summarized on pp. 86—87 of Notebook EH 8, probably written in 1759.

4) Even DANIEL BERNOULLI agreed to this, for in § 6 of *op. cit. ante*, p. 291, he speaks of "bells or drums, . . . which only the incomparable EULER has dared to treat."

culation are needed here. What must first be done is properly to reduce these motions . . . to calculation, which cannot be achieved without certain hypotheses regarding the structure of these bodies." The author establishes "rules" [*i. e.* partial differential equations] for the small motion of drums and bells ; for the former, these are of second order, for the latter, of fourth order. Since they are too difficult to yield to a general solution such as that for strings, the author investigates only regular vibrations corresponding to a certain sound. He observes that just as is the case for strings, a bell or drum that can emit several single sounds can also emit these same sounds simultaneously. However, for a bell or drum the various possible sounds are most disharmonious.

It is appropriate to regard a stretched cloth or membrane as composed of threads 1—2
along its length and breadth, as is really the case for a cloth, while in a membrane the
number of threads is to be regarded as infinite. Supposing the net of threads to be rectan- 3—4
gular, let the interval between them be δ and let the two sets of threads be subject to tensions $T_x\delta$ and $T_y\delta$, respectively. An element displaced to a height $z(x, y)$ at the point x, y is thus pressed downward by two forces, as follows:

From the thread parallel to the x-axis,

$$\text{by a force} = -T_x\delta\,\frac{z(x,y) - z(x+\delta,y) - z(x-\delta,y)}{\delta}\,, \tag{389}$$
$$= T_x\delta^2\frac{\partial^2 z}{\partial x^2}\,.$$

From the thread parallel to the y-axis,

$$\text{by a force} = T_y\delta^2\,\frac{\partial^2 z}{\partial y^2}\,. \tag{390}$$

By the "principles of mechanics", the sum of these forces equals $\tau\delta^2\frac{\partial^2 z}{\partial t^2}$, where τ is the surface density of the membrane. Thus we obtain the partial differential equation

$$\tau\frac{\partial^2 z}{\partial t^2} = T_x\frac{\partial^2 z}{\partial x^2} + T_y\frac{\partial^2 z}{\partial y^2}\,. \tag{391}$$

[It is plain that Euler expects the reader to have read the material on finite differences in
his *Differential Calculus*[1]).] Henceforth we consider only the case when the two tensions are 6
equal, $T_x = T_y = T$, say, and we put $c^2 = T/\tau$, so that (391) becomes

$$\frac{1}{c^2}\,\frac{\partial^2 z}{\partial t^2} = \frac{\partial^2 z}{\partial x^2} + \frac{\partial^2 z}{\partial y^2}\,. \tag{392}$$

We try a solution $z = v\sin(\omega t + \mathfrak{A})$ and find that 7

$$0 = \frac{\omega^2 v}{c^2} + \frac{\partial^2 v}{\partial x^2} + \frac{\partial^2 v}{\partial y^2}\,. \tag{393}$$

1) Ch. I of Part I of E 212, *Institutiones calculi differentialis cum eius usu in analysi finitorum ac doctrina serium*, Petrop. 1755 = Opera omnia I 10.

Since this equation has sinusoidal solutions, there are solutions of (392) having the form

$$z = A\sin(\omega t + \mathfrak{A})\sin\left(\frac{\beta x}{a} + \mathfrak{B}\right)\sin\left(\frac{\gamma y}{b} + \mathfrak{C}\right), \tag{394}$$

where

$$\frac{\omega^2}{c^2} = \frac{\beta^2}{a^2} + \frac{\gamma^2}{b^2}. \tag{395}$$

8—10 A simple vibration of the type

$$z = A\sin\omega t\sin\frac{\beta x}{a}\sin\frac{\gamma y}{b} \tag{396}$$

corresponds to vanishing initial velocity; the boundaries of a *rectangular* drum $0 \leqq x \leqq a$, $0 \leqq y \leqq b$ remain fixed if $\beta = 2m\pi$, $\gamma = 2n\pi$, where m and n are integers. From (395) follows

$$\nu = c\sqrt{\frac{m^2}{a^2} + \frac{n^2}{b^2}}; \tag{397}$$

for a square membrane, the ratios of these frequencies are in general irrational and most inharmonious. [The correct necessary and sufficient condition is $\beta = m\pi$, $\gamma = n\pi$, so that

$$\nu = \tfrac{1}{2}c\sqrt{\frac{m^2}{a^2} + \frac{n^2}{b^2}}. \tag{398}$$

EULER's slip has caused him to miss the fundamental. One would expect that he would have noticed that his formula (397) does not reduce to TAYLOR's formula (73) for the vibrating string when $n = 0$ and $m = 1$.]

13—15 To consider a *circular* drum, EULER transforms (393) to polar co-ordinates r, φ, obtaining

$$\frac{1}{c^2}\frac{\partial^2 z}{\partial t^2} = \frac{\partial^2 z}{\partial r^2} + \frac{1}{r}\frac{\partial z}{\partial r} + \frac{1}{r^2}\frac{\partial^2 z}{\partial \varphi^2}. \tag{399}$$

"Moreover, the way in which we have reached this equation constitutes a certain new kind of algorithm, which seems worthy of all attention." [The "new algorithm" is only straightforward calculation of $\partial^2 z/\partial x^2$ and $\partial^2 z/\partial y^2$ in terms of the polar co-ordinates $x = r\cos\varphi$, $y = r\sin\varphi$. The modern reader must realize that this is the first occurence of a simultaneous change of both independent variables in a partial differential equation[1]). The long calculation of partial derivatives, still known to few savants and often used awkwardly if not erroneously, must have seemed a brilliant and abstruse display.]

16 There are solutions

$$z = u(r)\sin(\omega t + \mathfrak{A})\sin(\beta\varphi + \mathfrak{B}) \tag{400}$$

1) EULER himself had carried out this transformation a little earlier for the case when the solution depends upon r only; *cf.* pp. XLVI—XLVII of the Introduction to L. EULERI Opera omnia II 13.

provided

$$u'' + \frac{1}{r}u' + \left(\frac{\omega^2}{c^2} - \frac{\beta^2}{r^2}\right)u = 0\,. \tag{401}$$

[This is the first appearance of "BESSEL's equation" in its now usual standard form.] The substitution $u = r^\beta s$ yields

$$s'' + \frac{2\beta+1}{r}s' + \frac{\omega^2}{c^2}s\,. \tag{402}$$

EULER then calculates the power series solution we now write as

$$u = J_\beta\left(\frac{\omega}{c}r\right)\,. \tag{403}$$

In order for the edge $r = a$ to remain fixed, we must have

$$J_\beta\left(\frac{\omega}{c}a\right) = 0\,, \tag{404}$$

which EULER asserts to have infinitely many roots ω, so that infinitely many simple sounds result. Since z must be periodic of period 2π in φ, it follows that $\beta =$ an integer. EULER 17
expresses little hope of being able to calculate the "infinity of infinities" of frequencies given by the roots ω of (404) for $\beta = 0, 1, 2, \ldots$.

To find a second solution of (402), put $s = p \sin \omega r/c + q \cos \omega r/c$. The resulting 18
solution is

$$\begin{aligned} s = \frac{u}{r^\beta} &= \left(A \sin\frac{\omega r}{c} + \mathfrak{A}\cos\frac{\omega r}{c}\right)P + \left(\mathfrak{A}\sin\frac{\omega r}{c} - A\cos\frac{\omega r}{c}\right)Q\,, \\ &= CR\sin(\theta - \eta + \epsilon)\,, \end{aligned} \tag{405}$$

where P and Q are power series containing only even and odd powers, respectively of $\omega r/c$. EULER says that increasing φ by must have the same effect as replacing r by $-r$. Thus only even powers may occur on the right in (405); hence $A = 0$, and we fall back upon (403). [EULER's reasoning is not correct; neither is his series solution (405), since it imputes to BESSEL's equation two independent solutions regular at $r = 0$.]

Finally, there are solutions of (392) of the form 22

$$z = \Phi(\alpha x + \beta y + \gamma t)\,, \quad \alpha^2 + \beta^2 = \frac{\gamma^2}{c^2}\,, \tag{406}$$

and any number of these may be superposed.

[From this paper, which in some ways is the supreme achievement of the eighteenth century in the theory of deformable solids[1]), the reader brings away a certain disappoint-

1) BERNOULLI and LAGRANGE published not a word on this problem, and D'ALEMBERT had better not have published the little he did: "Bands and elastic plates can be considered as a heap or bundle of elastic strings . . . When the plate is bent, some of its fibres elongate, and the different points of the same plate are differently elongated." (Encycl. 5 (1755), art. "*Élasticité*").

The only other author in the century who ventured to attack it was JORDAN RICCATI, a not in-ment. After the brilliant derivation of the governing partial differential equation and the calculation of the normal modes, both for rectangular and for circular membranes, we might reasonably expect some discussion of the forms of the nodal lines, the relation of these to the proper frequencies, and the possible effects of superposing several modes—in a word, the mechanical implications of the results—but of these there is no trace. To these failings, along with the difficulty of producing a truly free vibration in a uniformly stretched membrane, must be laid the want of experimental confirmation of EULER's theory for nearly a century. Even CHLADNI, who made no serious attempt to perform experiments on a membrane, dismissed EULER's theory as "scarcely in entire accord with nature and not at all analogous to what is observed in the vibration of other surfaces[1])." EULER's theory is entirely correct but not developed[2]). Had EULER put into study of this theory, proposed in the middle of his scientific life, a portion of the energy he wasted on details concerning the vibrating string, the course of the doctrine of vibrations might have been dif-

considerable scientist but one who stood helpless before a partial differential equation (*cf.*, *e. g.*, footnote 1 on p. LXXI of my Introduction to L. EULERI Opera omnia II 13). His paper on this subject is "*Dissertazione fisico-matematica delle vibrazioni del tamburo,*" Saggi sci. lett. accad. Padova **1**, 419—446 (1786). Perhaps influenced by D'ALEMBERT's blind guidance, RICCATI regards a circular drumhead as a sheet of radial strings, taking the density of each such string proportional to r in order that the density of the membrane be uniform. RICCATI's not very perspicuous derivation (§ V) leads to the correct equation for the simple modes of such a string, *viz*

$$\frac{d^2 z}{dr^2} \propto r z ,$$

but this is not the correct equation for the symmetrical modes of a uniformly dense circular membrane, which, as follows at once from EULER's equation (399), must satisfy

$$\frac{d^2 z}{dr^2} + \frac{1}{r}\frac{dz}{dr} \propto z .$$

Thus all of RICCATI's conclusions regarding the proper frequencies, *etc.*, are wrong.

To the historian it is enlightening that not only does RICCATI in 1786 pay no heed to EULER's paper E 302, published twenty years before, but also after citing EULER's paper E 318, also published in 1766, on strings of non-uniform thickness, and after acknowledging his great debt to this paper, RICCATI approaches even the vibrating string in the old clumsy way used by DANIEL BERNOULLI and EULER in their earliest researches, a half century earlier! The explanation, of course, is that RICCATI cannot understand or use the principle of momentum *when it is expressed as a partial differential equation*. Disregarding EULER's formulation of the general problem of the vibrating string, RICCATI refers to EULER's paper only in connection with its "construction" of certain ordinary differential equations.

1) §§ 63—64 of *op. cit. ante*, footnote 4, p. 329.

2) In the acoustical literature the theory of the vibrating membrane is usually attributed to POISSON, ¶¶ 59—64 of "*Mémoire sur l'équilibre et le mouvement des corps élastiques,*" Mém. acad. sci. (2) **8**, 357—570 (1829). POISSON, apparently unaware of EULER's work, obtains the correct formula (398) instead of (397) and makes a start toward determining the nodal lines of rectangular and circular membranes but in fact advances little beyond EULER.

ferent, since in fact a membrane possesses modes similar to some of those observed by
CHLADNI in vibrating plates, to be described presently.]

49. CHLADNI's experiments on the vibrations of free plates (1787). [After the passage
of a quarter century in which nothing was learned regarding two-dimensional elastic
systems, a new period in the history of elasticity and acoustics begins in 1787] with
CHLADNI's *Discoveries on the theory of sound*[1]), [the first purely experimental work to be
founded on an understanding of the principles of mechanics and existing theory, and also
the first work on elastic vibrations in almost a century to discover by experiment results
not previously predicted by theory. While DANIEL BERNOULLI's experiments, as he himself
many times emphasized, were conceived and executed so as to confirm the predictions of his
theories,] CHLADNI deliberately selects a domain for which no theory exists : "The elastic P. 1
vibrations of strings and [straight] rods . . . are so accurately and cleverly calculated as to
leave very little new to be said regarding them ; on the other hand, the true nature of the
sound of bodies for which elastic curvatures of whole surfaces . . . come into consideration
simultaneously is still shrouded in the deepest darkness, since neither calculations agreeing
with experience nor correct observations about it are available." Sand scattered upon a hori- 18
zontal plate takes on its motion, so that the parts which remain at rest are easily seen. The
motion is excited by stroking the edge with a violin bow. Some kinds of vibration are 19
easy to produce, other, quite difficult ; " . . . in continued experimenting one finally gets
what is wanted, and often a sound difficult to obtain appears unexpectedly when one is
looking for another." There are certain *nodal lines* where there is no motion, and it is best to
support the plate at the intersection of two such lines ; such support renders the tone purer.
On the opposite sides of a nodal line the motion occurs in opposite senses. When two nodal
lines cross, points located in the opposite angles are in motion in the same sense.

The experiments on rectangular strips of glass or metal refer to the end conditions 21—24
free-free, fixed-free, and fixed-fixed, the sides in all cases being left free. However, the variety
of possible motions is so great that CHLADNI merely gives notice of their existence along
with a description of a few of the simplest.

Most of the work concerns the vibrations of circular plates, which he considers pre- 24
ferable to bells both for facility of experiment and control and because they give out a
greater variety of sounds. The edge of the plate is free of support, but the vibrations are
excited by stroking it. In the lowest observed mode, which CHALDNI calls the fundamental, 25—29
there are two orthogonal nodal diameters and no nodal circles ; a similar motion occurs in
the water in a drinking glass when the edge is excited by a violin bow. A footnote tells us 29—30
that with difficulty a mode in which there is but a single nodal diameter may be excited,

1) *Entdeckungen über die Theorie des Klanges*, Leipzig, Weidmanns Erben und Reich, 1787.

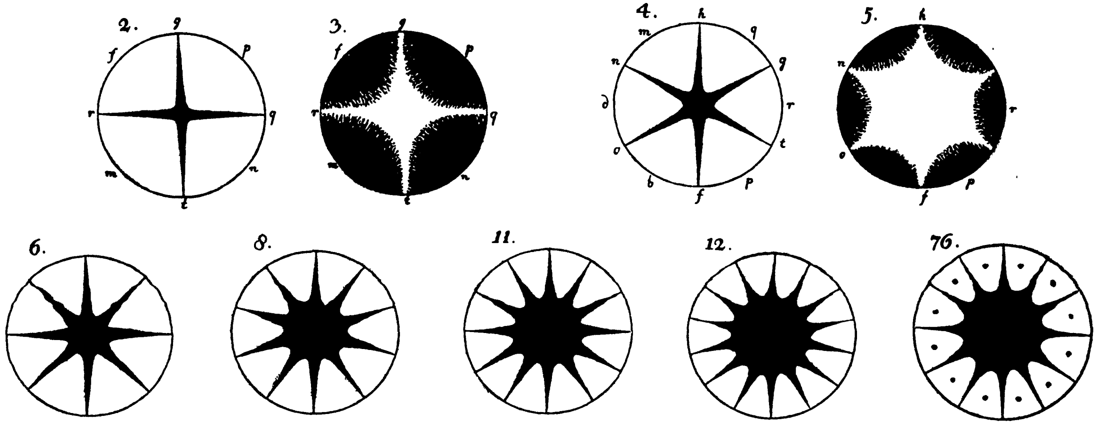

Figure 89. CHLADNI's figures showing nodal lines (1787)

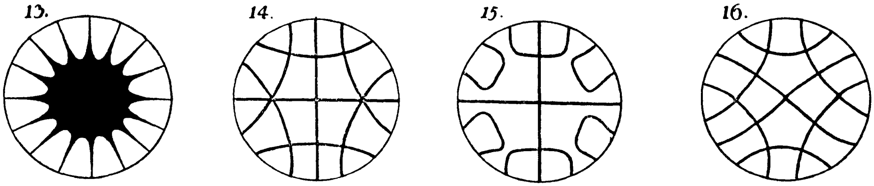

Figure 90. CHLADNI's "variants" of the mode with eight nodal lines and no nodal ring (1787)

but CHLADNI regards it as of another kind from the rest. There are modes in which any
number of nodal diameters occur (Figure 89). The greatest motion occurs at certain points
lying fairly near the edge and on the bisector of the angle between adjacent nodal dia-
33—34 meters; in the last drawing in Figure 89, these are indicated by dots. In many cases instead
of the star-like figure there results what CHLADNI calls a "variant", giving the same sound;
three such variants are shown in Figure 90 as belonging to the mode with eight nodal
34—35 lines. The sounds given out contradict EULER's conjecture that the tones of a bell
are the same as those of a circular rod,
whether the theoretical or experimental
results for the rod be used for comparison.
36 Both the variant figures and the positions of
the points of maximum excursion contradict
any analogy to the motion of a circular rod.

36—46 There are also modes in which there are
any number of nodal rings (Figure 91). Great
variations are possible; in practice, any large
number of nodal lines meeting in the center
is hard to produce. Several observed kinds of
motion are shown in Figure 92. These figures

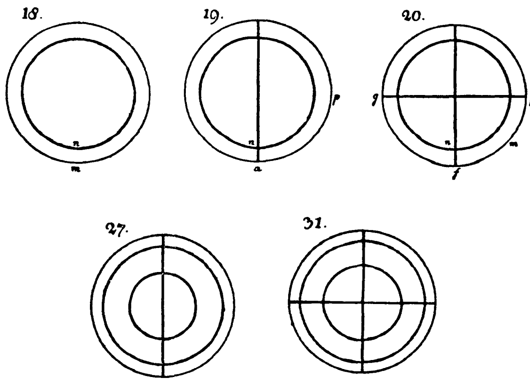

Figure 91.
CHLADNI's figures showing nodal rings (1787)

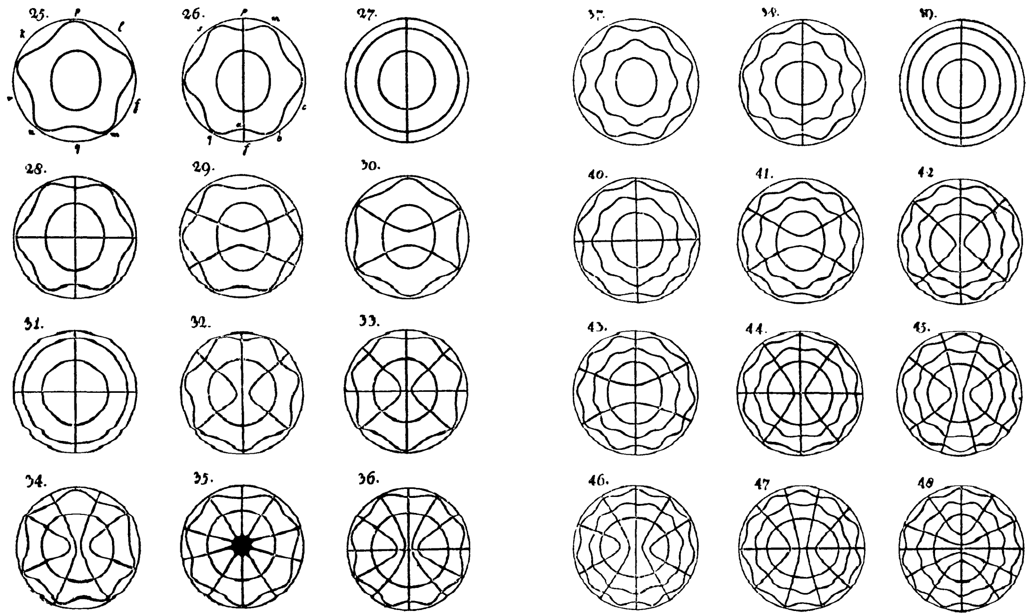

Figure 92. Two of CHLADNI's figures showing nodal patterns for circular plates (1787)

CHLADNI is able to classify, since "... the rings for each kind of sound exhibit a definite number of bends, except for the innermost one, which is sometimes quite circular but more usually oval. The number of bends is the same for each circle ..." Thus CHLADNI's Figures 28, 29, and 30 all show two nodal rings, two nodal diameters, and six bends, all three being variants of the pure mode shown in his Figure 31. If n_d, n_r, n_b denote the number of nodal diameters, nodal rings, and bends, respectively, then in all figures $n_b \geqq n_d + n_r$, but he indicates also such cases as $n_r = 2$, $n_d = 7$ or 8, $n_b = 8$. The experiments go as far as $n_r = 7$ and $n_d = 8$ or more.

Zahl der Linien:

Zahl der Kreiße:	0	1	2	3	4	5	6	7	8
0			fig. 2. C	4. d	6. 7. c	8--10. g--gis	11. cis	12. fis	13--17. b
1	18. Gis	19. b	20. 21. g	22. d--dis	23. gis	24. cis	e--f	g	
2	25. gis+	26. 27. e+	28--31. h	32. dis	33. 34. g	35. b--h	36. cis	dis	
3	37 b--h	38. 39. e+	40. 41. gis--a	42. 43. c	44. 45. dis	46. 47 fis	48. gis+	b	
4	49. a	50--52 cis	53. f—	54. g--gis	55. b	56. h--c	57. cis		
5	58. f	59. 60. gis	h	61. cis					
6	h	62 d	63. e	64. 65. f+					
7	66. e								

The pitches corresponding to the various modes are shown at the left: The tone C is chosen arbitrarily as corresponding to $n_r = 0, n_d = 2$; CHLADNI estimates the error as less than a semitone, even for the highest notes. Converting these pitches to frequencies, he infers that 46 47—48

$$\nu \propto (2n_r + n_l)^2 \quad (407)$$

There are also certain kinds of vibration which he is unable to explain satisfactorily as variants of the above. Some of these are shown in Figure 93. [CHLADNI is a firm believer in the theory of 49—51

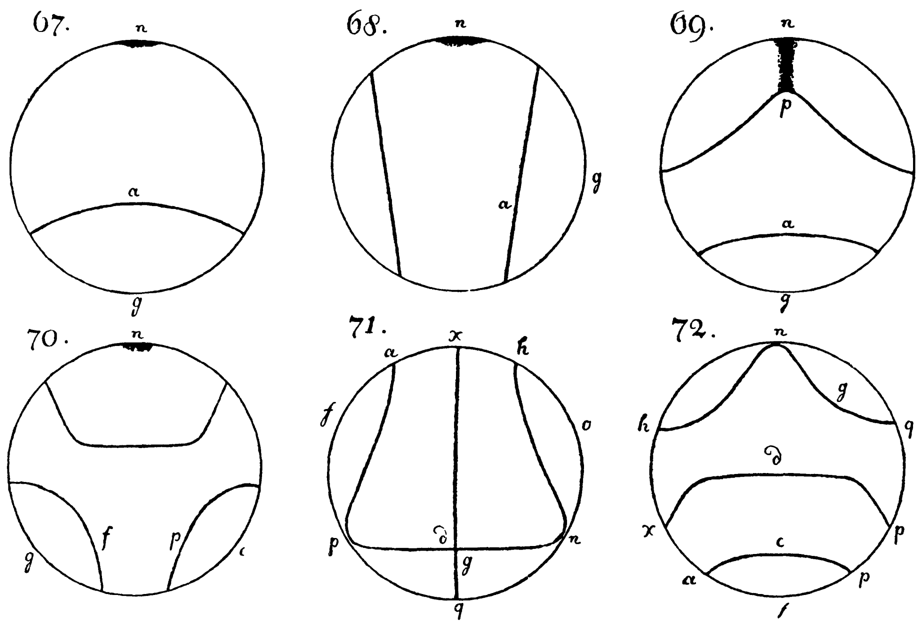

Figure 93. Nodal figures CHLADNI is unable to classify (1787)

simple modes,] so that his criterion for identity or difference of two motions is solely the frequency emitted.

Similar experiments on square plates are sufficiently summarized by some of the nodal diagrams (Figure 94), which "could suffice to enrich the sample books of the drapers and carpet makers." The frequencies are as follows :

	0	1	2	3	4	5
1		fig. 87. C				
2	88 G; 89. 90 B	91. 92 e	96--98. $\overline{\text{d}}$--$\overline{\text{dis}}$			
3	193--95 $\overline{\text{cis}}$--$\overline{\text{d}}$	99. 100 $\overline{\text{e}}$; 101 $\overline{\text{fis}}$	102--104 $\overline{\text{h}}$	111. 112 $\overline{\overline{\text{f}}}$		
4	105 106 $\overline{\overline{\text{c}}}$--$\overline{\overline{\text{cis}}}$; 107. 108 $\overline{\overline{\text{cis}}}$--$\overline{\overline{\text{d}}}$	109. 110 $\overline{\overline{\overline{\text{d}}}}$--$\overline{\overline{\overline{\text{dis}}}}$	113 $\overline{\overline{\text{fis}}}$; 114 $\overline{\overline{\text{g}}}$	120--122 $\overline{\overline{\text{h}}}$	125 126 $\overline{\overline{\overline{\text{dis}}}}$	
5	115--117 $\overline{\overline{\text{b}}}$	118. 119 $\overline{\overline{\text{b}}}$--$\overline{\overline{\text{h}}}$	123. 124 $\overline{\overline{\overline{\text{cis}}}}$	127 $\overline{\overline{\overline{\text{dis}}}}$ $\overline{\overline{\overline{\text{e}}}}$; 128 129 $\overline{\overline{\overline{\text{e}}}}$ $\overline{\overline{\overline{\text{f}}}}$	$\overline{\overline{\overline{\text{gis}}}}$	$\overline{\overline{\overline{\text{h}}}}$--$\overline{\overline{\overline{\text{c}}}}$
6	130. 131 $\overline{\overline{\overline{\text{e}}}}$--$\overline{\overline{\overline{\text{f}}}}$	132 $\overline{\overline{\overline{\text{f}}}}$	133-135 $\overline{\overline{\overline{\text{fis}}}}$; 136 $\overline{\overline{\overline{\text{g}}}}$	137. 138 $\overline{\overline{\overline{\text{a}}}}$	$\overline{\overline{\overline{\overline{\text{c}}}}}$	$\overline{\overline{\overline{\text{dis}}}}$
7	139 $\overline{\overline{\overline{\text{b}}}}$	$\overline{\overline{\overline{\text{b}}}}$	140--142 $\overline{\overline{\overline{\text{h}}}}$	143--145 $\overline{\overline{\overline{\overline{\text{cis}}}}}$	$\overline{\overline{\overline{\overline{\text{e}}}}}$	
8		$\overline{\overline{\overline{\text{d}}}}$--$\overline{\overline{\overline{\text{dis}}}}$	146 $\overline{\overline{\overline{\overline{\text{e}}}}}$	$\overline{\overline{\overline{\overline{\text{f}}}}}$		
9		$\overline{\overline{\overline{\text{fis}}}}$	$\overline{\overline{\overline{\text{g}}}}$			

The frequencies in the first column agree with those for a rod with both ends free ; "regarding the other tonal ratios I prefer to say nothing further, lest error result from too hasty

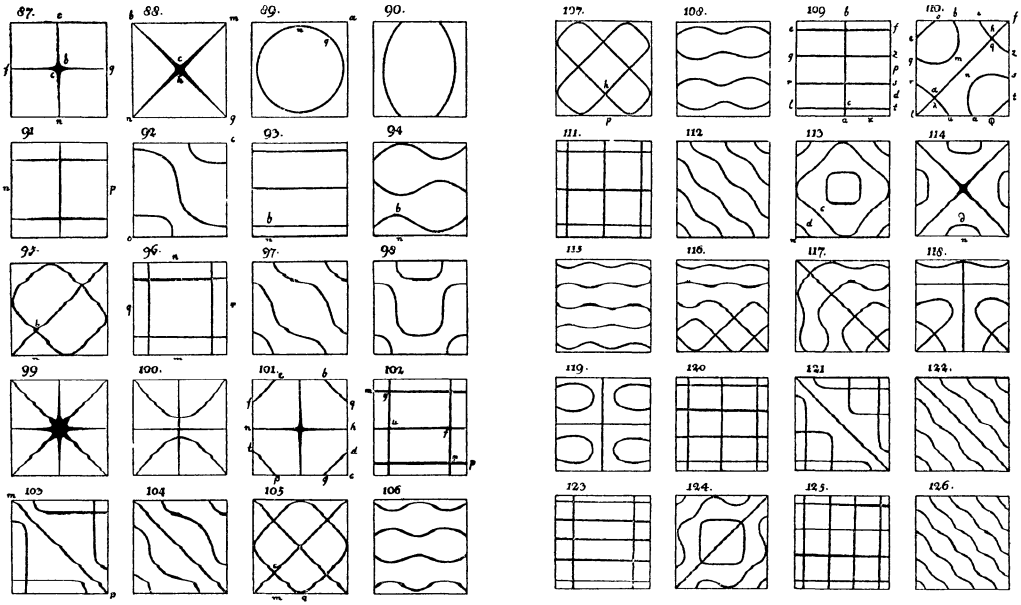

Figure 94. Two of CHLADNI's plates showing nodal patterns for square plates (1787)

claims." [As is now well known, the vibrations of a square plate present problems of a higher order of difficulty than those for a circular one.]

For plates of the same material, the frequency of a given mode obeys the rule 64

(408) $$\nu \propto \frac{h}{d^2}, \qquad h = \text{thickness}, \quad d = \text{diameter}.$$

CHLADNI takes pains to expose the errors of several contemporary non-mathematical 65—73
writers on acoustics. His discussion is based on the concept of coexistence of small oscillations[1]), which he attributes expressly to EULER and DANIEL BERNOULLI, and on his experiments. He considers it diffiuclt if not impossible to produce a pure tone unmixed with harmonics, but he is certain that every sound is composed only of tones which *could* (presumably, in principle) be emitted singly by the same sounding body. "A *sound* results when an elastic body makes isochronous and audible oscillations. The isochrony of vibrations is incontestably the single essential property that distinguishes a sound from any other noise . . . A sound is called a *tone* when account is taken only of . . . the greater or lesser speed of its vibrations" [*i. e.*, of its frequency].

1) On p. 68 CHLADNI cites TAYLOR's book and all the papers of EULER, DANIEL BERNOULLI, and LAGRANGE on the vibrating string, after which he mentions "various writings of D'ALEMBERT. Certain more recent authors should have made better use of the essays cited above than in fact they have."

73—75 After describing a three-dimensional vibratory motion of a rod analogous to the elliptical vibrations of strings discussed theoretically by Daniel Bernoulli and Euler
75—76 (below, pp. 376—377), Chladni mentions a motion of a plate in which the corners vibrate while there are two orthogonal nodal surfaces in the interior.

77 Chladni's work ends with a program for theory. "Perhaps the above remarks . . . may give the stimulus to develop further the theory of the curvatures of a surface or a body, which offers an unbounded field for further investigations . . ." and he cites Euler's remarks to this same effect (above, p. 330).

The challenge of Chladni's experiments was taken up at once by James II Bernoulli, but in disregard of Chladni's warning he made no preliminary attempt to analyse the deformation of surfaces. His *Theoretical essay on the free vibrations of rectangular elastic plates*[1]), [the first paper in the history of rational elasticity for which the problem is suggested by the results of experiment, is also one of the few early mathematical works that rests on entirely wrong principles,] for Bernoulli, heedless of Chladni's express contrary
9—10 admonition, postulates an analogy between a plate and a network of rods. Regarding the restoring force as arising solely in response to the bending of two orthogonal rods, he obtains as the equation for the simple modes

$$\frac{\partial^4 z}{\partial x^4} + \frac{\partial^4 z}{\partial y^4} = \frac{z}{c^4} \quad . \tag{409}$$

While a similar analogy between a flexible membrane and a network of strings had led Euler to correct results, [we nowadays see at once that (409) cannot represent any physical problem concerning an isotropic plane, since it is not invariant under rotation of co-ordinates]. Bernoulli admits the insufficiency of his model, but he mentions only two possible sources of error. (1) The motion is assumed normal to the plate, but he considers the error negligible for small vibrations. (2) A different set of rods, as for example concentric and axial rings, might lead to different results, but this he regards as indicative merely that his solutions are special ones. [Thus, unlike Chladni, he fails to grasp the mechanical reason invalidating his model for *all* vibrations except those for which $z = z(x)$, or $z = z(y)$.] "But also I give this memoir only as a first attempt . . ."

Bernoulli calculates the proper frequencies and nodal lines for a rectangular plate, and for a square plate he compares them with Chladni's experimental results. There is little or no agreement.

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1) "*Essai théoretique sur les vibrations des plaques élastiques rectangulaires et libres,*" Nova acta acad. sci. Petrop. 5 (1787), 197—219 (1788).

An unpublished work[1]) of James II Bernoulli discusses the vibrations of circular plates according to the same [erroneous] theory.

[Not failure of mechanics but lack of a sufficient differential geometry of surfaces, as Chladni had implied, left the eighteenth century geometers incapable of a proper theory of elastic plates.]

IV E. Discrete models

50. Discrete models[2]). Euler's paper of 1764, *On the equilibrium and motion of bodies connected by flexible joints*[3]), attempts to illumine the nature of elastic bodies, especially 1—2
elastic plates, by a model of rigid bodies connected by elastic junctions. Consider bodies with centers 3—4
of mass at A and B (Figure 95), where the perpendiculars Aa and Bb to the "axis of bending" Mm need not lie in a plane; the axis Mm represents an elastic wire which can be twisted. After the 5
twisting, regard Aa as unchanged and Bb as rotated through an angle ω (Figure 96) to the configuration bB'. Replace the elasticity of torsion by an elastic cord, shown dashed, along the arc from B to B'. The length of this arc is $2b\sin\frac{1}{2}\omega$. The "elastic force", being proportional to the change of length of the elastic cord, is $K\cdot 2b\sin\frac{1}{2}\omega$. The bending moment exerted by this force is

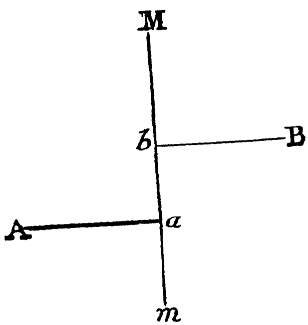

Figure 95. Euler's model for torsion (1763–1764)

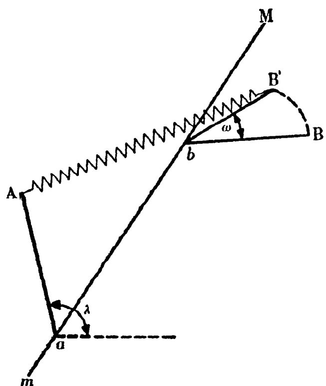

Figure 96. Clearer sketch of Euler's model for torsion

$$2Kb\sin\tfrac{1}{2}\omega\cdot b\cos\tfrac{1}{2}\omega = Kb^2\sin\omega\ . \tag{410}$$

Now regard A and B' as connected by a thread; the moment exerted by the tension T 6
of the thread must equal the bending moment (410). Writing $E = Kb^2$, we have

$$T = \frac{E\sin\omega\sqrt{a^2+b^2-2ab\cos(\lambda-\omega)}}{ab\sin(\lambda-\omega)}\ , \tag{411}$$

1) Universitätsbibliothek Basel, MS L Ia 39⁵, pp. 1—16. On pp. 6—8 Bernoulli "by an altogether different route" derives Euler's [false] second theory of the vibrations of circular rings, and on pp. 9—10 he obtains (409) in polar co-ordinates.

2) Lagrange's work of 1760 on setting up the equations of motion of discrete systems will be discussed below in § 55.

3) E 374, "*De aequilibrio et motu corporum flexuris elasticis iunctorum*," Novi comm. acad. sci. Petrop. **13** (1768), 259—304 (1769) = Opera omnia II **11**, 3—61. Presentation dates: 1 December 1763, 23 August 1764.

7—12 where λ is the angle between Aa and Bb before bending. Supposing $T = D$ is the tension required for equilibrium, EULER then regards the point A as fixed, takes $\lambda = 0$, and considers the tension T as a constant D, independent of ω. Then it is easy to set up equations of motion by equating the torque of D less the torque $Kb^2 \sin \omega$ to the rate of change of angular momentum. A discussion of the integration leads to nothing definite.

[The tentative and diverse nature of the contents of this paper is doubtless responsible for the neglect bestowed upon the results just obtained. EULER has in fact introduced the concept of *twist* of a straight rod and has constructed the first *theory of torsion*. The formula he derives from his discrete model implies that for small twists, the torque is proportional to the twist. This now classic law of elasticity is to be inferred directly from experiments by COULOMB more than a decade later (§ 61, below). EULER, however, intent on plates, gives no further attention to the result just derived.]

15—19 Problem 1: To find the condition of equilibrium for a body composed of an arbitrary number of parts connected by elastic joints and subject to arbitrary forces. EULER proposes a principle of solidification, asserting that if all the joints suddenly become rigid, equilibrium is not disturbed. Hence the total force and total moment of the external load must vanish. At a junction, in imagination divide the body in two. The bending moment of the load on one part equals the bending moment exerted by the joint. EULER regards this principle, which generalizes that he has used for many years for setting up special cases concerning bending (*e. g.*, in E 8, above, pp. 148—150), as of the greatest importance. [Indeed, we see here a step toward the principle of moment of momentum for general bodies.]

20 The solution of Problem 2 is a proof of the vectorial character of moments, in three dimensions.

The remainder of the paper consists in applications of these principles to special cases, leading to Problem 6, in which equations of motion are established for three bodies connected by elastic junctions and free to move in a plane. The moments of the joints are taken as proportional to the sines of the angles between the lines connecting the centers of gravity of the bodies to the junctions. EULER is unable to draw any conclusions from the equations.

[This paper, following shortly upon EULER's treatment of membranes by conceiving them as a network of taut cords, shows us that EULER, years before CHLADNI's warnings, saw that the elastic response of a plate is more complicated than that of a network of bent rods. Plainly he hoped, despite the mainly negative results of earlier experience with discrete models for cords, chains, and rods, that a discrete model might here suggest the right approach to a theory of plates[1]). The result is again negative. While EULER demonstrates his mastery of the principles of linear and angular momentum in setting up the equations

1) This is supported by the title, "Principles for the equilibrium and motion of elastic bodies," which EULER sets at the head of a preliminary study on p. 32 of Notebook EH 7 (1760 or 1761—1762).

for complicated discrete systems, the paper achieves no advance in the principles of elasticity.

All further work in the eighteenth century on discrete mechanical systems is of modest scope, aimed only toward details.]

About 1765 LAGRANGE mentions the problem of the weighted string hung up by one end[1]). The differential equations,

$$\frac{d^2 y_k}{dt^2} + \frac{y_k - y_{k+1}}{a} - (k-1)\,\frac{y_{k-1} - 2y_k + y_{k+1}}{a} = 0 \tag{412}$$

[derived long ago by D'ALEMBERT (above, pp. 188—189)], are of the type LAGRANGE can can solve[2]), [but he does not advance beyond EULER's old results (118) and (119).] He writes, "it would be difficult, perhaps impossible" to determine the proper frequencies, but "one may assure oneself, by the very nature of the problem, that these roots are necessarily all real, unequal, and negative", since otherwise the displacements could increase to infinity, "which would be absurd." [While LAGRANGE, like D'ALEMBERT before him (above, p. 192), proves nothing, his assertion is true, since the equation $L_n(x) = 0$ has n positive and distinct roots.]

A contrast of methods is furnished by two late papers of DANIEL BERNOULLI and EULER on the compound pendulum[3]). It is plain that EULER considers BERNOULLI's use of the old, directly postulated methods for obtaining the equations of motion of rigid bodies as unfortunate. EULER's paper, printed on the pages following BERNOULLI's, easily sets up the exact equations of motion on the basis of the "first principles of mechanics". BERNOULLI's results then follow as approximations.

The next attempt, on p. 36, has almost the same title as E 374; here EULER considers each member of the system subject to a force and to two equal and contrary forces acting at equal distances from its center of inertia [*i. e.* a couple], "so that these two forces should have no effect on the progressive motion." In addition, as in E 174 (above, p. 228), a force of arbitrary direction acts at each junction; this force represents the elasticity of the link. The "paradox" which is observed and explained in E 374 is noted on p. 38.

1) § 36 of *op. cit. ante*, p. 278.

2) As had EULER in solving (235), LAGRANGE is essence tries for solutions of the type $y_k = \mathfrak{A}_k e^{\varrho t}$. In § 30 he had discussed the more general system

$$\frac{d^2 y_k}{dt^2} + \sum_{p=1}^{k} A_{kp} y_p = 0 .$$

3) D. BERNOULLI, "*Vera determinatio centri oscillationis in corporibus qualibuscunque filo flexili suspensis eiusque ab regula communi discrepantia,*" Novi comm. acad. sci. Petrop. 18 (1773), 247—267 (1774).

EULER, E 455, "*Determinatio motus oscillatorii in praecedente dissertatione pertractati ex primis mechanicae principiis petita,*" *ibid.* 268—288 = Opera omnia II 11, 142—157. Presentation date: 9 December 1773.

In two further papers[1]) Bernoulli dwells upon special cases of the possible motion
of a pendulum of two parts. In the same year, Euler[2]) returns to the old problem of the
2 weightless string loaded by discrete weights. The equations of finite motion are obtained by
adding $M_k g$ to the right-hand side of $(209)_1$, so that the x-axis points vertically downward,
and by using the slope angles, $\theta_k = \frac{1}{2}\pi - \varphi_k$. When θ_k is small, these equations and the
constraints yield (156) and $y_k = \sum_{r=1}^{k} a_r \theta_r$. Putting these results into $(209)_2$ yields

$$\text{(413)} \qquad \frac{1}{g} \sum_{r=1}^{k} a_r \ddot{\theta}_r = -\alpha_k \theta_k + (\alpha_k - 1)\, \theta_{k+1} \,,$$

where $\alpha_k \equiv \dfrac{T}{M_k g} = \sum_{r=k}^{n} M_r / M_k$. Put $\theta_r = A_r z$. Then (413) admits a solution such that
$\ddot{z} = -Kgz$, provided that

$$\text{(414)} \qquad \sum_{r=1}^{k} a_r A_r = K\,[\alpha_k A_k - (\alpha_k - 1)\, A_{k+1}] \,.$$

In order that these linear equations for the coefficients be compatible, K must satisfy an
6—16 algebraic equation of degree k. Euler is unable to proceed explicitly except in the special
case when $k = 4$, and even then the result is complicated. The general solution of (413)
is to be gotten by superposition of particular solutions corresponding to these values of
16 K. Euler considers that "scarcely anyone would go to the trouble" of determining the
arbitrary constants from the initial conditions, so that "this solution, however elegant and
12 perfect, is plainly unsuited to be adapted ... to specific cases." Its importance lies in
showing that "the principle of the most illustrious Daniel Bernoulli ... is thoroughly
founded in the first principles of motion and can be derived immediately from them."
17—21 For the case of equal weights equally spaced, Euler derives again his old results (118) and
(119).

[Further studies of discrete oscillating systems do not seem to have contributed even indirectly to theories of flexible or elastic bodies[3]).]

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1) §§ 7—11 of *op. cit. ante*, p. 291, followed by "*Commentatio physico-mechanica specialior de motibus reciprocis compositis multifariis nondum exploratis qui in pendulis bimembribus facilius observari possint in confirmationem principii sui de coexistentia vibrationum simpliciorum*," Novi comm. acad. sci. Petrop. **19** (1774), 260—284 (1775).

2) E 468, "*De oscillationibus minimis penduli quotcunque pondusculis onusti*," Novi comm. acad. sci. Petrop. **19** (1774), 285—301 (1775). Presentation date: 3 October 1774.

3) Euler, E 469, "*De motu oscillatorio binarum lancium ex libra suspensarum*," Novi comm. acad. sci. Petrop. **19** (1774), 302—324 (1775). Presentation date: 10 October 1774.

Euler, E 470, *Explicatio motus oscillatorii mirabilis in libra majore observati*," *ibid.* 325—339. Presentation date: 10 October 1774.

Euler, E 525, "*De motu oscillatorio mixto plurium pendulorum ex eodem corpore mobili suspensorum*," Acta acad. sci. Petrop. 1779: I, 89—102 (1782). Presentation date: 13 October 1774.

Euler, E 533, "*De motu oscillatorio pendulorum ex filo tenso dependentium*," Acta acad. sci. Petrop. 1779: II, 95—105 (1783) = Opera omnia II 7, 91—100. Presentation date: 17 October 1774.

IV F. Plane static deflection and buckling of straight bars[1])

51. EULER's calculation of buckling loads for columns of non-uniform section (1757). [EULER's brilliant discovery of the phenomenon of buckling in 1743 had attracted no notice;] in 1759 he published a paper, *On the strength of columns*[2]), which presents calculations of buckling loads without much accompanying material on elastic curves. He remarks that X
"this difference between the action of a horizontal force and a vertical one will seem not a little paradoxical: It seems that if a large force bends a column, a smaller one ought always to produce a similar effect, even if perhaps imperceptible. The principle of continuity seems to require it ..." But in fact the bending may be "imaginary". [Instead of approx- XI, XV—XVI
imating the exact solution (172),] EULER now employs an approximate differential equation,

$$\mathcal{B} y'' + P y = 0 \,, \tag{415}$$

which follows from (171) by changes of notation and by supposing that the load P acts in the direction of the unbent bar and produces only slight bending. When $\mathcal{B} = \text{const.}$, the sinusoidal solution (178) follows at once, as does the critical load $(185)_2$. "If one develops XVIII
the calculation more accurately," not neglecting the difference between s and x, one will find that

$$\frac{P}{P_c} = \sec\alpha \,, \tag{416}$$

where α is the angle between the load and the tangent at the point of application. If $P < P_c$, the angle α is imaginary. [EULER does not disclose how he obtains the crude approximation (416). It is unfortunate that his work on buckling has become known mainly through this paper instead of through the exact treatment given in the work of discovery E 65, where, among other things, the bent forms are determined and, in particular, the exact formula (187) renders unnecessary any such approximation as (416). EULER's explanation that α is imaginary when P/P_c, while not illuminating now that LEIBNIZ's law of continuity is no longer believed, is true in respect to the exact formula (187).

The value of this paper lies in] its calculation of critical loads for *columns of non-uniform stiffness*. The differential equation to be solved is (415), with $\mathcal{B} = \mathcal{B}(x)$. EULER XIX
puts $y = e^{\int u\,dx}$ and obtains

$$u' + u^2 + P/\mathcal{B} = 0 \,. \tag{417}$$

N. FUSS, "*Determinatio motuum penduli compositi bifili ex primis mechanicae principiis petita,*" Nova acta acad. sci. Petrop. 1783: I, 184—202 (1787).

N. FUSS, "*Additiones analyticae ad dissertationem de motu penduli bifili,*" *ibid.* 204—212.

1) For this part I have found helpful the essay of NIKOLAI, cited above, p. 212.

2) E 238, "*Sur la force des colonnes,*" Hist. acad. sci. Berlin [13] (1757), 252—282 (1759). Presentation date: 1 September 1757.

XIX—XXXV For a column such that

(418) $$\mathcal{B} = \mathcal{B}_0\left(\alpha + \frac{\beta x}{l}\right)^{\lambda},$$

XX—XXI he carries out the integration for certain values of λ and discusses the results. *E. g.*, if $\lambda = 4$, he finds that

(419) $$y = A(\alpha l + \beta x)\sin\frac{\sqrt{P}\,l x}{\sqrt{\mathcal{B}_0}\,\alpha(\alpha l + \beta x)}\,.$$

Hence when both ends are pinned the critical load is given by

(420) $$P_c = \pi^2\frac{\alpha^2(\alpha+\beta)^2}{l^2}\mathcal{B}_0\,.$$

XXII If $\alpha = 0$ or $= \alpha - \beta$, then $P_c = 0$; "hence we see that a column pointed either above
XXXIV or below has no strength." As EULER remarks later, these results are appropriate to a conical column. [If the diameters at top and bottom are d_1 and d_2, so that $\alpha = d_1/d_0$, $\alpha + \beta = d_2/d_0$, where d_0 is a typical diameter, then EULER's result (420) may be written in the form

(421) $$P_c = \pi^2\frac{d_1^2 d_2^2}{l^2}\cdot\frac{\mathcal{B}_0}{d_0^4}\,;$$

EULER slips in deriving a result of this kind.] EULER begins some comparisons of the strengths of different conical columns, [but his results are inconclusive if not incorrect].

XXIII Considering (418) for any λ, EULER shows that the solution is of the form

(422) $$y = A\sqrt{R^2+Q^2}\,\sin\left[\frac{\sqrt{P}\,l\left(\alpha + \frac{\beta x}{l}\right)^{2\lambda-1}}{\beta\sqrt{\mathcal{B}_0}\,(2\lambda-1)} - \operatorname{Arc\,tan}\frac{Q}{R} + \text{const.}\right],$$

XXX—XXXI where Q and R are certain series which he gives explicitly. The buckling load can then be calculated in principle. The singular case $\lambda = \frac{1}{2}$, appropriate to a column having a parabolic profile, leads to

(423) $$P_c = \left(\frac{1}{4} + \frac{\pi^2}{\left[\log\left(1+\frac{\beta}{\alpha}\right)\right]^2}\right)\frac{\beta^2\mathcal{B}_0}{l^2}\,.$$

XXXVI Next EULER takes up the problem of bending of a column due to its own weight. Deriving afresh [DANIEL BERNOULLI's] formula (90), written in the form

(424) $$\mathcal{B}y'' + Py + g\sigma\int_0^x x\,dy = 0\,,$$

EULER puts $\xi = 1 + \dfrac{g\sigma x}{P}$, $y' = e^{\int v\,dx}$, and obtains

(425) $$\frac{dv}{d\xi} + v^2 + \frac{P^3}{(g\sigma)^2\mathcal{B}}\,\xi = 0\,,$$

XXXIX—XLII [but the result of his attempt to solve this equation approximately is faulty[1])].

1) EULER supposes that $P/W >> 1$, where $W = g\sigma l$ is the weight of the column. Setting $m \equiv W/P$, he takes $1 + \frac{mx}{l} \approx \left(1 - \frac{mx}{4}\right)^{-4}$ so as to effect the integration. A long approximate

Returning to the problem of buckling of weightless columns supporting a terminal load, EULER concludes the paper by discussing the scaling laws for tests by models made of the same material. Supposing that $\mathcal{B} \propto d^4$ for cylindrical columns (see below, pp. 403—404), from $(185)_2$ we have[1]) XLIV—XLVII

$$P_c \propto \frac{d^4}{l^2}\,. \tag{426}$$

52. The distinction of different kinds of buckling by DANIEL & JOHN III BERNOULLI (1766). While nothing concerning elastic curves was published in the following decade, valuable work on several aspects of elasticity was being done in Basel by DANIEL BERNOULLI in collaboration with his nephew, JOHN III BERNOULLI. This work is known, unfortunately, only through the unpublished correspondence[2]) between the two after JOHN III BERNOULLI arrived in Berlin in 1763 and through the papers he later published, with the encouragement of his uncle, in his own name alone. From the correspondence it appears that most if not all of the results of any importance in these papers are due to DANIEL BERNOULLI. Most of this work will be discussed in § 57, below.

The second of JOHN III BERNOULLI's papers, *Problems on the force and curvature of elastic bands*[3]), concerns only small deflection but gives the first solution of an important

---

calculation based on this dubious device leads to

$$P_c = \pi^2 \frac{\mathcal{B}}{l^2} - \frac{\pi^2 - 8}{2\pi^2} W\,. \tag{A}$$

This is not correct. First, (424) itself, for reasons we shall learn below (pp. 359—365), is not the correct differential equation of the problem, since it applies only when the top is free to move laterally. For this case, (A) is to be replaced, approximately, by

$$P_c = \pi^2 \frac{\mathcal{B}}{l^2} - \tfrac{1}{3} W\,, \tag{B}$$

as is suggested by EULER's later researches (below, p. 364) and is borne out by a result of N. GRISHCOFF, cited by TIMOSHENKO in § 23 of his *Theory of elastic stability*, New York and London, McGraw-Hill, 1936. When the top is pinned, an approximate result given by TIMOSHENKO (*loc. cit.*) is

$$P_c = \pi^2 \frac{\mathcal{B}}{l^2} - \tfrac{1}{2} W\,, \tag{C}$$

while the numerical factor in EULER's formula (A) is $\frac{\pi^2 - 8}{2\pi^2} \approx \frac{1}{10}$.

1) *Cf.* MUSSCHENBROEK's experimental law (94) for rectangular prisms and the criticism of it in footnote 3, p. 153.

2) Originals in the Basel University Library.

3) "*Sur la cohérence des corps, second mémoire. Problèmes sur la force et la courbure des lames élastiques,*" Hist. acad. sci. Berlin [22] (1766), 99—107 (1768). The paper was read in 1766.

In his first letter to JOHN III BERNOULLI, dated 7 December 1763, DANIEL BERNOULLI writes, "As for the force of beams, there one must speak less positively . . . The theorem on the relation that holds between the force of a vertical beam sustaining a weight and that of the same beam clamped horizontally in a wall deserves much attention, and the theory will agree with the experiments if proper care is taken." In a letter of June 1766 he mentions the subject again.

6—7 simple problem. For a uniform horizontal band of weight W, clamped at the end $x = 0$ and loaded at the end $x = l$ by a weight P, the differential equation is

$$\mathcal{B} y'' = P(l - x) + \tfrac{1}{2} W \frac{(l - x)^2}{l} . \tag{427}$$

Hence

$$\mathcal{B} y = \frac{1}{24} W \frac{(l - x)^4}{l} + \tfrac{1}{6} P (l - x)^3 + \tfrac{1}{2}(P + \tfrac{1}{3} W)\, l^2 x - \tfrac{1}{6}(P + \tfrac{1}{4} W) l^3 . \tag{428}$$

The deflection δ at the end $x = l$ is given by

$$\delta = \left(\tfrac{1}{3} P + \frac{1}{\delta} W\right) \frac{l^3}{\mathcal{B}} , \tag{429}$$

1 and this may be used to eliminate $\mathcal{B}$. For example, if $W = 0$, we have

$$\frac{y}{\delta} = \tfrac{3}{2}\left(\frac{x}{l}\right)^2 - \tfrac{1}{2}\left(\frac{x}{l}\right)^3 , \tag{430}$$

a formula which expresses the shape of the loaded beam without reference to the load acting or to the elastic properties of the material. [These results generalize (159) and (160).]

10—16 The last part of the paper discusses buckling in compression; [EULER's work is not cited, and JOHN III BERNOULLI's derivation is clumsy and ill explained,] but his figures (Figure 97) show that he distinguishes two kinds
10—11 of buckling. [At bottom, BERNOULLI's argument is the same as that EULER had used to derive (179);] his contribution is to observe that when the top of the column is free to move laterally, (179) applies with $f = l$. That is,

$$P_c = \tfrac{1}{4} \pi^2 \frac{\mathcal{B}}{l^2} \tag{431}$$

14 for this case. Since the amplitude is not involved in (431), "... thus it will always happen that for any weight P considerably too small to break the spring, in virtue of its elasticity this band will immediately reestablish itself in the vertical." Further discussion, mentioning the "latitude of equilibrium", indicates that BERNOULLI knows a spring can assume a bent form without breaking, though most of his remarks rest on the assumption
16 that a column breaks when its critical load is reached. "If the length of the spring is $2l$, but it is not fixed except by supporting it [*i. e.*, if it is pinned at each end,] it can support the same weight as the spring previously considered ...," since each half is a spring of

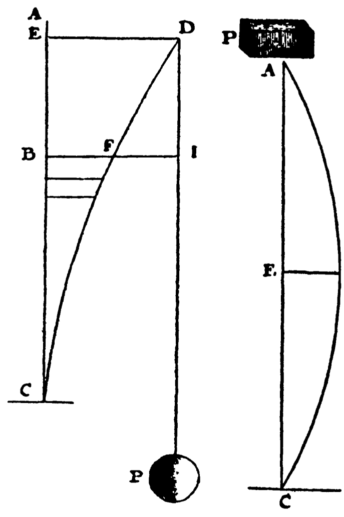

Figure 97. The two kinds of buckling distinguished by JOHN III BERNOULLI (1766)

length l subject to the conditions of the previous problem. Writing l for the entire length yields $(185)_2$.

[Thus JOHN III BERNOULLI explains, though not very clearly, how different buckling loads are appropriate to different end conditions. His argument does not sufficiently emphasize the fact that EULER's formula $(180)_3$ applies *always;* the different cases are distinguished only by the relation of the length l of the column to the parameter f, which, for all, is the length of the quarter period of the bent form. Indeed, as EULER is to remark later (below, p. 362), if both ends of the band are clamped in the same vertical, we must take $l = 4f$ in (180), yielding

$$P_c = 4\pi^2 \frac{\mathcal{B}}{l^2} \,. \tag{432}$$

These arguments rest only upon the symmetries of the elastic curve; they apply not only to buckling but to all deflection problems. In this more general context they were made long before by JAMES BERNOULLI (*cf.* his Remark 3, p. 93, above); in connection with rupture, by GALILEO (above, pp. 40—41); and they were repeated, in more definite form, by DANIEL BERNOULLI in his letter of criticism of this paper by his nephew[1]); his drawings are shown in Figure 98. It is a pity JOHN III BERNOULLI did not see fit to publish] his uncle's clear argument to show that *if a load P produces a certain deflection when applied at the free end of a clamped band, then a load 4P produces the same deflection when applied at the middle of band pinned at each end, while a load 8P produces the same deflection if applied at the middle of a band clamped at each end.*

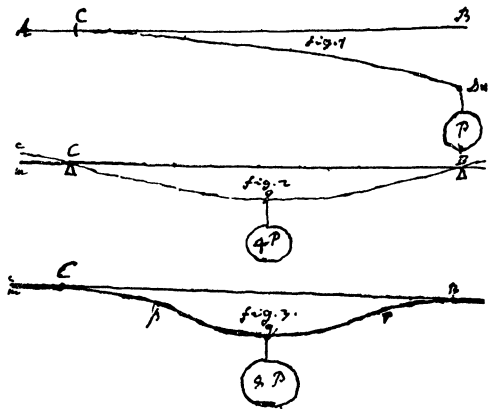

Figure 98. DANIEL BERNOULLI's sketch to show the three results that can be read off from any one solution for the elastica (1766)

53. LAGRANGE's two memoirs (1770, 1773). The problem of the elastica now became again a major issue. LAGRANGE's paper *On the force of bent springs*[2]) takes up an inverse

1) Universitätsbibliothek Basel MS L Ia 676, 45.

2) "*Sur la force des ressorts pliés,*" Mém. acad. sci. Berlin [25] (1769), 167—203 (1771) = Œuvres **3**, 77—110. Read 20 September 1770.

In his letter of 8 November 1771 to LAGRANGE, D'ALEMBERT raises numerous objections to points in this paper. LAGRANGE on 16 December admits a minor error pointed out by D'ALEMBERT (but left uncorrected in the reprint in LAGRANGE's *Œuvres*). D'ALEMBERT repeats in his letter of 26 February 1772 that since for the case of purely tangential load the exact solution, which is given in the footnote on our next page, does not allow a finite length to the rod, doubt is cast upon LAGRANGE's linearized theory supposedly appropriate to just this case. LAGRANGE in his letter of 24 Febru-

problem suggested by the design of the spiral spring of a watch, [which had been discussed unsuccessfully by JAMES BERNOULLI (above, Note 1, p. 105)]: One end of a spring of given length being pinned, find the force which is sufficient when applied at the other end
II to bring that end to a specified point. Taking the origin at the end where the force acts, LAGRANGE derives the appropriate special case of (91); he introduces the slope angle θ and thus expresses [EULER's solution (172)] in the equivalent form

$$
(433) \qquad
\begin{aligned}
s(\theta) &= \sqrt{\tfrac{1}{2}\mathcal{B}} \int_0^\theta \frac{d\xi}{\sqrt{P_x(1-\cos\xi) - P_y \sin\xi}} \,, \\
y(\theta) &= \sqrt{\tfrac{1}{2}\mathcal{B}} \int_0^\theta \frac{\sin\xi\, d\xi}{\sqrt{P_x(1-\cos\xi) - P_y \sin\xi}} \,, \\
x(\theta) &= \sqrt{\tfrac{1}{2}\mathcal{B}} \int_0^\theta \frac{\cos\xi\, d\xi}{\sqrt{P_x(1-\cos\xi) - P_y \sin\xi}} \,,
\end{aligned}
$$

where θ is taken as 0 at the origin. If $\theta = \varrho$ at the end $s = l$ where $x = X$, $y = Y$, then (433) gives three equations for the three unknowns ϱ,
IV P_x, P_y in terms of $l = s(\varrho)$, $Y = y(\varrho)$, and $X = x(\varrho)$. If the load P_x, P_y is resolved into forces R and T along and normal to the chord between the two ends (Figure 99), and if $R = p\cos q$, $T = p\sin q$, then

$$
(434) \qquad
\begin{aligned}
P_x &= p\cos(q + \alpha - \varrho), \quad X = r\cos(\varrho - \alpha) \,, \\
P_y &= p\sin(q + \alpha - \varrho), \quad Y = r\sin(\varrho - \alpha) \,.
\end{aligned}
$$

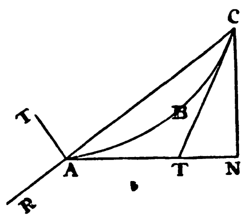

Figure 99. LAGRANGE's diagram for the elastica (1770)

III, V Abandoning the exact problem[1]), LAGRANGE takes α, ϱ, and q as small quantities of

ary is unable to find an effective defense; on 25 March D'ALEMBERT repeats the objection; on 19 April 1772 LAGRANGE replies that he will "speak about the theory of springs on another occasion," but when on 25 September 1776 LAGRANGE finally comes back to the matter, he contents himself with agreeing that the straight line is included as a possible exact solution when the load is tangential. On 14 February 1777 D'ALEMBERT replies with polite formulae to break off the discussion, remarking only that he still sees "some clouds over this theory".

1) Not mentioning EULER's result (182), LAGRANGE in § III obtains the exact integral of (433) when $P_y = 0$:

$$
\begin{aligned}
s &= \sqrt{\frac{\mathcal{B}}{P_x}} \log\tan\tfrac{1}{2}\theta + A \,, \\
y &= \sqrt{\frac{\mathcal{B}}{P_x}} \sin\tfrac{1}{2}\theta + B \,, \\
x &= s + 2\sqrt{\frac{\mathcal{B}}{P_x}} \cos\tfrac{1}{2}\theta + C \,.
\end{aligned}
$$

the same order and by approximate evaluation of (433) obtains

$$\frac{q}{\alpha} = \frac{\sin\omega}{\omega\cos\omega - \sin\omega},$$

(435) $$\frac{\varrho}{\alpha} = \frac{\omega(\cos\omega - 1)}{\omega\cos\omega - \sin\omega},$$

$$\frac{\frac{l}{r} - 1}{\alpha^2} = \frac{(\sin\omega - \omega)^2 + \frac{1}{4}\omega\sin 2\omega - 2\omega\sin\omega + \frac{3}{2}\omega^2}{2(\omega\cos\omega - \sin\omega)^2},$$

where $\omega \equiv l\sqrt{-P_x/\mathcal{B}}$, the load P_x being assumed negative. When l and r are given, ω is determined by $(435)_3$ as a function of α^2; this result put into $(435)_{1,2}$ determines q and m; since $p \approx -P_x = -\mathcal{B}\omega^2/l^2$, the problem is solved. Since $R \approx p$ and $T \approx pq$, we have

(436) $$R = -\frac{\mathcal{B}\omega^2}{l^2}, \quad T = -\frac{\mathcal{B}\omega^2}{l^2}q.$$

If $T = 0$, so that the load acts along the chord, then $q = 0$, and hence $\sin\omega = 0$, but $\omega\cos\omega - \sin\omega \neq 0$; therefore $\omega = m\pi$, $m > 1$, and

(437) $$R = -m^2\pi^2\frac{\mathcal{B}}{l^2}, \quad m = 1, 2, 3, \ldots$$

These are the only possible loads acting along the chord that can equilibrate the band. In particular, this result yields EULER's buckling formula (185), since the smallest possible chordal load is given by putting $m = 1$. Putting $\omega = \pi - t$, LAGRANGE effects the solution of (435) when t is small, so constructing an approximate theory of nearly straight columns subject to nearly tangential end loading; [later writers have found errors in the analysis [1])]. He applies it to a discussion of the balance wheel, concluding that any leaf spring bent but VII slightly exerts a force proportional to the arc through which its end is displaced and thus will produce isochronous oscillation of the balance wheel, "which no one, so far as I know, has yet proved with all rigor." [It is scarcely necessary to comment on so trivial a conclusion to pages of calculation [2]).]

When there is a couple L, visualized by [JAMES BERNOULLI's] device of a force acting X—XI

Since, as EULER had observed long ago, this curve cannot pass through θ and be of finite length unless it is a straight line, LAGRANGE decides that the angle θ must be infinitely small. Another attempt in §§ VIII—IX results only in formulae "too complicated to yield any enlightenment". A numerical error here has been corrected by the editors of LAGRANGE's Œuvres.

1) According to TODHUNTER, §§ 153—154 of *op. cit. ante*, p. 11, there are two errors which invalidate all the results, and the first of these was noticed by PLANA in a work, dated 1809, where a presumably correct linearized theory is obtained.

2) PEARSON, § 103 of *op. cit. ante*, p. 11, not noticing the remarks of TODHUNTER cited in the foregoing footnote, describes it as "this elegant property".

at the end of a rigid rod, the quantity $\frac{1}{2}L^2/\mathcal{B}$ should be added under the radicals in (433).
XII If $P=0$, the band is circular, [as EULER had shown long ago]; assuming $P/(\frac{1}{2}L^2/\mathcal{B})$ to be small, LAGRANGE linearizes (433). Putting $R \equiv \mathcal{B}/L$, $T \equiv \mathcal{B}P_x/L^2$, $V \equiv \mathcal{B}P_y/L^2$, he thus obtains

$$(438)\qquad \begin{aligned} s &= R\,[\Phi - T(\Phi - \sin\Phi) - V(1-\cos\Phi)]\ , \\ y &= R\,[1-\cos\Phi - T(\tfrac{3}{4} - \cos\Phi + \tfrac{1}{2}\cos 2\Phi) - V(\tfrac{1}{2}\Phi - \tfrac{1}{4}\sin 2\Phi)]\ , \\ x &= R\,[\sin\Phi + T(\tfrac{1}{2}\Phi - \sin\Phi + \tfrac{1}{2}\sin 2\Phi) - V(\tfrac{1}{4} - \tfrac{1}{4}\cos 2\Phi)]\ . \end{aligned}$$

XIII—XVI For this approximate theory, some further approximate calculations lead to a solution of the inverse problem set at the beginning of the paper.

[Thus LAGRANGE's paper, dense with calculations though it is, succeeds only in deriving two new linearized theories; one, incorrect in detail, for terminal load that is nearly tangential, the other for loading that differs little from a couple. Although more elaborate in tone, LAGRANGE's paper accomplishes less than the simple little note, which he does not cite, that was published three years before by his junior colleague, JOHN III BERNOULLI.]

LAGRANGE's next paper, *On the shape of columns*[1]), attacks a more important prob-
1—2 lem, [but with even less success. Referring to the entasis of the columns made in classical antiquity and copied in modern times, he proposes to determine *that figure of revolution which should be given to a column of given height and mass in order that it support the greatest*
4—6 *possible weight without bending*. [Repeating EULER's derivation] of (415) and its sinusoidal solution (178), LAGRANGE observes that the end conditions for a bent form are satisfied if and only if $P = -R$, where R is given by (437). Thus in order for a vertically loaded column to assume a bent form with $m-1$ nodes, it is necessary that the load be at least as great as $m^2\pi^2\mathcal{B}/l^2$. [LAGRANGE does not discuss the matter further.]

7—8 Turning to the exact theory, LAGRANGE [in effect] puts $x = c\sin\Phi$ in [EULER's] formula $(172)_2$ and obtains

$$(439)\qquad \frac{ds}{d\Phi} = \frac{a}{\sqrt{2}}\cdot\frac{1}{\sqrt{1-\frac{c^2}{2a^2}\cos^2\Phi}} = \frac{a}{\sqrt{2}}\left[1+\sum_{n=1}^{\infty}\frac{(2n-1)!!}{(2n)!!}\left(\frac{c^2\cos^2\Phi}{2a^2}\right)^n\right]\ ,$$

9 where $P = 2\mathcal{B}/a^2$. Integration from 0 to $m\pi$ yields[2]) [EULER's] formula $(175)_1$ with $l = 2mf$, where l is the whole length of the band, assumed to be bent into a form with
10 $m-1$ nodes. Hence $l > \dfrac{m\pi a}{\sqrt{2}}$; equivalently, for the form with $m=1$ nodes it is neces-

1) "*Sur la figure des colonnes*," Misc. Taurin. 5_2 (1770/1773), 123—166 = Œuvres 2, 125—170.

2) PEARSON, § 110 of *op. cit. ante*, p. 11, notes that LAGRANGE makes a slip here, but he does not note that the result was given correctly by EULER long before.

sary, according to the exact theory, that

$$P > m^2\pi^2 \frac{\mathcal{B}}{l^2} \,. \tag{440}$$

"Hence it follows that if $P < \pi^2\mathcal{B}/l^2$, the column cannot be curved; if

$$\pi^2\mathcal{B}/l^2 < P < 4\pi^2\mathcal{B}/l^2 \,,$$

the column will be curved but will have only one loop; if $4\pi^2\mathcal{B}/l^2 < P < 9\pi^2\mathcal{B}/l^2$, the column will necessarily be curved but may form either one or two loops; *etc.*"

[While the existence of this sequence of critical loads and this multiplicity of possible bent forms are obvious from the periodicity of the bent forms which had been proved long ago by EULER (above, p. 206), EULER did not infer them, and LAGRANGE deserves credit for being the first to remark this *bifurcation of equilibrium*. We notice that he carefully avoids any conjecture as to which one of the possible bent forms will actually be assumed.]

LAGRANGE claims that since the critical load as calculated from the linearized theory 11
has been proved to be the exact critical load for the uniform band, this agreement will hold for non-uniform bands also. We are to integrate (415). LAGRANGE seeks solutions of 12—13
the form $y = \xi \sin \Phi$, [as indeed is most natural in view of EULER's solutions (419) and (422)]; (415) will be satisfied if

$$\mathcal{B}(\xi'' - \xi\Phi'') + P\xi = 0, \quad 2\Phi'\xi' - \xi\Phi'' = 0 \,. \tag{441}$$

Integrating the second of these equations, substituting the result into the first, and rearranging, LAGRANGE shows that a solution of (415) is given by

$$y = \sqrt{hu}\,\sin\int_{x_0}^{x}\frac{dx}{u} \,, \tag{442}$$

where h is a constant of integration and u is chosen so as to satisfy

$$4Pu^2 + \mathcal{B}(2uu'' - u'^2 - 4) = 0 \,. \tag{443}$$

Since (442) contains two constants of integration, x_0 and h, we need not solve (443) in 14
general but may rest content with any particular solution. Taking $x_0 = 0$, we have a form of length l with $m - 1$ nodes if

$$\int_0^l \frac{dx}{u} = m\pi \,. \tag{444}$$

Since (443) contains no arbitrary constants, (444) is an equation for determining $P/\mathcal{B}(0)$ as a function of l and m. We should then select m so as to give the smallest value to $P/\mathcal{B}(0)$, "and this value will be the desired limit".

[After this interesting start toward the STURM-LIOUVILLE problem,] LAGRANGE 15—16
abandons the general theory and turns to "the simplest hypothesis", that the column has

the form of a conic of revolution, $d^2 = \alpha + \beta x + \gamma x^2$; taking $\mathcal{B} \propto d^4$, because "theory and experiment agree sufficiently . . ., as one can see in the works where this subject is treated" (see below, pp. 403—404), LAGRANGE puts

$$\mathcal{B} = \mathcal{B}_0(\alpha + \beta x + \gamma x^2)^2 \ . \tag{445}$$

A particular solution of (443) for this case is

$$u = g(\alpha + \beta x + \beta x^2)^2 \,, \quad \text{where} \quad g = \frac{1}{\sqrt{P/\mathcal{B}_0 - \alpha\gamma - \frac{1}{4}\beta^2}} \ . \tag{446}$$

17 Putting $A \equiv \int_0^l (\alpha + \beta x + \gamma x^2)^{-1} dx$, from (444) we obtain $A/g = m\pi$, so that the critical load is given by

$$P_c = \left(\frac{m^2\pi^2}{A^2} + \tfrac{1}{4}\beta^2 - \alpha\gamma\right)\mathcal{B}_0 \ , \tag{447}$$

with $m = 1$ giving the least critical load. [When $\gamma = 0$ and $m = 1$, this reduces to
18 EULER's formula (423).] If $\beta^2 = 4\alpha\gamma$, the profile is a straight line, and the column is conical. LAGRANGE shows that (447) with $m = 1$ then takes the form (421). He calculates the ratio of P_c to the square of the volume of the column and finds that it is a maximum when $d_1 = d_2$; thus he concludes that among all conical columns, the cylindrical one is strongest.

19 More generally, he takes P_c/V^2 as the measure of the "relative force" of a column.
20—26 Considering various cases, he concludes that among all columns such that (445) holds,
27 the cylinder is the strongest. He then gives a definite formulation to the problem he set at the beginning, "a problem of a rather new kind, the solution of which requires some special
28 devices that may be useful to me on other occasions" : To find a curve $z = z(x)$ such that P_c/V^2 is a maximum when P_c is given by the solution of (444) with $m = 1$, when u is
29—40 any solution of (443) with $\mathcal{B} \propto z^4$, and $V = \pi \int_0^l z^2 dx$. There follow pages of manipulations, from which LAGRANGE concludes that the strongest column is the cylindrical one. [J.-A. SERRET, the editor of LAGRANGE's works, notices that the analysis contains many errors, which he is able to correct only up to a certain point. However, it does not seem to have been remarked that LAGRANGE's formulation of the problem is not adequate. The quantity $P_c l^4/(EV^2)$ is dimensionless and is independent of the form of the column, but it is not clear that maximizing this quantity is equivalent to maximizing P_c when V and l are fixed[1]). The problem LAGRANGE seems to think he solves[2]) may be put as follows :

1) PEARSON, § 112 of *op. cit. ante*, p. 11, approves the use of P_c/V^2 as a measure of the efficiency of a column, since it "frees us from the indeterminateness which would otherwise arise from the possibility of infinitely increasing the magnitude of P_c by simply increasing the dimensions of the column."

2) PEARSON, § 113 of *op. cit. ante*, p. 11, oblivious of the mass of errors noted above, says that this paper by LAGRANGE "may fairly be said to have shaken the then current architectural fallacies,"

Among all columns of given length and volume, to find that one whose critical load is greatest. This problem has been solved subsequently[1]); the solution is not of uniform cross-section.

LAGRANGE's contribution to the theory of buckling has been exaggerated.]

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but what these fallacies were, he does not state. Since LAGRANGE on the basis of his faulty analysis concluded that the cylindrical column is the strongest, possibly PEARSON took him as a torch carrier for Victorian architectural practise, according to which, it seems, the ugliest forms turn out to be the most useful. For the later views of PEARSON, see the next footnote.

1) Apparently no one has ever really gone through LAGRANGE's calculations. The problem was reformulated by CLAUSEN, "*Über die Form architektonischer Säulen,*" Bull. cl. physico-math. acad. St. Pétersbourg 9, 369—380 (1851), as that of finding the form of given length and buckling load for which the volume is least. His analysis is somewhat simplified by PEARSON, §§ 477—479 of *A history of the theory of elasticity and of the strength of materials from GALILEI to the present time*, Vol. 2, Cambridge, 1893, but elements of mystery remain. Both CLAUSEN and PEARSON seem to think LAGRANGE merely went astray somewhere in the integration; both of them accept LAGRANGE's measure of efficiency. PEARSON cites a second memoir of CLAUSEN, which I have been unable to see, in Mélanges math. astron. St. Pétersbourg 1 (1849/53), 279—294 (1853). CLAUSEN draws a figure representing half of the optimum profile; its form, essentially that of a phallos, he regards as "eine dem Auge nicht ungefällige", but PEARSON disagrees. CLAUSEN asserts that among similarly situated similar cross-sections, the circle is not the best, but he gives no analysis; PEARSON concludes that certain rectangular sections are better than circular ones (§ 480).

H. F. WEINBERGER and J. KELLER have kindly shown me simple and irreproachable methods whereby the original problem of LAGRANGE can be solved directly, with no use made of his measure of efficiency. Both for a circular section and for a rectangular section of fixed breadth, suitable entasis increases strength, and the optimum profile is flat at the ends and similar in form to an arch of a cycloid.

For the circular section, the radius z is given by

$$\text{(I)} \qquad \frac{z}{l} = \sqrt{\tfrac{4}{3}K}\sin\theta\,, \quad \frac{x}{l} = \frac{1}{\pi}\left(\theta - \tfrac{1}{2}\sin 2\theta\right), \quad K \equiv \frac{V}{\pi l^3}\,.$$

The critical load,

$$P_c = \tfrac{2}{3}\pi\frac{EV^2}{l^4}\,,$$

is $\frac{4}{3}$ that for the uniform column of equal length and volume. As compared with the uniform column, the form (I) is thinner in the region between the end and parts not farther from the ends than the distance $l\left[\frac{1}{3} - \frac{\sqrt{3}}{4\pi}\right] \approx 0{,}1955\,l$, thicker elsewhere; the greatest diameter is $\sqrt{\frac{4}{3}}$ that of the corresponding uniform column.

For the rectangular section of fixed breadth B, the half depth $\frac{1}{2}D$ is given by

$$\text{(II)} \qquad \frac{\frac{1}{2}D}{l} = \tfrac{5}{4}K'\sin^2\theta\,, \quad \frac{x}{l} = \tfrac{3}{4}\left[\tfrac{2}{3} - \cos\theta + \tfrac{1}{3}\cos^3\theta\right], \quad K' \equiv \frac{V}{2Bl^2}\,.$$

The critical load is

$$P_c = \frac{125}{108}\,\frac{EV^3}{B^2 l^5}\,,$$

greater than that for the uniform rectangular column in the ratio $\frac{125}{9\pi^2} \approx 1{,}407$. As compared with the uniform column, the form (II) is thinner in parts not farther from the ends than the distance $\frac{1}{2}l\left(1 - \frac{7}{25}\sqrt{5}\right) \approx 0{,}187\,l$; the greatest diameter is $\frac{5}{4}$ that of the corresponding uniform rectangular column.

54. EULER's determination of the height at which a heavy column buckles (1776—1778).
Before describing the more important works which followed it, we summarize EULER's
paper entitled, *On the wonderful properties of the elastic curve contained in the equation*
$y = \int x^2dx/\sqrt{1-x^4}$[1]), written by 1775 but not published until after his death; it is
6 devoted to accurate calculation of the rectangular elastica. EULER begins by obtaining
[JAMES BERNOULLI's] series (50) and (51) for the rectangular elastica, but he considers
7—8 them too slowly convergent to be useful. Then he shows that

$$\begin{aligned}
\frac{y(x)}{c\sqrt{1-\left(\frac{x}{c}\right)^4}} &= \sum_{n=0}^{\infty}\frac{(4n+1)(4n-3)\cdots 1}{(4n+3)(4n-1)\cdots 3}\left(\frac{x}{c}\right)^{4n+3}, \\
\frac{s(x)}{c\sqrt{1-\left(\frac{x}{c}\right)^4}} &= \frac{x}{c}+\sum_{n=1}^{\infty}\frac{(4n-1)(4n-5)\cdots 3}{(4n+1)(4n-3)\cdots 1}\left(\frac{x}{c}\right)^{4n+1},
\end{aligned} \tag{448}$$

9—10 but these cannot be used when $x = c$. Another expansion leads to

$$\begin{aligned}
\frac{y(c)}{c} &= \tfrac{1}{2}\pi\left\{\tfrac{1}{2}+\sum_{n=1}^{\infty}(-1)^n\left[\frac{(2n-1)!!}{(2n)!!}\right]^2\frac{2n+1}{2n+2}\right\}, \\
\frac{s(c)}{c} &= \tfrac{1}{2}\pi\left\{1+\sum_{n=1}^{\infty}(-1)^n\left[\frac{(2n-1)!!}{(2n)!!}\right]^2\right\}.
\end{aligned} \tag{449}$$

11—12 A recurrence formula shows that

$$\begin{aligned}
\frac{y(c)}{c} &= \int_0^1\frac{dx}{\sqrt{1-x^4}} = \prod_{n=1}^{\infty}\frac{4n-1}{4n-3}\int_0^1\frac{x^{1+\infty}dx}{\sqrt{1-x^4}}, \\
\frac{\pi}{4} &= \int_0^1\frac{x\,dx}{\sqrt{1-x^4}} = \prod_{n=1}^{\infty}\frac{4n}{4n-2}\int_0^1\frac{x^{1+\infty}dx}{\sqrt{1-x^4}}, \\
\frac{s(c)}{c} &= \int_0^1\frac{x^2dx}{\sqrt{1-x^4}} = \prod_{n=1}^{\infty}\frac{4n+1}{4n-1}\int_0^1\frac{x^{1+\infty}dx}{\sqrt{1-x^4}}, \\
\tfrac{1}{2} &= \int_0^1\frac{x^3dx}{\sqrt{1-x^4}} = \prod_{n=1}^{\infty}\frac{4n+2}{4n}\int_0^1\frac{x^{1+\infty}dx}{\sqrt{1-x^4}}.
\end{aligned} \tag{450}$$

13 EULER asserts that the four integrals indicated on the right are all equal and hence may
be divided out between any pair of the four formulae (450). [The inference is not strict
14 but the results are correct.] Among them are

The problem in its most general form has been solved definitively by KELLER, "*The shape of the strongest column*," Arch. Rational Mech. Anal. 5, 275—285 (1960). He shows that twisting cannot increase strength; that the best cross-section is an equilateral triangle; that combination of this form with entasis as above increases the strength by 61,2% over the uniformly circular column.

1) E 605, "*De miris proprietatibus curvae elasticae sub aequatione* $y = \int\frac{xxdx}{\sqrt{1-x^4}}$ *contentae*," Acta acad. sci. Petrop. 1782_2, 34—61 (1786) = Opera omnia I **21**, 91—118. Presentation date: 4 September 1775.

$$(451)\quad \begin{aligned} \frac{4y(c)}{\pi c} &= \prod_{n=1}^{\infty} \frac{(4n-1)(4n-2)}{(4n)(4n-3)} = \prod_{n=1}^{\infty}\left[1+\frac{1}{2n(4n-3)}\right], \\ \frac{2s(c)}{c} &= \prod_{n=1}^{\infty} \frac{(4n)(4n+1)}{(4n-1)(4n+2)} = \prod_{n=1}^{\infty}\left[1+\frac{1}{(2n+1)(4n-1)}\right]; \end{aligned}$$

these, too, he considers of little use for calculation.

Going back to the alternating series (449), EULER decides to sum them by his method 15—20
of differences[1]). His results are $s(c)/c = \pi \cdot 0{,}417314 = 1{,}311031$, $y(c)/c = \pi \cdot 0{,}190687 = 0{,}599061$; [*cf.* JAMES BERNOULLI's estimates (52) and EULER's own earlier result (p. 208)].

By multiplying together $(451)_1$ and $(451)_2$, EULER's old result (141) follows[2]). Then 20—25
he gives another proof based on the transformation

$$(452)\quad \begin{aligned} sy &= \int (y\,ds + s\,dy)\,, \\ &= \int \frac{y + s x^2/c^2}{\sqrt{1-\frac{x^2}{c^2}}}\,dx\,, \end{aligned}$$

where y and s are given by the series (51).

EULER then proves a remarkable property of the rectangular elastica. First he recalls 26
the addition theorem for elliptic functions that he had discovered earlier[3]) : If

$$(453)\quad \begin{aligned} H(\omega) &\equiv \int_0^{\omega} \frac{(\alpha + \beta\xi^2)\,d\xi}{\sqrt{1+m\xi^2+n\xi^4}}\,, \\ z &\equiv \frac{x\sqrt{1+my^2+ny^4} + y\sqrt{1+mx^2+nx^4}}{1-nx^2y^2} \end{aligned}$$

then

$$(454)\quad H(z) = H(x) + H(y) + \beta xyz\,.$$

The quadratures defining $s(x)$ and $y(x)$ are both of the form $(453)_1$, so the addition theorem 28—35

1) See §§ 8—12 *et seqq.* of Ch. I of Part II of E 212, *Institutiones calculi differentialis*, Petrop., 1755 = Opera omnia I 10. While the modern literature often asserts that EULER neglected questions of convergence, both his explicit statements and his examples show that he regarded this transformation as sometimes useful in hastening the convergence of a convergent series, in transforming a divergent series into a convergent one, or in transforming "a very divergent series . . . into a more convergent one, which, though still not sufficiently convergent, in the same way may be converted into a more convergent one" (§ 10). The case mentioned in the text above is a striking example of the first kind.

2) This is essentially the proof EULER had published in E 122, cited in footnote 2, p. 174, but here it is easier to follow.

3) See § 29 of E 581, "*Plenior explicatio circa comparationem quantitatum in formula integrali* $\int \frac{Z\,dz}{\sqrt{1+mzz+nz^4}}$ *contentarum denotante Z functionem quancunque rationalem ipsius zz*," Acta acad. sci. Petrop. 1781_2, 3—22 (1785) = Opera omnia I 21, 39—56. Presentation date: 14 August 1775.

(454) may be applied to each. The result is as follows : Given two points distant s_1 and s_2 from the end of the elastica, with abscissae x_1 and x_2, then at the point where

$$s = s_1 + s_2 \tag{455}$$

we have

$$xc = \frac{\frac{x_1}{c}\sqrt{1-\left(\frac{x_2}{c}\right)^2} + \frac{x_2}{c}\sqrt{1-\left(\frac{x_1}{c}\right)^2}}{1+\left(\frac{x_1 x_2}{c^2}\right)^2} \; . \tag{456}$$

Also

$$y = y_1 + y_2 + \frac{x_1 x_2 x}{c^2} \; . \tag{457}$$

37 The proof is achieved by simple substitutions in (453) and (454). The special case when $x = c$ yields

$$\left(\frac{x_2}{c}\right)^2 = \frac{1-\left(\frac{x_1}{c}\right)^2}{1+\left(\frac{x_1}{c}\right)^2} \; , \tag{458}$$

$$s(c) = s(x_1) + s(x_2) \; ,$$

$$y(c) = y(x_1) + y(x_2) + \frac{x_1 x_2}{c} \; .$$

38—39 Since x_1/c and x_2/c are both smaller than 1, the series (51) will converge much more rapidly than (50), and both $s(c)$ and $y(c)$ may be calculated more easily. EULER selects $(x_1/c)^2 = \frac{1}{2}$,
40—43 $(x_2/c)^2 = \frac{1}{3}$ and writes down the resulting series. The rest of the paper gives the solution to the following problem : Given three points whose distances from the end of the rectangular elastica are s_1, s_2, and s_3, to find the abscissa of a fourth point at s such that
44 $s - s_3 = s_2 - s_1$. The value of $x(s_3)$ is exhibited as a rational function of $x(s_1)$, $x(s_2)$, and $x(s_3)$. EULER remarks that since both the quadratures (170) can be reduced to special cases of $(453)_1$, corresponding results may be obtained for the general elastic curve.

EULER's *Determination of the weights that columns may bear*[1]) concerns the buckling

1) E 508, "*Determinatio onerum, quae columnae gestare valent,*" Acta acad. sci. Petrop. **2** (1778), 121—145 (1780). Presentation date: 16 December 1776 = Opera omnia II **17**.

In §§ 1—2 occurs what is, so far as I know, the unique case in which EULER enters a claim for priority: In regard to his discovery of the phenomenon of buckling, he writes, "It seemed to me . . not only entirely new but also most remarkable . . . Therefore in recently leafing over the very famous French Encyclopaedia I was not a little astonished to find my result brought forward fluently in the midst of the article on columns as if commonly known . . .; nor is any other author cited as confirming this result either by experiment or by theory." In fact, EULER is unjust. While we might expect that he is replying to one of the vicious attacks D'ALEMBERT put into the Encyclopaedia (*cf.* above, pp. 245, 262, 311), this is not the case. The article "Colonnes" (Ency. **3**, 1753), signed by the Chevalier

of a heavy vertical column. The equation to be solved is (424) with $P = 0$. Writing 30—32
$m \equiv \mathcal{B}/\sigma g$, EULER shows that the only power series solution is 33—35

$$(459) \qquad \frac{y}{A} = \frac{x}{m^{1/3}} + \sum_{k=1}^{\infty} (-1)^k \frac{(3k-2)!!}{(3k+1)!!} \left(\frac{x}{m^{1/3}}\right)^{3k+1},$$

where the end $x = 0$ is the top. [In the notation of BESSEL functions this solution may be written as

$$(460) \qquad \frac{y}{m^{1/3}\,\mathfrak{a}} = \int_0^{\frac{2}{3}\sqrt{\frac{x^3}{m}}} J_{-1/3}(v)\,dv\,,$$

where $\mathfrak{a}$ is a dimensionless constant.] The length l at which the column will buckle is then given by $l = (mv)^{1/3}$, where v is the smallest positive root of the equation

$$(461) \qquad 0 = 1 + \sum_{k=1}^{\infty} (-1)^k \frac{(3k-2)!!}{(3k+1)!!}\, v^k\,.$$

"It is plain that this series converges emphatically, however great is v." EULER asserts 36—37
that the sum of the series is always positive if $v < 24$; then, putting $v = 6u$, he attempts
to determine by successive approximation the root of the series in u. Obtaining for u the 37—38
sequence of values 4, 7, 11, 20, —23, he says that these "converge to no certain end" and concludes that (461) "plainly has no real root." Trying $u = 10$, he shows that the sum
of the series on the right in (461) is $0{,}1577663 \pm 0{,}0000012$. He decides that these results 39
constitute "a most remarkable paradox, namely, that cylindrical columns, no matter to what height they are erected, never fall from their own weight," but, seeing the unreasonableness of such a conclusion, he says "all these matters call for a more accurate examination, which we shall begin in the following paper."

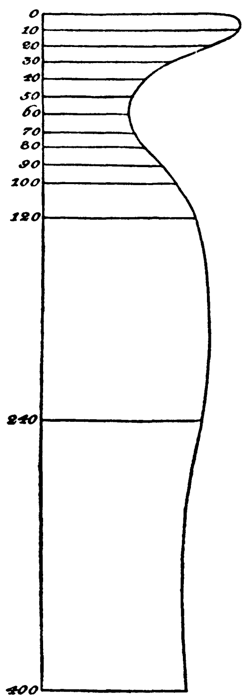
The immediately following paper is EULER's *Examination of a remarkable paradox in*
the theory of columns[1]). EULER decides first that the "paradox" arose not from mechanics 4

DE JAUCOURT, cites some specific experimental results of MUSSCHENBROEK on the collapse of columns; while no author is cited when the rule $P_c \propto 1/l^2$ is stated a little further on, the reader would naturally connect it with MUSSCHENBROEK, who indeed obtained the more definite rule (94). This makes EULER's claim all the stranger, since in §§ 26—28 he cites some of MUSSCHENBROEK's experimental data from the very passage where (94) is inferred.

1) E 509, "*Examen insignis paradoxi in theoria columnarum occurrentis,*" Acta acad. sci. Petrop. 2 (1778), 146—162 (1780). Presentation date: 22 January 1778 = Opera omnia II 17.

EULER's work on columns must have been called to DANIEL BERNOULLI's attention at this time, for on 18 March 1778 he wrote to FUSS, "I recall having examined this matter some fifteen years ago [*i. e.*, in 1763 (*cf.* above, p. 347, footnote 3)] and having subjected my results to experiments which confirmed my theory well enough, except for those I made on the strength of vertical columns, where I was but middlingly satisfied. Could you not engage Mr. KOULIBINE to test Mr. EULER's theory by similar experiments, without which it will be only hypothetically true. I did my experiments with

Figure 100. EULER's drawing for the form of a heavy vertical column with free top (1778). The vertical coordinate is the cube root of the height, measured downward; thus the drawing is much distorted from the actual shape.



but from improper analysis, since "the method[1]) is . . . extremely slippery and very often can lead to error," as he indeed verifies in a simple case. "The reason for this imperfection is to be sought in the imaginary roots, and since our equation doubtless involves very many imaginary roots . . . , there is no wonder that the operation
5 failed of success." He decides to suspend judgement regarding the roots but to investigate instead the shape which a column loaded
6 only by its own weight assumes. Plotting the curve (459) should solve the problem, since if x increases to ∞ but y remains positive, this would certainly prove that a column does not buckle under its
7—8 own weight. By numerical summation of the series, EULER obtains the form shown in Figure 100, where the numbers are values of x^3/m, [so that the top part of the column is represented on a much
9—10 compressed scale]. EULER notices the many maxima and minima, including the rapid increase of y/A to its first maximum $y/A = 1{,}60$ at approximately $x^3/m = 8$. Carrying the calculation as far as
11—16 $x^3/m = 400$, he finds no indication that y ever vanishes. He then determines more accurately the values of x^3/m at which the extremes occur :

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parallelepipeds of dry and hard wood having several different lengths, but all of exactly the same base, of the same kind of wood and cut in the same direction, and on these I made very various tests." It is strange that BERNOULLI does not mention the work of MUSSCHENBROEK.

1) In § 3 EULER says that his method of "recurrent series" is the same as that he used "with happy event . . ." in the oscillatory motion of a chain, but I do not understand this reference. The method used by EULER in connection with the hanging rope rests upon obtaining expressions for the sum of the p^{th} powers of all the roots and has been explained above, p. 318. For it to be effective, the roots must decrease rapidly. The method of "recurrent series" used in E 509 takes account of only one root; while EULER does not explain it, his steps may be motivated by the following argument. To solve

$$1 = \sum_{m=1}^{\infty} A_m x^m ,$$

consider the approximation

$$(*) \qquad 1 = \sum_{m=1}^{n} A_m x^m .$$

For $n = 1$, we have $x = 1/A_1 \equiv x_1$, say. For larger values of n, replace x^m in (*) by $x_n x_{n-1} \ldots x_{n-m+1}$. Thus

$$x_n = \frac{1}{A_1 + A_2\, x_{n-1} + A_3\, x_{n-1}\, x_{n-2} + \cdots + A_n\, x_{n-1}\, x_{n-2} \cdots x_1} .$$

This is EULER's result. For it to be effective, A_1 should be large and the root x small.

7,84 Max.
56,10 Min.
149,59 Max.
288,31 Min.
472,26 Max.
701,44 Min.
975,85 Max.
1295,49 Min.

[The series whose roots EULER is finding we should now write as

$$\frac{dy}{dx} = \mathfrak{A} t^{\frac{1}{6}} J_{-\frac{1}{3}}\left(\tfrac{2}{3}\sqrt{t}\right) = 0\,, \tag{462}$$

where $t \equiv x^3/m$. Thus, in effect, he calculates the first eight roots[1]) of $J_{-\frac{1}{3}}(v) = 0$. The first three he obtains by trial, the remaining, by a method not presented in sufficient detail to be understood.]

Unable to draw any definite conclusion from the analysis, EULER attempts an explanation from the principles of mechanics. To this end, he returns to the problem of buckling of a weightless loaded column, but he now represents the load P as due to the weight of a rigid vertical column of length p superimposed upon the given column, made of the same material and having the same cross-sectional area. There results "almost a single column PAB . . . subject to its own weight" (Figure 101). "Since this column is almost disjointed at A, there is no doubt at all that such a column if continuous should be regarded as much stiffer." [*I.e.*, the buckling load for a continuous column forced to remain straight in the part PA will be at least as great as that for the column AB loaded at A by means of the superincumbent rigid column PA.] For the 17 18 19—21

Figure 101. EULER's device for bounding the buckling load of a heavy vertical column with pinned top (1778)

1) The values t_k of t are related to the roots v_k of $J_{-\frac{1}{3}}(v) = 0$ as follows: $t_k = \frac{9}{4} v_k^2$. From a modern table of $J_{-\frac{1}{3}}(v)$ I infer the following values of t_k:

7,8369
55,53
148,35
285,28
466,56
687,72
963,17
1277,71

comparison, however, we should regard the composite column as having continuous slope; since the upper part is vertical, the tangent to the lower part must also be vertical at A.
22 "For this purpose, let us conceive the upper end of the column AB to be fixed within a stationary panel, or so acted upon by horizontal forces that it cannot be bent out of a
23—24 vertical position." [We should now describe EULER's process as assuming that] a couple
25—26 $-Q\alpha$ acts at A so as to maintain the vertical tangent there. The differential equation is
27 now $Py - Q\alpha + \mathcal{B}y'' = 0$; the solution is $y = b\left(1 - \cos\frac{x}{c}\right)$, where $c^2 = \mathcal{B}/P$. If the length of AB is l, we must have $\frac{l}{c} = 2\pi$, and hence (432) follows. [While this result, the buckling load for a column whose ends are clamped in the same vertical, may be read off more simply from EULER's exact theory of thirty-five years previous (*cf.* above, p. 211), here we encounter its first recognition.]

28—29 Let p be the height of a column of weight W and of like material and cross-section as the column AB. Then $W = \sigma g p$, so that $p = \frac{4\pi^2\mathcal{B}}{l^2\sigma g}$; the total length h of the composite column is then given by

$$h = l + p = l + \frac{4\pi^2\mathcal{B}}{l^2\sigma g}\,. \tag{463}$$

[As EULER has said, a simple column subject to its own weight will always be weaker than the compound column considered here, since additional stabilizing horizontal forces are supplied at A. Hence the *least* value of h for which the compound column will buckle is not less than the buckling height for the column subject to its own weight.] For a given value of $\mathcal{B}/(\sigma g)$, the least possible value of h arises from taking $l^3 = \frac{8\pi^2\mathcal{B}}{\sigma g}$; *i. e.*

$$h_c = 3\sqrt[3]{\frac{\pi^2\mathcal{B}}{\sigma g}} \approx 6{,}5\sqrt[3]{\frac{\mathcal{B}}{\sigma g}}\,. \tag{464}$$

Thus the paradox is resolved, for a column of height h will surely not fail to break from its
30 own weight. If W is the weight of a column of length l and P is the buckling load for a weightless column of the same length pinned at each end, then by (464) and (185) we have

$$h_c = 3l\sqrt[3]{\frac{P}{W}}\,. \tag{465}$$

31 EULER conjectures that (464) corresponds to a root of (461); this would give $x^3/m = 27\pi^2 \approx 266$, [but this conjecture, scarcely compatible with his calculated data and figure, is false.] Since for columns of the same material we have $\sigma \propto d^2$ and $B \propto d^4$, where d is the thickness, (464) yields the "very remarkable theorem"

$$h_c \propto d^{\frac{2}{3}}\,. \tag{466}$$

There follows immediately EULER's paper *On the height of a column that collapses from its own weight*[1]). EULER writes that "the very remarkable curve, having innumerable maximum and minimum abscissae," which is given by (459) "led me so far astray as to conclude that a column may be made infinitely long without danger of breaking. Afterward, from different principles, I showed very clearly that the matter is otherwise ...
Since, however, the equation is derived from the most certain principles of equilibrium, 2
no error can be demonstrated if all circumstances ... are properly taken into account ... It was assumed that the top of the column ... is subject to the action of no force, so that it can move freely from its place ..., which circumstance is very different from the condition which ... we consider. For ... plainly we are supposing that the top and bottom are constantly held in the same vertical ... But ... if full freedom were given to the top, there would be nothing at all absurd in that remarkable curve ..."

Indeed, if we imagine the beam as clamped at any of the maximum or minimum 4
abscissae (Figure 100), we have a possible bent form for an initially vertical column subject to its own weight. [Though EULER does not go into detail, this observation of his, together with his earlier proof that the first maximum occurs at $x^3/m = 7{,}84$, implies that a column of weight W clamped at the bottom and free at the top will buckle when h reaches the value

$$h_c = \sqrt{7{,}84\,\frac{\mathcal{B}}{W}} = 2{,}80\sqrt{\frac{\mathcal{B}}{W}}\,; \tag{467}$$

this is to be compared with (431), which gives a numerical factor $\frac{1}{2}\pi \approx 1{,}57$ for a weightless column subject to vertical load W at its top. If we write $\mathcal{B} = EI = \alpha E A D^2$, where α is a numerical factor depending on the cross-section, and $\sigma = \varrho A$, then (467) becomes[2])

$$h_c = \sqrt[3]{\frac{7{,}84\alpha E D^2}{\varrho g}}\,. \tag{468}$$

Moreover, if we pin the band at a point of inflection in Figure 100, we again get a pos- 5
sible bent form for a column resting on a floor. [EULER does not determine the point of

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1) E 510, "*De altitudine columnarum sub proprio pondere corruentium*," Acta acad. sci. Petrop. 2 (1778), 163—193 (1780). Presentation date: 22 January 1778 = Opera omnia II 17.

2) PEARSON, § 910 of *op. cit. ante*, p. 11, fails to notice this passage. He compares solutions by HEIM (1838) and GREENHILL for this same problem with EULER's formula (481), failing to observe that it refers to a different problem. According to PEARSON, the numerical factor obtained by HEIM is 7,837325. I have been unable to see HEIM's work; the numerical result quoted is not accurate beyond the third decimal (*cf.* footnote 1, p. 361). The paper of A. G. GREENHILL, "*Determination of the greatest height consistent with stability that a vertical pole or mast can be made, and of the greatest height to which a tree of given proportions can grow*," Proc. Cambridge phil. soc. 4 (1880—1883), 65—73 (1881), in § I gives a treatment inferior to EULER's; for the smallest root of $J_{-\frac{1}{3}}(v) = 0$ GREENHILL obtains the value 1,88, which leads to the factor 7,95 in place of EULER's correct factor 7,84.

inflection exactly; from his numerical table we see that it occurs at approximately $x^3/m = 29$, so that

$$h_c \approx 5{,}4\sqrt{\frac{B}{W}}\,; \tag{469}$$

this is to be compared with (185), which gives a numerical factor π for a weightless column
6 subject to vertical load W at its top.] But none of these cases can be applied to the present problem, where we have to assign *a horizontal force sufficient to restrain the top to lie in the same vertical as the bottom.* The magnitude of this horizontal force is unknown and will have to be determined as part of the solution of the problem.

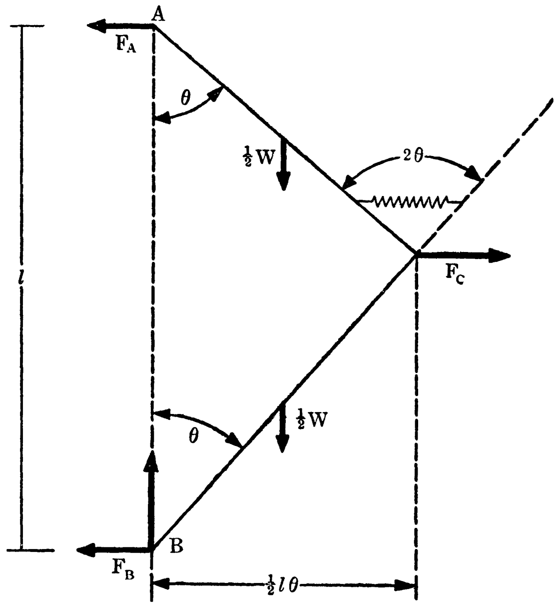

Figure 102. Modern sketch of EULER's framework model for the buckling of a heavy vertical column with pinned top (1778)

6—8 To clarify the question, EULER considers a model in which the column is represented as two rigid rods of weight $\frac{1}{2}W$ and of length $\frac{1}{2}l$, hinged together by an elastic joint exerting a moment proportional to the angle between them (Figure 102). Forces F_A, F_B, F_C are supplied as needed.
9—10 Equilibrium of forces and of moments about A yields $F_A + F_B = F_C$, $F_A =$
11 $\frac{1}{2}F_C + \frac{1}{4}W$, $F_B = \frac{1}{2}F_C - \frac{1}{4}W$. Since the torque of the spring is $K \cdot 2\theta$, equilibrium
12—13 of moments about C yields $\frac{1}{8}\,Wl\theta + \frac{1}{2}F_A l = 2K\theta$. Hence

$$F_C = \frac{8K - Wl}{l}\,\theta\,. \tag{470}$$

This force vanishes, for $\theta \neq 0$, if and only if

$$l = \frac{8K}{W}\,. \tag{471}$$

That is, in order for the bent form to be possible without a horizontal force F_C being supplied, the weight, length, and elasticity of the column must satisfy the definite relation (471). [The presence of the horizontal force F_A provides the resolution of the paradox, since EULER's earlier formulation took no account of the fact that the load, being the weight of the column, lies to one side of the vertical through the bottom when the top is constrained to lie directly above the bottom.]

EULER proceeds to solve the same problem for the uniform continuous band. Hori- 14—17
zontal forces F_A and F_B act at the top and bottom; the load distributed along the column is taken as a uniform weight σg and a uniform force f_0 pressing the column outward. Equilibrium of the band as a whole requires that

$$(472)\qquad \begin{gathered} F_A + F_B = h f_0 \\ F_A = \tfrac{1}{2} f_0 h + \sigma g c, \quad F_B = \tfrac{1}{2} f_0 h - \sigma g c, \end{gathered}$$

where c is the deflection of the center of gravity from the vertical, $ch = \int_0^h y\,dx$. While 18—21
EULER derives the equation of local equilibrium in detail, [it follows at once from appropriate substitution and linearization in his old general equation (91), *viz*]

$$(473)\qquad F_A x - \tfrac{1}{2} f_0 x^2 + \sigma g \int_0^x x\,dy + \mathcal{B} \frac{d^2 y}{dx^2} = 0,$$

where the x-axis points vertically downward. Eliminating F_A by $(472)_2$ yields

$$(474)\qquad \frac{\mathcal{B}}{\sigma g} y'' + \int_0^x x\,dy + \left(c + \tfrac{1}{2}\frac{f_0 h}{\sigma g}\right) x - \tfrac{1}{2}\frac{f_0}{\sigma g} x^2 = 0 .$$

This equation is to be solved in such a way that $y \approx \alpha x$ for small x, where α is the slope 22
at the top, "and it is to be noted in advance that this angle is the whole effect that the
horizontal force f_0 ... can produce." But the "principal condition" to be satisfied by the 23
integral is that $y(h) = 0$. Thus should follow an equation connecting the constants
occurring in (474) with the angle α. The quantity c is to be eliminated through the relation 24
$ch = \int_0^h y\,dx$. For any f_0 will follow a definite angle α. The case of interest is when $f_0 = 0$. 25
[It would have been possible to set $f_0 = 0$ from the start; the horizontal force acting on
the column served only to help EULER visualize the need for horizontal forces at the sup-
ports[1]). We are thus to solve 26

$$(475)\qquad m y'' + \int_0^x x\,dy + cx = 0,$$

where $m \equiv \mathcal{B}/(\sigma g)$. A solution is given by $y = \alpha p + c q$, where q is a solution of the 27
case when $c = 1$ and p is a solution of the homogeneous equation. For the solution such that $y \approx \alpha x$ for small x, take $p(x)$ as the right-hand side of (459) and

$$(476)\qquad q(x) = \sum_{k=1}^{\infty} (-1)^k \frac{(3k-3)!!}{(3k)!!} \left(\frac{x^3}{m}\right)^k .$$

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1) PEARSON, §§ 83—84 of *op. cit. ante*, p. 11, fails to understand what EULER does here, and he persists in describing the problem of a beam with both ends pinned in EULER's incorrect formulation of E 508 rather than in the correct formulation EULER achieves here. In any case, PEARSON's description of EULER's process makes no sense.

28 Now $ch = \alpha \int_0^h p dx + c \int_0^h q dx$; therefore

$$c = \frac{\alpha \int_0^h p dx}{h - \int_0^h q dx} \quad . \tag{477}$$

The condition $\alpha p(h) + cq(h) = 0$ then implies that

$$hp(h) - p(h) \int_0^h q dx + q(h) \int_0^h p dx = 0 \quad . \tag{478}$$

Since p and q do not depend upon any constant but m, (478) furnishes an equation relating h and m. It is this equation that determines the greatest height h_c the column may have before it bends beneath its own weight.

29 Put $t \equiv h^3/m$, $P \equiv p(h)/h$, $-Q \equiv q(h)$, $\int_0^h p dx \equiv h^2 P^*$, $\int_0^h q dx = -hQ^*$; then (478) assumes the form[1])

$$P + PQ^* - QP^* = 0 \quad . \tag{479}$$

This is an equation for t alone. If t is its smallest root, then the desired height h_c is given by $h_c = \sqrt[3]{tm} = \sqrt[3]{\dfrac{tB}{\sigma g}} = \sqrt[3]{\dfrac{t\beta EA}{\varrho g}}$, where β is a numerical constant. Therefore for
31 columns of the same material we again obtain the remarkable scaling law (466). Expressing βE in terms of the buckling load P_c for a column made of the same material and form but of length l and cross-sectional area A', by (185) we have

$$h_c = l \sqrt[3]{\frac{tA}{\pi^2 A'} \cdot \frac{P_c}{W}} \tag{480}$$

where $W = \varrho g A' l$ is the weight of the column used for comparison (but P_c is its buckling load when weight is neglected).

30, 32 From (476) and (459) follow series for P, Q, P^*, and Q^*. To solve (479) Euler has no
34 alternative but to try substitution of values of t in the series. From the result (464) of the
33 preceding paper, he is sure that $t < 266$ [$\approx 27\pi^2$], and he begins by trying $t = 200$.
34—38 After a staggering numerical calculation he finds that this first guess is indeed a good one, the true root being slightly less than 200, and "it would be superfluous to seek more accurately for its value." Thus (464) is to be corrected by

$$h_c < \sqrt[3]{\frac{200\mathcal{B}}{\sigma g}} \approx 5{,}8 \sqrt[3]{\frac{\mathcal{B}}{\sigma g}} \,, \quad \text{or} \quad h_c < \sqrt{\frac{200\mathcal{B}}{W}} \approx 14 \sqrt{\frac{\mathcal{B}}{W}} \;. \tag{481}$$

1) This equation can be expressed in terms of Bessel functions but becomes less intelligible.

Suppose a column of weight W is just long enough to bend under its own weight in these conditions; then the same column, similarly supported but regarded as weightless, will buckle subject to terminal load P as soon as P reaches the value 39

$$P_c = \frac{\pi^2}{t} W \approx \frac{1}{20} W , \tag{482}$$

this being the special case of (480) obtained by supposing $A = A'$, $hc = l$. This settles the problem EULER is attacking: Long before a column will collapse under its own weight, it will collapse from terminal load. "Since only such columns as may bear a load much greater [than their own weight] are used, it is plain that the error [resulting from neglect of the weight] is of no importance at all and may be safely neglected in practice . . ." [While the inference is not strict, the result is true. 40

These three extraordinary papers, which form EULER's last major work in our subject and which were written when he was approaching or past his seventieth year, make a powerful impression. More revealing of his methods than are any pages from his unpublished notebooks, they frankly lay before the reader the course of EULER's thought and question. His serene confidence in the principles of mechanics refused to let stand the paradoxical conclusion from the first attempt. After a period of doubt whether the analysis itself is correct, EULER returns to the mechanical problem and by a simple and just model derives (464) as an upper bound. The idea used here leads him to see, at last, the easy resolution of the paradox: His first treatment had neglected the torque which the supports must supply in order to equilibrate the torque due to the weight of the bent beam. The corrected analysis leads to the formidable transcendental equation (479), but EULER, as indefatigable a calculator at the end of his life as at the beginning, by a combination of insight, brute force, and good luck arrives at the excellent bound (481).

The least of the many impressions left by these papers is that, judged on any grounds, EULER is the topless giant of mechanics in his century. The greatest of his intense love for the subject. Perhaps as valuable as any of the other definite results is the "wonderful curve[1])" determined in the first paper, by which the shape and the maximum height (467) of a heavy straight rod clamped vertically at the bottom and free at the top are determined.

While EULER was writing these masterpieces[2]), D'ALEMBERT attempted to start

1) In regard to it PEARSON, § 79 of *op. cit. ante*, p. 11, says "the process is extremely complex and leads to no definite result."

2) Printed immediately following them is a work of N. FUSS, "*Varia problemata circa statum aequilibrii trabium compactilium oneratarum, earumque vires et pressionem contra anterides*," Acta acad. Petrop. 1778 I: 194—215 (1780). The major part gives simple calculations of the forces acting at the joints of statically determinate trusses. While essentially equivalent problems had been solved successfully by STEVIN, VARIGNON, and others, they were not put in the explicit context of frameworks,

another of his miserable polemics.] His *Reflections on the theory of springs*[1]) [presents a pitiful accumulation of errors and misunderstandings which need no detailed comment[2]).

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and the possible occurrence of compressive forces in the members was not emphasized. Fuss considers a truss of arbitrary polygonal form without supernumerary members, but the applied loads are always assumed vertical. While this paper seems extraordinarily simple in regard to the *mathematics* of 1780, it should be viewed against the appalling vacuity of the *structural engineering theory* of that time. *Cf.* § VI of S. B. Hamilton, "*Building and civil engineering construction,*" *A History of Technology* 4, 442—488 (1958). Thus it is not surprizing that Thomas Young awards to this little budget of freshman exercises, which, in his typical jargon, he classifies under "carpentry", one of his rare marks of "superior merit and originality" (see below, pp. 413—414).

In § 2 Fuss mentions Euler's critical load but uses it only to determine the minimum thickness of the members of a given truss when the tensions have been determined.

In § 17 Fuss passes to the limit as the number of members becomes infinite. Thus he obtains the equations of a perfectly flexible line subject to loads parallel to a fixed direction.

1) "*Reflexions sur la théorie des ressorts,*" Opusc math. 7, No. 52, § I (pp. 1—38) (1780). There are additional remarks on pp. 384, 388—390.

§§ XVII—XXX of Lambert's paper of 1777, cited above, p. 325, concern the terminally loaded elastica but fail by far to reach the level of Euler's treatment of 1742.

2) In ¶¶ 2—8 he raises and dispels a doubt that the bent elastica is a lever; his "proof" is no more than a complicated restatement of this unprovable postulate of the equilibrium of a deformable line. His incomprehensible ¶¶ 12—16 seem to conclude that loads must be distributed unless they happen to be isolated.

The articles written by d'Alembert for the French Encyclopaedia reveal a limited and defective knowledge of elasticity, both theoretical and experimental. The article "Élasticité" (Ency. **5** (1755)) cites 's Gravesande as the author of the law of proportionality between mean stress and strain (*cf.* above, pp. 116—117) and gives an incomplete form of Mersenne's law (8). *Cf.* also footnote 3, p. 245, footnote 1, p. 262, and footnote 2, p. 311. The article "Élastique" (*ibid.*) compliments James Bernoulli but gives none of his results. Nothing whatever is said concerning the nature and properties of the elastic curves. Instead, a few manipulations are reproduced from the inferior treatment of John Bernoulli (above, p. 89). The article "Résistance" (Ency. **14** (1765)) gives little beyond Galileo's theory. The work of Mariotte and Varignon is mentioned vaguely. D'Alembert explicitly supposes that the neutral line is the fibre on the concave side and concludes that the cross-section of given area having the greatest resistance to bending is that having the greatest moment of inertia about its lower edge. (While Euler always took the neutral line on the concave side, he never discussed any consequences of this assumption or made any use of it. D'Alembert's conclusion, in effect, that a T-beam is stiffer than an I-beam of equal cross-sectional area, is so manifestly in contradiction to experience that we might be justified in expecting it to have aroused his celebrated physical intuition.)

D'Alembert offers a paradox to the history of science. It is generally conceded that he had a good knowledge of experimental phenomena. In formal pure mathematics he had unusual talent. But in attempting to *connect* physical experience with mathematics, he heaped folly on folly. His critical gift was high but almost entirely sterile, and he never applied it to his own work, where misconceptions and misunderstandings jostle slips in easy algebra. Though an accomplished littérateur and an acknowledged philosopher of his day, in his mathematical writings he swaggered from one obscurity to another.

It is difficult to account for the high reputation gained by d'Alembert despite the contempt he

As usual, D'ALEMBERT finds the major problem to be "impossible", this time, the problem 9—11
of the elastica subject to a force that is not tangential. [How he falls into this ridiculous 17—18
blunder is obscure. Spinning out the matter,] he concludes that for a flexible line the distrib-
uted load must be normal and hence proportional to the curvature. JAMES BERNOULLI, 22, 32, 19, 27—28
EULER, and all others who have treated this problem with the exception of LAGRANGE are
wrong; LAGRANGE's construction D'ALEMBERT wishes to replace by a simpler argument[1]).
He presents a theory of his own which seems to be the special case of the theory of flexible, 37—41
not elastic lines when the load is normal; his work seems to be faulty at that. He finds that 42—51
his solution will not fit the [overdetermined] conditions of the problem. Criticizing EULER's 52—63
theory of buckling, D'ALEMBERT lets the terminal load act in any direction; the solution
is no longer uniquely determined. Also, oblique loads less than EULER's critical load can
produce a bent form. [All this is of course obvious.] D'ALEMBERT then obtains some approx- 64—70
imate solutions for elastic curves. After more objections, he concludes "Most of the questions 71—77, 83
I have discussed ... are rather doubts proposed to the mathematicians than positive
assertions. I should consider myself recompensed for my work and my reflections on this
subject if they stimulate the geometers to search for a theory ... subject to no difficulty."
[This is his "Olympian tone", to which BERNOULLI and EULER so often refer in their letters.
D'ALEMBERT wishes to provoke EULER into citing him, and this time he succeeds.]

EULER replies with a note *On the shape of the elastic curve, against certain objections of
the illustrious* D'ALEMBERT[2]), which contains a simple and straightforward treatment of the
equilibrium of an elastic band when the load P acts parallel to the wall into which the band
is clamped. [The problem is the same as that solved long ago in E 65 (above, pp. 203—206),
but the choice of variables is different.] Let $x = 0$ be the clamped end, $x = X$ the 2—4
end where the load is applied, and take the y-axis vertically downward. If θ is the slope

earned from almost all the few who in his own day were competent to judge mathematical researches in the subjects which he studied, a reputation which lives on today. Here and there in his writings are sparkling jewels. These, however, are unknown to his enthusiasts, who praise him in general terms, attributing to him general ideas misrepresented as being clearer and more nearly correct than any independent reader would be likely to infer at first hand. It is not at all to be laid to the difficulty of reading the older authors: CLAIRAUT, the BERNOULLIS, MACLAURIN, HUYGENS are indeed difficult to read, but after the labor there is real fruit, while I can think of no more distaseful part of the historian's duty than that of giving a fair trial to D'ALEMBERT.

1) This is politeness on the part of D'ALEMBERT. As we have seen in § 53, LAGRANGE's treatment is merely derivative from those of JAMES BERNOULLI and EULER.

2) E 537, "*De figura curvae elasticae contra obiectiones quasdam illustris* D'ALEMBERT," Acta acad. sci. Petrop. 1779_2, 188—192 (1783) = Opera omnia II 11, 276—279. Presentation date: 10 June 1782.

Meanwhile LEXELL, §§ 13—17 of *op. cit. ante*, p. 322, had corrected D'ALEMBERT's major error and some minor ones, but did not attempt to follow "all the labyrinths of the arguments by which from his first fallacy he draws further precarious conclusions ..."

angle, then (171) assumes the form

$$\frac{\mathcal{B}}{P}\frac{d\theta}{ds} = X - x\,; \tag{483}$$

since $dx/ds = \cos\theta$, we have

$$\tfrac{1}{2}\frac{\mathcal{B}}{P}\left(\frac{d\theta}{ds}\right)^2 = \sin\varrho - \sin\theta\,, \qquad X - x = \sqrt{\frac{2\mathcal{B}}{P}}\sqrt{\sin\varrho - \sin\theta}\,, \tag{484}$$

where $\theta = \varrho$ when $\dfrac{d\theta}{ds} = 0$, *i. e.*, at $x = X$. [Thus $\varrho = \alpha - \frac{1}{2}\pi$, where α is the angle between the load and the tangent to the band at the end $x = X$.] EULER says that D'ALEMBERT seems to have forgotten to take account of the condition $\theta = 0$ when $x = 0$. By $(484)_2$, this gives $X = \sqrt{\dfrac{2\mathcal{B}}{P}\sin\varrho}$. Hence

$$\begin{aligned} x(\theta) &= \sqrt{\frac{2\mathcal{B}}{P}}\left(\sin\varrho - \sqrt{\sin\varrho - \sin\theta}\right), \\ s(\theta) &= \sqrt{\frac{\mathcal{B}}{2P}}\int_0^\theta \frac{d\varphi}{\sqrt{\sin\varrho - \sin\varphi}}\,, \\ y(\theta) &= \sqrt{\frac{\mathcal{B}}{2P}}\int_0^\theta \frac{\sin\varphi\, d\varphi}{\sqrt{\sin\varrho - \sin\varphi}}\,. \end{aligned} \tag{485}$$

[This system is essentially a simplification of LAGRANGE's form (433).] Then

$$\begin{aligned} l = s(\varrho) &= \sqrt{\frac{\mathcal{B}}{2P}}\int_0^\varrho \frac{d\varphi}{\sqrt{\sin\varrho - \sin\varphi}} = \sqrt{\frac{\mathcal{B}}{2P}}\int_0^{\sin\varrho} \frac{dz}{\sqrt{\sin\varrho - z}\sqrt{1 - z^2}}\,, \\ Y = y(\varrho) &= \sqrt{\frac{\mathcal{B}}{2P}}\int_0^\varrho \frac{\sin\varphi\, d\varphi}{\sqrt{\sin\varrho - \sin\varphi}} = \sqrt{\frac{\mathcal{B}}{2P}}\int_0^{\sin\varrho} \frac{z\,dz}{\sqrt{\sin\varrho - z}\sqrt{1 - z^2}}\,. \end{aligned} \tag{486}$$

Expanding $(1 - z^2)^{-\frac{1}{2}}$ in series and integrating term by term, EULER obtains

$$\begin{aligned} &\sqrt{\frac{P}{2\mathcal{B}}}\, l = \sqrt{\sin\varrho}\, F\,, \qquad \text{where} \quad F \equiv 1 + \sum_{n=1}^{\infty} \frac{[(2n-1)!!]^2}{(4n+1)!!}\, 2^n \sin^{2n}\varrho\,, \\ &l = XF\,, \\ &\sqrt{\frac{P}{2\mathcal{B}}}\, Y = \tfrac{2}{3}\sin^{\frac{3}{2}}\varrho\left\{1 + \sum_{n=1}^{\infty}\frac{(2n+1)!!\,(2n-1)!!}{(4n+3)!!}\, 2^n \sin^{2n}\varrho\right\}. \end{aligned} \tag{487}$$

These formulae express the length and the deflection in terms of the angle ϱ through which the end is turned by application of the load. When ϱ is small, these formulae yield (430).

[While this note of EULER does not present anything new, it reformulates the problem of the elastica in terms of the equations (485) and (486), used often in later researches.]

IV G. The statics and dynamics of skew curves

55. LAGRANGE's variational equations of motion for flexible lines (1761). [Although the formulation of the differential equations for the three-dimensional motion of discrete models and perfectly flexible lines was made easy by EULER's "first principles" (above, § 35),] the first occurrence of such equations is in a work of LAGRANGE, published in 1762 and concerned with the principle of least action[1]). For a discrete system of mass-points LAGRANGE lays down the variational principle

$$\delta \sum_k M_k \int u_k ds_k = 0 \ , \tag{488}$$

where u_k is the speed and s_k the space traversed by the k^{th} mass. [The conditions under which this holds are not stated clearly.] Setting x_k, y_k, z_k for the co-ordinates of the k^{th} body, LAGRANGE regards (488) as equivalent to

$$\int \sum_k M_k \left[d\left(u_k \frac{dx_k}{ds_k}\right) \delta x_k + d\left(u_k \frac{dy_k}{ds_k}\right) \delta y_k + d\left(u_k \frac{dz_k}{ds_k}\right) \delta z_k - u_k \delta u_k \right] dt = 0 \ . \tag{489}$$

In all applications he sets $dt = ds_k/du_k$; thus (489) becomes

$$\int \sum_k M_k \left[\ddot{\boldsymbol{x}}_k \cdot \delta \boldsymbol{x}_k - u_k \delta u_k\right] = 0 \ , \tag{490}$$

where $\boldsymbol{x}_k$ stands for the position vector (x_k, y_k, z_k) and where $u_k = |\boldsymbol{x}_k|$. This is the form of the equations of motion that LAGRANGE actually uses. "To find the motion of a thread XXIV fixed at one of its ends and loaded by an arbitrary number of heavy bodies ...," he sets up the equations of constraint

$$(x_k - x_{k-1})(\delta x_k - \delta x_{k-1}) + (y_k - y_{k-1})(\delta y_k - \delta y_{k-1}) + (z_k - z_{k-1})(\delta z_k - \delta z_{k-1}) = 0 \ . \tag{491}$$

Thus

$$\delta x_k - \delta x_{k-1} = -\frac{1}{x_k - x_{k-1}} \left[(y_k - y_{k-1})(\delta y_k - \delta y_{k-1}) + (z_k - z_{k-1})(\delta z_k - \delta z_{k-1})\right] . \tag{492}$$

Summing this formula from $k = 1, 2, \ldots, n$, and taking $\delta x_0 = \delta y_0 = \delta z_0 = 0$, LAGRANGE expresses all the δx_k in terms of the δy_j and δz_j:

$$\delta x_k = \sum_{j=1}^{\infty} (Y_{kj} \delta y_j + Z_{kj} \delta z_j) \ ,$$

$$Y_{kj} = \begin{cases} \dfrac{y_{j+1} - y_j}{x_{j+1} - x_j} - \dfrac{y_j - y_{j-1}}{x_j - x_{j-1}} & \text{if} \quad 1 \leqq j \leqq k-1 \ , \\[2ex] \dfrac{y_k - y_{k-1}}{x_k - x_{k-1}} & \text{if} \quad j = k \ , \end{cases} \tag{493}$$

---

1) "*Application de la méthode exposée précédente à la solution de différens problêmes de dynamique,*" Misc. Taur. 2_2 (1760/1761), 196—298 [1762] = Œuvres **1**, 365—468.

there being a similar expression for Z_{kj}. Take the x-axis as vertical. Then $\Sigma M_k u_k \delta u_k = g \Sigma M_k \delta x_k$, and (490) yields

$$(494)\quad \begin{aligned} 0 &= \int \sum_{k=1}^{n} M_k [\ddot{y}_k \delta y_k + \ddot{z}_k \delta z_k + (\ddot{x}_k - g)\, \delta x_k] \;, \\ &= \int \sum_{k=1}^{n} M_k [\ddot{y}_k \delta y_k + \ddot{z}_k \delta z_k + (\ddot{x}_k - g) \sum_{j=1}^{k} (Y_{kj} \delta y_j + Z_{kj} \delta z_j)] \;. \end{aligned}$$

Since the variations δy_k and δz_k are independent, from (494) follows

$$(495)\quad \begin{aligned} M_k \ddot{y}_k + \sum_{j=k}^{n} M_j (\ddot{x}_j - g)\, Y_{jk} &= 0 \;, \\ M_k \ddot{z}_k + \sum_{j=k}^{n} M_j (\ddot{x}_j - g)\, Z_{jk} &= 0 \;. \end{aligned}$$

[Thus in LAGRANGE's result the tensions and the constraints are eliminated, and the formalism is unsymmetrical.]

XXV Results for the continuous case are obtained by a limit process. Setting $T = \int_0^l \sigma (g - x)\, ds$, we obtain

$$(496)\quad \sigma ds \left[\ddot{y} + \frac{\mathrm{d}y}{\mathrm{d}x}(g - \ddot{x})\right] - \mathrm{d}\frac{\mathrm{d}y}{\mathrm{d}x}[T - \int_0^s \sigma (g - \ddot{x})\, dx] = 0 \;,$$

where $\mathrm{d}y/\mathrm{d}x \equiv \frac{\partial y}{\partial s} \Big/ \frac{\partial x}{\partial s}$, together with a like equation for z. The constraint is

$$\mathrm{d}x \frac{\partial}{\partial t} \mathrm{d}x + \mathrm{d}y \frac{\partial}{\partial t} \mathrm{d}y + \mathrm{d}x \frac{\partial}{\partial t} \mathrm{d}z = 0 \;.$$

For small motions in a string of uniform thickness we have $\ddot{x} = 0$,

$$\int_0^s \sigma (g - \ddot{x})\, ds = \sigma g s, \; T = \sigma g l \;,$$

and (496) becomes

$$(497)\quad ds \left[\ddot{y} + g \frac{\mathrm{d}y}{\mathrm{d}x}\right] - g\, \mathrm{d}\frac{\mathrm{d}y}{\mathrm{d}x}(l - s) = 0 \;, \quad etc.$$

XXVI [As usual, to treat the case of an *elastic* cord variationally an entirely new approach is required. The constraint (491) must be replaced by

$$(498)\quad a_k \delta a_k = (x_k - x_{k-1})(\delta x_k - \delta x_{k-1}) + (y_k - y_{k-1})(\delta y_k - \delta y_{k-1}) + (z_k - z_{k-1})(\delta z_k - \delta z_{k-1}) \,,$$

and if "the forces of elastic extension or contraction" are F_k, then

$$(499)\quad \sum_k M_k u_k \delta u_k = g \sum_k (M_k \delta x_k - F_k \delta a_k) \;.$$

Combination of (490) and (499) leads to the three-dimensional generalization of [EULER's]
XXVII system (209), with $\ddot{x}_k$ replaced by $\ddot{x}_k - g$. The limit form for the continuous case is

$$(500)\qquad \sigma\left(\frac{\partial^2 x}{\partial t^2} - g\right) = \frac{\partial}{\partial s}\left(T\,\frac{\partial x}{\partial s}\right),\quad \sigma\,\frac{\partial^2 y}{\partial t^2} = \frac{\partial}{\partial s}\left(T\,\frac{\partial y}{\partial s}\right),\quad \sigma\,\frac{\partial^2 z}{\partial t^2} = \frac{\partial}{\partial s}\left(T\,\frac{\partial z}{\partial s}\right),$$

[Thus LAGRANGE is the first to publish the correct and general equations of finite motion of a flexible line in this form. *Cf.* EULER's result (222), which we showed to be equivalent to (227).] For small motion, neglect g and take $x \approx s$; then $T = \text{const.}$, and $(500)_{2,3}$ become

$$(501)\qquad \frac{1}{c^2}\,\frac{\partial^2 y}{\partial t^2} = \frac{\partial^2 y}{\partial x^2},\qquad \frac{1}{c^2}\,\frac{\partial^2 z}{\partial t^2} = \frac{\partial^2 z}{\partial x^2},\qquad c^2 \equiv \frac{T}{\sigma}.$$

LAGRANGE remarks that the limit process is not necessary, since the same variational XXVIII
method may be applied directly to the continuous string. For the loaded inextensible XXIX
string we have

$$(502)\qquad \begin{aligned} 0 &= \delta \textstyle\int dm \int u\,ds\,,\\ &= \textstyle\int dm \int (u\,\delta ds + \delta u\,ds)\,,\\ &= \textstyle\int dm \int (u\,\delta ds + u\,\delta u\,dt)\,. \end{aligned}$$

Now

$$\int_0^l u\,\delta ds = \int_0^l \frac{u}{ds}\,[dx\,d\delta x + dy\,d\delta y + dz\,d\delta z]\,,$$

$$(503)\qquad \int_0^l \left[\frac{dx}{dt}\,d\delta x + \frac{dy}{dt}\,d\delta y + \frac{dz}{dt}\,d\delta z\right],$$

$$= \left[\frac{dx}{dt}\,\delta x + \frac{dy}{dt}\,\delta y + \frac{dz}{dt}\,\delta z\right]_0^l - \int_0^l \left[d\left(\frac{dx}{dt}\right)\delta x + d\left(\frac{dy}{dt}\right)\delta y + d\left(\frac{dz}{dt}\right)\delta z\right].$$

Also

$$(504)\qquad \textstyle\int dm\,u\,\delta u = -\int dm\,\boldsymbol{F}\cdot\delta\boldsymbol{x}\,.$$

Hence when $\delta\boldsymbol{x} = 0$ at the ends, (502) becomes

$$(505)\qquad 0 = \textstyle\int dm \int (d\dot{\boldsymbol{x}} + \boldsymbol{F}dt)\cdot d\boldsymbol{x}\,.$$

Various boundary conditions are considered: One or both ends fixed, one end attached to XXX—XXXI
a ring sliding along a line; and both ends fixed to rings sliding along arbitrary curves.
Then follows the modification for the case when a mass is added at one end. XXXII

In the extensible case, (504) is to be replaced by XXXIII

$$(506)\qquad \begin{aligned} \int_0^l dm\,u\,\delta u &= -\left[\int_0^l dm\,\boldsymbol{F}\cdot\delta\boldsymbol{x} + \int_0^l T\,\delta ds\right],\\ &= -\left[\int_0^l dm\,\boldsymbol{F}\cdot\delta\boldsymbol{x} + \int_0^l \frac{T}{ds}\,d\boldsymbol{x}\cdot\delta\boldsymbol{x}\right],\\ &= -\,T\,\frac{\partial\boldsymbol{x}}{\partial s}\cdot\delta\boldsymbol{x}\,\Big|_0^l\,dt - \int_0^l dm\,\boldsymbol{F}\cdot\delta\boldsymbol{x} + \int_0^l d\left(T\,\frac{\partial\boldsymbol{x}}{\partial s}\right)\cdot\delta\boldsymbol{x}\,. \end{aligned}$$

[With the slipperiness all too common among adherents of variational principles,] LAGRANGE now abandons (502) and replaces it by (505), [irrespective of whether $\delta \boldsymbol{x} = 0$ at the ends or not]. Thus follows

$$0 = \int\int \left[dm(d\dot{\boldsymbol{x}} + \boldsymbol{F} dt) - d\left(T \frac{\partial \boldsymbol{x}}{\partial s}\right)\right] \cdot \delta \boldsymbol{x} - \int T \frac{\partial \boldsymbol{x}}{\partial s} \cdot \delta \boldsymbol{x} \Big|_0^l dt \,. \tag{507}$$

Writing $dm = \sigma ds$, we obtain the differential equations

$$\sigma(\ddot{\boldsymbol{x}} + \boldsymbol{F}) = \frac{\partial}{\partial s}\left(T \frac{\partial \boldsymbol{x}}{\partial s}\right) \tag{508}$$

with the general end condition

$$T \frac{\partial \boldsymbol{x}}{\partial s} \cdot \delta \boldsymbol{x} \Big|_0^l = 0 \,. \tag{509}$$

If at $x = 0$ and $x = l$ we have $\delta \boldsymbol{x} = 0$, then no restriction on $\dfrac{\partial \boldsymbol{x}}{\partial s}$ follows; if $\delta \boldsymbol{x} \propto \boldsymbol{n}$, then $\boldsymbol{n} \cdot \dfrac{\partial \boldsymbol{x}}{\partial s} = 0$ at the ends, *etc.*

56. EULER's theory of the skew elastica. [While LAGRANGE's analysis concerned only flexible mechanical systems, for which the three-dimensional equations of motion were virtually obvious once those for plane motion were known, a decade later EULER attacked a more difficult problem.] His paper *On the whirling motion of musical strings, wherein also the whole theory of equilibrium and motion of flexible and also elastic bodies is briefly explained*[1]), which follows directly on E 481 (described below, pp. 395—396), establishes the general equations for the bending of an initially straight band into a skew curve. [The exposition, apparently unrevised from the method of discovery, reveals the course of thought which enabled EULER to recognize his first and plausible proposal as false and to substitute a correct one for it.] The idea is to write down a set of equations that reduce to (91) in each co-ordinate plane. EULER makes the [unfortunate] choice of using the apparently simpler
3—4 method of moments in place of the "first principles". From the previously derived result (576), below, we see that in space the general expression for the moments about a point A exerted by the load force $\boldsymbol{F}$ per unit length acting along the thread from B to A is

$$\boldsymbol{M} = -\int_A^B \boldsymbol{R}' ds \times \int_A^S \boldsymbol{F} ds \;, \tag{510}$$

1) E 471, "*De motu turbinatorio chordarum musicarum ubi simul universa theoria tam aequilibrii quam motus corporum flexibilium simulque etiam elasticorum breviter explicatur,*" Novi comm. acad. sci. Petrop. **19** (1774), 340—370 (1775) = Opera omnia II **11**, 158—179. Presentation date: 10 November 1774. BURKHARDT, footnote 9 of *op. cit. ante*, p. 11, attributes to this paper the distinction between the number of particles and the number of degrees of freedom, but this seems to be a slip.

where $\boldsymbol{R}' \equiv d\boldsymbol{R}/ds$, $\boldsymbol{R}(s)$ being the radius vector to a typical point on the curve. The tension T, as suggested by $(578)_1$, is

$$\pm T = \boldsymbol{R}' \cdot \textstyle\int \boldsymbol{F} ds \ , \tag{511}$$

where the sign is determined by a convention; EULER usually takes it as $-$. In all prob- 8—9
lems concerning motion, we have only to replace $\boldsymbol{F}$ by $\boldsymbol{F}^*$, where

$$\boldsymbol{F}^* \equiv \boldsymbol{F} - \sigma \frac{\partial^2 \boldsymbol{R}}{\partial t^2} \ . \tag{512}$$

For perfectly flexible bodies, the equation of equilibrium is 4

$$\boldsymbol{M} = 0 \ . \tag{513}$$

For an elastic band, EULER first suggests that the bending moments M_x, M_y, M_z 6
be taken as proportional to the radii of curvature of the projection of the band upon the respective co-ordinate planes:

$$M_x = \frac{D(dy\,d^2z - dz\,d^2y)}{(dy^2 + dz^2)^{3/2}} \ , \quad \textit{etc.}, \tag{514}$$

with three possibly different elasticities D, E, F. Then he remarks that it is strange that 11
three equations are required to express equilibrium of moments in space, while one suffices for the plane. "But if we think more closely about the matter, we shall easily find that the three equations depend upon each other in such a way that any one is implied by the other two." Indeed, if (513) is to hold for all portions of the curve, we must have

$$\boldsymbol{M}' = 0 \ , \tag{515}$$

where

$$\boldsymbol{M}' = -\boldsymbol{R}' \times \textstyle\int \boldsymbol{F} ds \ . \tag{516}$$

From (516) follows

$$\boldsymbol{M}' \cdot \boldsymbol{R}' = 0 \ , \tag{517}$$

so that if any two components of (515) hold, so does the third. But the right-hand sides of 12
(514) do not satisfy (517); "therefore we are driven to reject them and to inquire more closely into the true principles . . ."

EULER now decides, as he explains [rather obscurely] at the end of the paper (§ 35), to regard the bending moment as of amount $\mathcal{B}/r$, *acting in the osculating plane*. "For since in order to produce the curvature occurring in our thread there is required a certain moment of forces acting in the osculating plane itself, . . . this very moment of forces can be resolved according to the [co-ordinate] planes . . ." To resolve this moment, EULER needs not only r but also the cosines b_x, b_y, b_z of the angles made by the osculating plane with the co-ordinate planes [*i. e.* the *components of the binormal*]. The "not a little tedious calculation" of these quantities he puts into an appendix (§§ 29—34); the results are[1])

$$\text{(518)} \qquad \boldsymbol{b} = r(\boldsymbol{R}'' \times \boldsymbol{R}'), \quad r = \frac{1}{|\boldsymbol{R}'' \times \boldsymbol{R}'|} .$$

14 To replace (514) EULER then proposes the equations

$$\text{(519)} \qquad \boldsymbol{M} = \frac{\mathcal{B}}{r}\boldsymbol{b} = \mathcal{B}(\boldsymbol{R}'' \times \boldsymbol{R}') .$$

Since $\boldsymbol{M}' = \mathcal{B}(\boldsymbol{R}''' \times \boldsymbol{R}')$, the hypothesis (513) is compatible with (517).

[In modern terms, we should say that EULER here gives the first example of the testing of a proposed constitutive equation for the *right kind of invariance*. While in fact (514) is not vectorially invariant, this is not what EULER observes; rather, the requirement (517) is a mechanical principle. The proposal (514) fails the test and thus is rejected; the proposal (518), based on a well poised geometrical concept, meets it (and in fact is vectorially invariant as well).] "This criterion . . . furnishes us the firmest ground that our formulae are now consonant with the truth, even though perhaps it would have been difficult to discern the basis of these formulae *a priori*." [While the inference is not just, anyone who has faced and solved such a problem appreciates the temptation.

After this extraordinary display of geometrical and mechanical power,] EULER descends to the vibrating string[2]). Considering small motion, he puts $T = -\int F_x ds$, $F_x^* = F_x$, $F_y^* = -\sigma\frac{\partial y}{\partial t^2}$, $F_z^* = -\sigma\frac{\partial^2 z}{\partial t^2}$, and from the differential form of (513) obtains

1) This seems to be the first occurrence of the osculating plane and the binormal. They are implied, of course, by any determination of the acceleration of a particle moving upon a skew curve. For example, in §§ 223—277 of E289, *Theoria motus corporum solidorum seu rigidorum* . . ., Rostock and Greifswald, Röse, 1765 = Opera omnia II 3—4, EULER determines the direction of the normal force on such a particle; this is the direction of the principal normal, but EULER does not mention the osculating plane. For his later and fuller development of the geometry of skew curves, see E 602, "*Methodus facilis omnia symptomata linearum curvarum non in eodem plano sitarum investigandi,*" Acta acad. sci. Petrop. 1782_1, 19—57 (1786) = Opera omnia I 28, 348—381; *cf.* Professor SPEISER's description on pp. XLIII—XLIV. Notice that the presentation date of E602 is 28 May 1775; thus there is reason to suppose that EULER's geometrical researches on skew curves were occasioned by the mechanical problem described in the text above.

2) On p. 173 of Notebook EH 6 (1750—1757) EULER had derived and had attempted to solve the differential equation of small motion of a string when longitudinal as well as transverse vibration is allowed.

$$(520)\qquad \begin{aligned} &-T dy+\sigma ds \int ds \frac{\partial^2 y}{\partial t^2}=0 , \\ &-\sigma dz \int ds \frac{\partial^2 y}{\partial t^2}+\sigma dy \int ds \frac{\partial^2 z}{\partial t^2}=0 , \\ &-\sigma ds \int ds \frac{\partial^2 z}{\partial t^2}+T dz=0 . \end{aligned}$$

From these equations it follows at once that each transverse displacement y, z satisfies 18
an equation of the type (251), "and hence it is plain that the determinations of the two
variables y and z are altogether independent . . ., which circumstance is without doubt
of the greatest importance, since it allows all whirling motions to be determined just as
easily as those that take place in one plane." If both y and z experience simple harmonic 22
motions of the same frequency, the points of the string move in ellipses normal to the
x-axis, but if the frequencies are different, the string gives out two sounds simultaneously,
and the path is a curve of higher order. More generally, BERNOULLI's principle of composi- 23
tion of sounds continues to hold, with a double infinity of arbitrary constants, so that
"an even greater multiplicity of motions may take place." However, EULER still considers 24—28
the class of motions so obtained as special, and he shows how the solution (257) still holds
and may be applied to extend to whirling motions all the known properties of the plane case.

EULER's paper, *On the pressure of taut ropes stretched upon bodies and upon their motion when hindered by friction . . .*[1]), does not concern skew curves, with the possible exception of helices, but it rests upon results derived in the paper just described. Its subject is the force exerted by a rope wound about a cylinder and the effect of friction upon the motion of such a rope. EULER writes that this problem has been investigated before in special cases, but he is now in a position to give a fully general theory[2]). A rope is hung over a

1) E 482, "*De pressione funium tensorum in corpora subjecta eorumque motu a frictione impedito ubi praesertim methodus traditur motum corporum tam perfecte flexibilium quam utcunque elasticorum non in eodem plano sitorum determinandi, dissertatio prior et dissertatio altera,*" Novi comm. acad. sci. Petrop. 20 (1775), 304—326, 327—342 (1776) = Opera omnia II **11**, 194—210, 211—222. Presentation date: 15 May 1775.

The latter part of the title refers only to an introductory paragraph repeating (510) and (513).

2) The problem of purely normal load, friction being neglected, has been traced above, footnote 8, pp. 80—81. In the work of SAUVEUR, cited there, friction is considered but no specific law is proposed, although SAUVEUR somehow manages to conclude that the tension increases proportionally to the amount of rope in contact with the cylinder.

I have been unable to see the work of SEGNER, *Programma de pressionibus, quas fila corporibus certis circum ducta et utriusque viribus aequalibus tracta in ea corpora exercent, et lineis in eorum corporum superficiebus describendis, quibus imposita eo modo fila quiescunt*, 4to, Göttingen, 1735.

The idea that frictional force is proportional to normal force, widely applied in the eighteenth century, derives from a famous paper of AMONTONS, "*De la resistance causée dans les machines, tant par*

[convex] cylinder and held taut by masses M_1 and M_2 hanging vertically from its two ends.
2—6 Considering first the perfectly flexible case and assuming that the rope exerts only a normal pressure F_n upon the cylinder, [by an unnecessarily elaborate process] using formulae from the previous paper EULER arrives at results [which can be read off from (40) and (42),] *viz*, $T = M_1 g = M_2 g$, hence $M_1 = M_2$, and $F_n = M_1 g/r$.

10—11 For a [flexurally] elastic rope, the same plan of calculation, using (519), leads to

$$(521)\qquad F_n = -\frac{\mathcal{B}}{1+y'^2}\frac{d^2}{dx^2}\frac{1}{r} + \frac{\mathcal{B}y'}{\sqrt{1+y'^2}}\frac{dr}{dx} - \frac{\mathcal{B}}{2r^3} - \frac{C}{r}$$

12 [as corrected]. When the cylinder is circular, we again find that $C = M_1 g$.

13—24 When friction is assumed to exert a tangential force λF_n per unit length on a perfectly flexible rope, the same process leads to the result

$$(522)\qquad F_n = \frac{M_1 g}{r}\, e^{-\lambda\varphi}, \quad T = M_1 g e^{-\lambda\varphi},$$

where φ is the non-negative angle between the tangent to the rope and the direction opposite to that of the load M_1, the greater of the given loads M_1 and M_2. With M_1 given,
25 we thus obtain $M_2 = M_1 e^{-\lambda\vartheta}$, where ϑ is the value of φ at the end where M_2 acts. But if we regard M_2 as given, we may use the same result to calculate the load M_1 just

---

les frottemens des parties qui les composent, que par la roideur des cordes qu'on y employe, et la maniere de calculer l'un et l'autre," Mém. acad. sci. Paris 1699, 3rd ed., 4to, Paris, 206—227 (1732). On p. 208 AMONTONS announces the following experimental laws for the static and dynamic friction of solids in contact:

1. The resistance depends upon the normal force only.
2. The resistance of greased surfaces is independent of the material of which they are made.
3. The modulus of static resistance is $\frac{1}{3}$.
4. Dynamic resistance is proportional to the normal force, the time, and the velocity.

AMONTONS is not able to state such simple conclusions from his experiments on the friction of a rope encompassing a circular rod (pp. 217—220); instead, he gives extensive tables (pp. 223—227).

Immediately thereafter, PARENT gave an ingenious static model in which the apparently rough plane surfaces are in fact perfectly smooth but studded with small hemispherical bosses; the frictional force is identified with the tangential force sufficient to pull one set of bosses out of the troughs and onto the summits of the others. PARENT calculates this force and confirms some of AMONTONS' experimental results. See Hist. acad. sci. Paris 1700, 2nd 4to ed., 151—152 (1761).

EULER may have forgotten that he himself had obtained long ago all the results of real interest in the paper analysed in the text above. On pp. 361—362 of Notebook EH3, a passage that can be dated with some certainty as having been written between 24 May and 20 December 1738, by the method used in the second part of the present paper EULER had derived (40), (522), and (524), from which everything else follows easily. There he found also the "mean direction of all the forces".

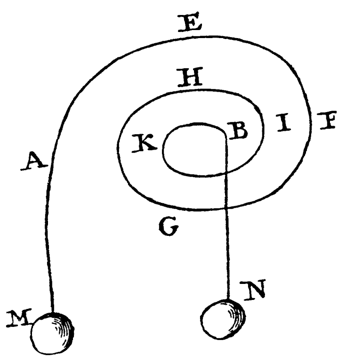

Figure 103. Euler's problem of the frictional force of a rope on a cylinder (1775)

sufficient to maintain equilibrium, namely, $M_2 e^{-\lambda\vartheta}$. Thus for equilibrium we must have

$$(523) \qquad M_2 e^{-\lambda\vartheta} \leqq M_1 \leqq M_2 e^{\lambda\vartheta} .$$

In all this, there is no need that the cross-section be a re- 27
entrant curve: The results apply equally to a curve such as that shown in Figure 103.

The second part of the paper starts by observing that 1—6
most of the calculations of the preceding may be replaced
by use of (40) and (42). For the flexible rope with the 7—10
hypothesis of friction given in the first part we have then

$$(524) \qquad \frac{dT}{ds} = -F_t = -\lambda F_n = -\lambda \frac{T}{r} = -\lambda T \frac{d\varphi}{ds} ,$$

whence (522) follows at once.

These results are so simple that one can proceed to determine the motion in the case 11—13
when the difference of the forces on the two ends is too great to permit equilibrium. We 14—17
have only to replace (524) by

$$(525) \qquad \frac{dT}{ds} = -\sigma u - \lambda F_n = -\sigma u - \lambda T \frac{d\varphi}{ds} ,$$

where u is the acceleration and σ is the line density. Integration yields

$$(526) \qquad \frac{T}{\sigma} = e^{-\lambda\varphi} \left[U(t) - u \int_0^s e^{\lambda\varphi} ds\right] ,$$

where $U(t)$ is an arbitrary function of t. Consider a cylinder whose tangent is vertical at 18
its two edges, so that $\varphi = 0$ at one end, $\varphi = \pi$ at the other. Then at $s = 0$ we have $T = \sigma U$. If the motion is produced by a weight M_1 attached at the end where $s = 0$, the balance of linear momentum there yields

$$(527) \qquad T\,|_{s=0} = \sigma U = M_1 g - u(\sigma g + M_1) ,$$

where q is the displacement, *i. e.*, the amount of rope which has slipped over the cylinder.
Similarly, if we write $C = \int_0^l e^{\lambda\varphi} ds$, where $s = l$ corresponds to $\varphi\pi$, we obtain 19

$$(528) \qquad \sigma e^{-\lambda\pi}(U - uC) = M_2 g + u\left[\sigma(c - q) + M_2\right] ,$$

where c is the length of rope initially hanging down from the point $s = l$ to the suspended
20 mass M_2. Elimination of U between (528) and (527) yields

$$\frac{d^2q}{dt^2} = u = g\frac{M_1 e^{-\lambda\pi} - M_2}{e^{-\lambda\pi}(\sigma q + M_1 + \sigma C) + \sigma(c - q) + M_2} . \tag{529}$$

This result is consistent with (523) in that we must have $M_1 e^{-\lambda\pi} > M_2$ in order for the
21—25 displacement q to increase, as assumed in establishing the equations. While a first integral is easily obtained, EULER is not able to proceed further except by replacing the right-hand side of (529) by its value when $q = 0$.

EULER's later paper, *A more accurate development of the formulae found for the equilibrium and motion of flexible threads*[1]), takes up the three-dimensional theory of E 471 and converts it, through rather long calculations, to intrinsic form. The first part deals
6—8 with the perfectly flexible string in equilibrium. From (515) and (516) it follows that $\int \boldsymbol{F} ds$ is parallel to $\boldsymbol{R}'$; from (511), then,

$$\int \boldsymbol{F} ds = -T\boldsymbol{R}' . \tag{530}$$

9 Therefore

$$\boldsymbol{F} = -T'\boldsymbol{R}' - T\boldsymbol{R}'' . \tag{531}$$

10—13 The tangential and normal components of $\boldsymbol{F}$ are given by $F_t \equiv \boldsymbol{R}' \cdot \boldsymbol{F}$, $\boldsymbol{F}_n = \boldsymbol{R}' \times \boldsymbol{F}$. From (531), then,

$$F_t = -T' , \; \boldsymbol{F}_n = T(\boldsymbol{R}'' \times \boldsymbol{R}) . \tag{532}$$

By $(518)_2$ it follows that

$$F_n = T\,|\,\boldsymbol{R}'' \times \boldsymbol{R}\,| = \frac{T}{r} . \tag{533}$$

The results (532) and (533) generalize [JAMES BERNOULLI's] formulae (40) and (42) to
15 three dimensions. From $(532)_1$ and (533) we have

$$F_t = -(rF_n)' , \qquad rF_n = -\int F_t ds . \tag{534}$$

16 The theory of friction given in E 482 is defined by $F_t = \lambda F_n$. From $(534)_1$ it follows at once in this case that

$$\frac{F_n'}{F_n} + \frac{r'}{r} + \frac{\lambda}{r} = 0 . \tag{535}$$

1) E 608, "*Accuratior evolutio formularum pro filorum flexibilium aequilibrio et motu inventarum,*" Nova acta acad. sci. Petrop. 1782_2, 148—169 (1786) = Opera omnia II 11 335—354. Presentation date: 22 May 1775.

Setting

(536) $$\varphi = \int \frac{ds}{r} \,,$$

we obtain

(537) $$F_n = \frac{Ce^{-\lambda\varphi}}{r} \,, \quad F_t = \frac{\lambda C e^{-\lambda\varphi}}{r} \,,$$

generalizing (522). On a smooth surface, the stretched rope would assume the curve of 17
shortest possible length between its end points, but so as to visualize the results more generally, we think of the rope as lying in a groove of given shape.

EULER now introduces formally the components p, q, r of the vector 19

(538) $$\boldsymbol{B} = \boldsymbol{R}'' \times \boldsymbol{R}' \,,$$

which had appeared in (518) and (519). EULER remarks upon $(518)_2$, *i. e.* $B = 1/r$, and

(539) $$\boldsymbol{B} \cdot \boldsymbol{R}' = 0 \,.$$

From (531) follows also 20—21

(540) $$\boldsymbol{F} \cdot \boldsymbol{B} = 0 \,,$$

"a remarkable property of the state of equilibrium." [EULER's purely algebraic procedure is somewhat difficult to follow; what has been shown, indeed already at (532), is that in order for there to be no resultant torque on any section of a line subject to a force $\boldsymbol{F}$, that force must lie in the osculating plane.

There follows a sequence of manipulations rather difficult to motivate; a result equi- 22—23
valent to EULER's may be derived by a shorter analysis he himself uses as check, as follows.] Differentiating (531) yields

(541) $$\boldsymbol{F}' = -T''\boldsymbol{R}' - 2T'\boldsymbol{R}'' - T\boldsymbol{R}''' \,,$$

From this result and from (531) we have

(542) $$\boldsymbol{R}'' \times \boldsymbol{F} = -T'\boldsymbol{B}, \ \boldsymbol{R}' \times \boldsymbol{F}' = 2T'\boldsymbol{B} + T\boldsymbol{R}''' \times \boldsymbol{R}' = 2T'\boldsymbol{B} + T\boldsymbol{B}' \,,$$

so that

(543) $$\boldsymbol{R}' \times \boldsymbol{F}' + 2\boldsymbol{R}'' \times \boldsymbol{F} = T\boldsymbol{B}' \,.$$

Both this last result and $(532)_2$, which we may express in the form $\boldsymbol{F}_n = T\boldsymbol{B}$, assert the collinearity of two vectors, and in both cases the factor of proportionality is T. We may write this result as a *condition on the force* $\boldsymbol{F}$, as follows:

(544) $$\frac{\boldsymbol{B}'}{\boldsymbol{R}' \times \boldsymbol{F}' + 2\boldsymbol{R}'' \times \boldsymbol{F}} = \frac{\boldsymbol{B}}{\boldsymbol{R}' \times \boldsymbol{F}} \,.$$

EULER observes that if this proportionality holds for any *one* component, and if (540) holds, then (544) holds for all three components. EULER's result then is that *for a force field* $\boldsymbol{F}$ *acting on a curve to yield zero resultant torque, it is necessary and sufficient that it lie in the osculating plane and satisfy* (544).

24—27 For the helix $x = \alpha \cos s$, $y = \alpha \sin s$, $z = ns$, we reduce (540) and one component of (544) to the forms

$$(545) \qquad \begin{aligned} -\alpha n F_x \sin s + \alpha n F_y \cos s - \alpha^2 F_z = 0 \ , \\ 2F_x \cos s + F'_y \sin s - 2F_x \sin s + F'_x \cos s = 0 \ . \end{aligned}$$

These formulae enable us to calculate two of the components F_x, F_y, F_z in terms of any one. If instead we regard the tension T as given, then we obtain

$$(546) \qquad \begin{aligned} F_x &= \alpha T' \sin s + \alpha T \cos s \ , \\ F_y &= -\alpha T' \cos s + \alpha T \sin s \ , \\ F_z &= -nT' \ . \end{aligned}$$

29—30 Before turning to elastic wires, EULER notes that (538) implies not only (539) but also

$$(547) \qquad \boldsymbol{B} \cdot \boldsymbol{R}'' = 0 \ .$$

Also

$$(548) \qquad \boldsymbol{B}' = \boldsymbol{R}''' \times \boldsymbol{R}', \quad \text{so that} \quad \boldsymbol{B}' \cdot \boldsymbol{R}' = 0, \ \boldsymbol{B}' \cdot \boldsymbol{R}''' = 0$$

and

$$(549) \qquad B^2 = R'^2 R''^2 - (\boldsymbol{R}' \cdot \boldsymbol{R}'')^2$$

(which EULER writes as

$$(550) \qquad B^2 = \frac{ddx^2 + ddy^2 + ddz^2 - dds^2}{ds^4} \Bigg) \ .$$

31 By (516), a differential form of (519) is

$$(551) \qquad -\boldsymbol{R}' \times \int \boldsymbol{F} ds = \mathscr{B} \boldsymbol{B}' \ ;$$

32 hence

$$(552) \qquad \int \boldsymbol{F} ds = -T\boldsymbol{R}' + \mathscr{B} \boldsymbol{R}' \times \boldsymbol{B}' \ ,$$

and

$$(553) \qquad \boldsymbol{F} = -T'\boldsymbol{R}' - T\boldsymbol{R}'' + \mathscr{B}(\boldsymbol{R}'' \times \boldsymbol{B}' + \boldsymbol{R}' \times \boldsymbol{B}'') \ ,$$

33 and therefore

$$(554) \qquad \begin{aligned} F_t = \boldsymbol{F} \cdot \boldsymbol{R}' &= -\mathscr{B} \boldsymbol{B} \cdot \boldsymbol{B}' - T' \ , \\ &= \mathscr{B} \frac{r'}{r^3} - T' \ . \end{aligned}$$

34 Further progress does not seem possible.

[While Euler is unable to solve any particular problems concerning the equilibrium of skew lines, he has assembled many of the formulae that are to be used in later theories. Completely lacking is any concept of twist. Euler's results here pertain rather to the geometry of space curves than to the more complicated problems concerning the spatial deformations of elastic bands. The reader will have remarked Euler's easy mastery of the methods of vectorial algebra; the formulae we have presented are shortened by use of vector symbols, but the operations indicated are those used by Euler.]

IV H. The laws of elasticity and flexibility

57. Miscellaneous researches (1754—1768). Shortly before his death in 1754 James Riccati wrote two qualitative essays[1]) on the general nature of elasticity. The second of these puts forward a fundamental idea: "To induce constipation [*i. e.*, condensation] there is required beyond any doubt a live force, which is lost in producing this effect. And since this employed force does not go into nothing, it is necessarily absorbed by the body and passes into dead force, which the body conserves within itself." This dead force is available, at least in part, for unbending the fibres. Riccati explains that part of this force can go into producing vibrations of the body, and part or even all of it can be held in "a stable condensation", so that the fibres persist in a "violent configuration". These conclusions he infers from the fact that "the present universe is a well conceived system," which could not allow nature "to go successively more slowly, until matter . . . became but a lazy mass." Moreover, "since the communicated force, which maintains itself in the fibres, must have some effect in its quality of dead force, it necessarily manifests itself only in mutual attempts and in equal and opposite pressures. So struggling together, the pushing not prevailing over the counter push, the elements of the body dispose themselves in equilibrium . . ."

[Thus Riccati suggests that a part, at least, of *the work done in deforming a body is stored as potential energy*[2]) available for reversing the deformation; in its deformed state, the body manifests the presence of this energy by equilibrated *mutual forces* (now called "stresses") in its interior. While these ideas in the hands of Green and Cauchy a century later were to furnish the basis of the general theory of elasticity, Riccati expresses them

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1) They appear in his "*Saggio intorno il sistema dell'universo,*" Opere 1, 598 pp., Lucca (1761): Book II, Part 1, Ch. III (pp. 152—164), "*Delle forze elastiche.*" *Ibid.*, Ch. IV (pp. 164—173), "*Da quali primi principj derivi la forza elastica.*"

The first of these is merely physical and seems to lead to no definite conclusion.

2) While Daniel Bernoulli had proposed the definite and indeed correct "potential live force" (140) for the elastica in 1738, he described it only in connection with a minimal principle and did not discuss the availability of this live force for reversing the deformation.

but vaguely, and, lacking the mathematical apparatus necessary to put them in a definite form, he rests content perforce with repeating them often in different words.]

In 1767 his son JORDAN RICCATI published a paper *On the proportion between the extensions of strings and the forces that produce them*[1]). After citing his father's statement that longitudinal and transverse vibrations obey similar laws, he points out that these two types of vibrations arise from different causes: the "intrinsic rigidity" [*i. e.*, elasticity] "which depends upon the structure of the string and the entanglement of the fibres," and the "extrinsic ..., which depends on the stretching forces."

II—V JORDAN RICCATI claims to demonstrate seven "canons"; [if these be drawn from experiment, he does not say so]. A wire is of length L_0 when loaded by a tension P_0; then the "canons" are summarized by the following formula relating the present length L to the present tension P:

$$b\frac{dL}{L} = \frac{dP}{P + \frac{EAL_0}{L}}, \quad L = L_0 \quad \text{when} \quad P = P_0, \tag{555}$$

where b and E are constants of proportionality and A is the cross-sectional area. [The integral of (555) is

$$P = \frac{EAbL_0}{(b+1)L}\left[\left(\frac{L}{L_0}\right)^{b+1} - 1\right] + P_0\left(\frac{L}{L_0}\right)^b. \tag{556}$$

XIII Therefore Eb is what is now called "YOUNG's modulus."] RICCATI obtains a formula similar to (556) but not identical with it; [his integration is faulty[2])].

In a following paper[3]) RICCATI remarks that if EA is very large [and $L - L_0$ is

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1) Sched. I, "*Della proporzione fra la distensioni delle corde, e le forze che le producono,*" *op. cit. ante*, p. 280. As mentioned above, p. 115, an earlier version of this paper was published as an annotation to the *Opere* of JAMES RICCATI in 1761.

2) From experiment, RICCATI observes that "not all materials called by the same name are equally rigid. For example, not all brasses have the same rigidity." There follows a reference to drawing which I do not fully understand; it seems to mean that if a wire of cross-section A and rigidity $h = EA$ is drawn until it has a smaller cross section A', then h' is a little greater than EA'; *i. e.*, drawing increases "YOUNG's modulus" (§ VI). On the basis of (556) RICCATI claims to evaluate the frequency of longitudinal oscillation by means of an energy argument. I do not follow the reasoning; the result is

$$\nu = \frac{1}{2\pi}\sqrt{\frac{bg(EA + P_0)}{L(\frac{1}{3}mg + P_0)}}.$$

No end conditions are mentioned; when $P_0 = 0$, this result does not give the correct answer for any of the usual end conditions.

3) In the same volume, Sched. III, "*Della proporzione fra le forze applicate a squadra alla metà delle corde tese, ed i varj effetti da esse forze cagionati,*" pp. 33—64, see § XIX. The bulk of this paper deals with the problems treated by RICCATI's father in the third and first papers cited in note 1, p. 116, *supra*. The difference is that the younger RICCATI uses his own law (555) for the elastic force

small], (555) implies that $P \propto L - L_0$ when $P_0 = 0$. "Now since the rigidity EA is rather large in respect to the weights used in practice, this is the reason why the extensions are found to be in the same ratio as the added weights." [This is the earliest appearance of an *incremental law of elasticity*, unless the vague words of the elder RICCATI (above, p. 116) may be so interpreted. The proposal (555) would be more interesting if RICCATI had brought forward more definite support for it, either from experience or from reason.

We have described above, pp. 347—349, the second of the three papers in which JOHN III BERNOULLI published the results of researches done earlier under the direction of his uncle, DANIEL BERNOULLI. The first of these, *Researches on the extension suffered by wires prior to breaking*[1]), attacks some of the same problems [as those studied by the RICCATIS]; BERNOULLI uses [HOOKE's] law and thus arrives at simpler results, but the configurations he considers are somewhat more general. All of his work is phrased in terms of *rupture;* [following DANIEL BERNOULLI[2]),] he assumes that *rupture occurs when a certain elongation or deflection* is attained, but as he supposes tacitly that bodies are linearly elastic up to this limit, all his calculations are set within elastic theory, [in terms of which we phrase them here].

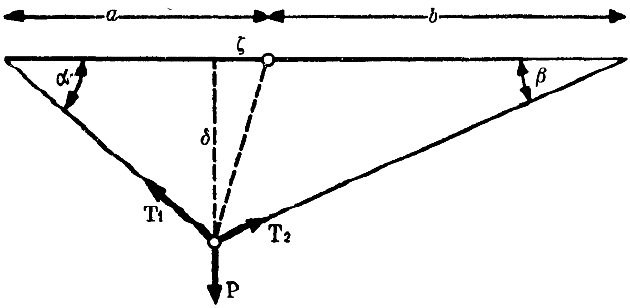

Figure 104. Variables used in DANIEL BERNOULLI's calculation of the form of an elastic cord loaded at one point (1766)

The problem including most of the results at the beginning of the paper is explained in Figure 104. An elastic cord ACB is loaded by a weight P applied at the point C, not necessarily the midpoint; we are to find the deflections ζ and δ. JOHN III BERNOULLI gives two solutions; for the first, he acknowledges EULER's

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rather than his father's law (72). Both from measured frequencies of oscillation (§§ III—IX) and from direct experiments (§ XVIII), he confirms his (false) integrated form. Here he states more clearly that wires are made more rigid by drawing (§ XIX).

1) "*Recherches sur l'extension que souffrent les fils avant de se rompre,*" Hist. acad. sci. Berlin [22] (1766), 78—98 (1768). A footnote says the paper was read in 1764.

In his first letter to JOHN III BERNOULLI, dated 7 December 1763, DANIEL BERNOULLI writes, "It is good that you are putting in order the researches we did on the force of wires and beams. Begin with the force of wires, more susceptible of exact determination; there is enough material to make two fine memoirs. The effect of shocks on wires is still new and worthy of note. For example, it is very paradoxical that a string which is much stronger than another can be broken by a much weaker shock. I am sure that the experiments will conform to the theory the more precisely, the more precise are the experiments themselves. But it is inconceivable how many ways there are to sin against precision of experiment . . ."

2) Indeed, in an undated letter of 1764, DANIEL BERNOULLI expresses the more plausible hypothesis of rupture at a certain maximum *strain*. Given a cord of length l that breaks when its elongation

help, and the second he cites from a letter of his uncle, DANIEL BERNOULLI[1]). This latter is the simpler. We have

$$T_1 = \frac{P\cos\beta}{\sin(\alpha+\beta)} \approx \frac{P}{\alpha+\beta} \approx \frac{P}{\frac{\delta}{a}+\frac{\delta}{b}} = \frac{Pab}{\delta(a+b)} \approx T_2 , \tag{557}$$

$$AE = \sqrt{(a-\zeta)^2+\delta^2} \approx a - \zeta + \frac{\delta^2}{2a} ,$$

$$\Delta l_1 \equiv AE - AC \approx \frac{\delta^2}{2a} - \zeta , \quad \Delta l_2 \equiv BE - BC \approx \frac{\delta^2}{2b} + \zeta .$$

Since $T = K\frac{\Delta l}{l}$ and $T_1 \approx T_2$, we have

$$\frac{\frac{\delta^2}{2a}-\zeta}{a} = \frac{\frac{\delta^2}{2b}+\zeta}{b} , \quad \text{or} \quad \zeta = \frac{b-a}{2ab}\,\delta^2 . \tag{558}$$

Thus the transport ζ, "which seems to characterize the problem," is of the second order in the small deflection δ, and to neglect it would lead to very small error. To determine δ, we apply the elastic law to the whole wire, which is subject to tension $\frac{Pab}{\delta(a+b)}$ and which has experienced the total elongation $\Delta l_1 + \Delta l_2$:

$$\frac{Pab}{\delta(a+b)} = \tfrac{1}{2}K\,\frac{\delta^2\left(\frac{1}{a}+\frac{1}{b}\right)}{a+b} , \quad \text{or} \quad \delta = \sqrt[3]{\frac{2Pa^2b^2}{K(a+b)}} . \tag{559}$$

If $a = b = \frac{1}{2}$, this gives $P = \frac{8\delta^3}{l^3}K$.

11—12 Next is considered the deflection caused by a weight allowed to fall upon the midpoint of a horizontal wire. BERNOULLI's hypothesis is that the wire when its midpoint has descended the distance y exerts upon the striking mass just the same force as would be required to effect the static deflection y. He then calculates the velocity the striking mass

is α, take another cord of length λ; then "one sees at once that the cord will break if the shock of the little body π can extend the string more than the quantity $\frac{\lambda}{l}\alpha$; if not, the cord will not be broken." This passage refers to "the strength of wires or cords intended to sustain shocks," the results being "the newest, the most curious, and the most unexpected in all this matter."

1) In an undated letter, probably of 1766, DANIEL BERNOULLI, after acknowledging receipt of a draft of this paper, advises JOHN III BERNOULLI to check it over and rewrite it in such a way as to emphasize not the calculation but the experiments, many more of which he then proposes. He criticizes EULER's solution of the problem indicated by Figure 104 and then gives his own. A later letter, also undated, contains a further claim of the superiority of his solution over EULER's.

A solution of this problem is given on p. 358 of EULER's notebook EH 4, written in 1740—1744. On p. 359 is a solution for the inverse problem of the flexible and extensible cord; given the shape and the forces, the amount of extension is calculated.

must have when it first encounters the wire in order that it be just brought to rest when
the wire breaks at an assigned deflection d.

Now consider a wire such that a weight P hung from one end produces an elongation 14
α according to [HOOKE's] law, $P = K\alpha$; then attach to the end a weight W and let it
drop from a height s. How great must s be in order that the resulting elongation be exactly
α? The weight is assumed to fall freely until the wire reaches its natural length, after which
time gravity is neglected and the weight is regarded as a mass subject to a linear spring.
An elementary calculation yields

$$s = \tfrac{1}{2}\frac{W}{P}\alpha\ , \tag{560}$$

[This result resembles the equally elementary but not yet published remark that a load
suddenly applied to a linear spring produces a maximum deflection twice as great as does
the same load applied statically.] BERNOULLI solves the same problem for the more general 15—16
case when gravity is taken into account and when the oscillatory elongation is compared
with the static elongation of a string of different length. Then he considers the oscillatory 17
motion of a flexible elastic wire strung horizontally and supporting a weight at its mid-
point. The paper closes with remarks which seem to imply that BERNOULLI considers the 18
laws of bending and extension to be analogous.

We have seen that by 1727 EULER had derived the formula (86) expressing the bend-
ing moment in an elastic band in terms of properties of the cross-section, and thus that he
had in his hands results equivalent to the basic law (87), by which JAMES BERNOULLI's
law for the elastic band is derived from HOOKE's law for the fibres comprising it. [This
result, however, EULER appears to have forgotten,] since in his work of 1743 he not only
omitted it but proposed instead the [incorrect] formula (189). In connection with his second
analysis of the buckling of beams[1]), EULER writes in 1757 that the "absolute elasticity" I, III
of a bent beam should be called the "moment of spring" or "moment of stiffness," since he
now considers the theory of the elastica to be applicable to the loading of non-elastic beams
as well as elastic ones; "... it makes no difference at all whether or not the body after II
bending is endowed with a force for reestablishing itself." Taking the depth D as the maxi- IV—V
mum dimension in the plane of bending and the breadth B as the maximum dimension
perpendicular to that plane, EULER considers it "rather plain" that $\mathcal{B} \propto B$, but the
depth "offers more resistance to bending," and it seems that $\mathcal{B} \propto D^2$ or D^3. For circular
cylinders of diameter d, the two possibilities lead to $\mathcal{B} \propto d^3$ and d^4 respectively. EULER VI
calls for experiments and possibly also a theory, [forgetting his own unpublished work of
1727,] to relate $\mathcal{B}$ to the material of the beam and the shape of its cross-section. Since
dim $\mathcal{B}$ = [Force] [Distance]^2, the moment of stiffness is "similar to the expressions denot-

1) E 238, cited above, p. 345.

XLIII ing the moments of inertia of bodies." Tentatively EULER adopts the relation $\mathcal{B} \propto d^3$ at first, but by the end of the paper he has decided [correctly] that both theory and experiment favor $\mathcal{B} \propto d^4$.

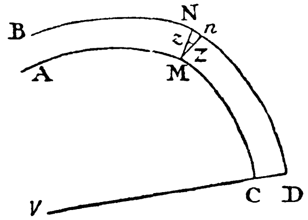

Figure 105. EULER's second derivation of BERNOULLI's law from HOOKE's law (1760)

[While EULER does not state what the theory may be, it is natural to conjecture that he has recalled his own of many years before,] since in his paper on bells[1]), written not long after the paper we have just discussed, he publishes a clarified and shortened form of his old analysis for a beam of circular form and rectangular cross-section. We are to calculate the bending moment about M, a point on the inside of the bent ring (Figure 105), where $\omega = NMn$, the small angle produced by the bending. Since the elongation ωz of the filament zZ at a distance z from M is assumed to obey [HOOKE's] law, the force acting on the filament is proportional to $\omega z dz$, and hence the moment of that force about M is proportional to $\omega z^2 dz$. The integral of all these moments is $\frac{1}{3}\omega D^3$, where D is the depth MN. "I do not take account of the absolute magnitude of this moment, since it may be studied by experiment in each particular case." Thus if B is the breadth of the ring, the total moment is $\frac{1}{3}ED^3B\omega$, where E is a constant of proportionality. Since $\omega = \frac{1}{r} - \frac{1}{R}$, where r is the radius of curvature of the bent ring "at some mean point between M and N" and R is the original radius, we obtain (87) in the special case when $I = \frac{1}{3}D^3B$. [The "constant of proportionality", which in EULER's notation is $3E$, is "YOUNG's modulus."]

In this work as in all his later papers, EULER tacitly supposes that the *neutral line is the fibre on the concave side*. [This had been recognized as false by several earlier authors, as EULER must surely have known. In none of his papers or notes is there any discussion of the matter, perhaps because it does not affect the theory itself but only the manner in which the theoretical results are to be interpreted in practice.]

The effect of the location of the neutral line is recognized in a work of 1766, *Problems*
1—7 *on the resistance of beams*[2]), by JOHN III BERNOULLI[3]). The first part of the paper tacitly puts the neutral line at the bottom and follows the older writers (above, pp. 60—62, 102—104) in neglecting the curvature though taking account of the tension

1) See §§ 3—4 of E 303, cited above, p. 320.

2) "*Sur la cohérence des corps, troisième mémoire. Problèmes sur la résistance des poutres,*" Hist. acad. sci. Berlin [22] (1766), 108—116 (1768).

3) The silence of all the great geometers of the eighteenth century regarding the neutral line is difficult to explain. Thus DANIEL BERNOULLI's criticism of this paper, given in an undated letter of 1766, does not mention the subject. Indeed, the consideration of the neutral line is the only matter in JOHN III BERNOULLI's entire *œuvre* on elasticity that cannot be identified as coming directly from DANIEL BERNOULLI.

varying linearly over the cross-section. BERNOULLI again regards rupture as occurring when 2
a certain *maximum elongation* is attained; he compares the breaking strength in tension 6
and in bending by supposing that a beam breaks in bending when the elongation of the outermost fibre equals that which is sufficient to break all the fibres of a beam stretched along its length, [but his rough calculations do not yield the correct scaling laws because his theory, as just mentioned, is incomplete].

Then he concedes that "if the experiment were done, it might perhaps be found" 8
that the breaking strength in extension would be much greater than calculated. This can
be explained by "a special theory, founded in nature itself." To construct this theory, 9
BERNOULLI observes that according to the previous treatment the point on the under side of the beam at the wall would be the fulcrum and thus "would support all the stress of the weight P ... and thus would suffer prodigiously, which is repugnant to the order of nature." To reduce this suffering, one can suppose the neutral line somewhere higher up in the cross-section, so that the fibres below are compressed and help to bear the weight. The location of this "point of rest ... is not determined, but probably enough, it is just about in the middle. In any case it cannot be on the top; the experiment indicated will be the best way of determining it." [It is difficult to explain BERNOULLI's work, since he does not have the complete formula (87), but rather a result of the old type (61). What he has done is to rediscover PARENT's observation (above, p. 112): In effect, though not in his own words,]

(561) $$I = \alpha A D^2 \ ,$$

where the numerical constant α depends upon the position of the neutral line.

BERNOULLI compares the resistances of a cylindrical beam of diameter D with that of 10
a square beam of the depth D. First taking the neutral line on the concave side, by an elaborate calculation he gets the ratio $\frac{15}{64}\pi$; [this is correct, since the two moments of inertia are $\frac{5}{64}\pi D^4$ and $\frac{1}{3}D^4$, respectively]. For the case when the neutral line is in the middle, the ratio is $\frac{3}{16}\pi$, [since the respective moments of inertia are $\frac{1}{64}\pi D^4$ and $\frac{1}{12}D^4$]. "And since several experiments made by Mr. MUSSCHENBROEK give just about this ratio, they confirm the hypothesis by which we have found it."

BERNOULLI observes that a beam whose cross-section is a triangular prism with one 11
face horizontal can be broken part way through by a terminal weight. This suggests to him that a trapezoidal beam may be stronger than a triangular one of greater cross-sectional area. [On his hypothesis that rupture occurs when a certain elongation is achieved, this is true[1].] He calculates the resistance of the lower part of a slender triangular section

1) The idea was suggested by DANIEL BERNOULLI in a letter of June 1766. In this same letter he explains DE RÉAUMUR's paradox (above, p. 58): In a twisted rope, some of the fibres have been

and finds that this resistance is a maximum when the ratio of the height of the trapezoid to that of the triangle from which it is cut is $\frac{8}{9}$.

In the same year appeared D'ALEMBERT's note, *On the law of compression of springs*[1]).
1—3 Referring to the old essay of JOHN II BERNOULLI (above, p. 171) and to DANIEL BERNOULLI's criticism of it (above, p. 172), D'ALEMBERT objects, in effect, that the force F of a spring need not be a smooth function of the elongation x when $x = 0$. For example, if $F \propto x^m$ and $m > 1$, small motion is not isochronous, contrary to DANIEL BERNOULLI's
4—7 principle[2]). After a long calculation he decides that small motion subject to force of the
8 type $F = \alpha x + \beta x^2$ is sensibly isochronous. He attempts[3]) to treat the case when $F = \alpha x + \beta x^3$, but decides to "leave the discussion to others."

It is perhaps to these remarks that LAGRANGE alludes in 1770 when he writes[4]) that since the law of the elastica has been doubted by "a very great geometer", he will establish it in a manner "as simple as it is rigorous." He represents the band as a polygonal linkage in which each link is pulled back toward the straight form by a spring whose tension is proportional to the angle between the links. [This is the law that was set up by EULER in his first paper (*cf.* Figure 54) as a postulate to be tested by experiment; in principle, LAGRANGE's approach by means of a model recalls JAMES BERNOULLI's first treatment, except that LAGRANGE's arrangement of springs seems entirely *ad hoc*, while JAMES BERNOULLI's is plausible[5]).]

58. EULER's introduction of shear force and derivation of the general equations of equilibrium for a deformable line (1771, 1774). [A pinnacle of our subject is achieved by

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stretched in the fabrication and hence cannot bear their full breaking loads, so that the breaking strength of the rope falls short of the sum of the breaking strengths of all its fibres.

1) "*Sur la loi de la compression des ressorts,*" Opusc. math. 5, No. 36, § I (pp. 216—222) (1768).

2) It is typical of D'ALEMBERT that while his own manipulations nearly always presuppose analytic functions, he is quite ready to call non-analytic functions to his aid in attacking others.

3) In § IV of No. 44, pp. 503—504, D'ALEMBERT seems to have decided that when $F = \alpha x + \beta x^n$, $n \neq 2$, it is impossible for the motion to be approximately isochronous.

4) § I of *op. cit. ante*, p. 349.

In his letter of 4 April 1771 to D'ALEMBERT, LAGRANGE mentions this paper as giving a "rigorous proof" of the law of the elastica, which D'ALEMBERT had doubted. D'ALEMBERT replies on 17 August that he remains unconvinced, for a spring is "an imperfect lever . . ., neither perfectly stiff nor perfectly flexible."

5) There is a paper which is sometimes cited as giving a false theory of the spiral spring: JOHN III & JAMES II BERNOULLI, "*Mémoire sur l'usage et la théorie d'une machine qu'on peut nommer* Instrument ballistique" (1782), Nouv. Mém. acad. sci. Berlin 1781, 347—376 (1783). § III indeed considers the work done in compressing a spiral spring but quite explicitly presupposes no elastic law whatever for it; the force F as a function of the displacement x is to be measured experimentally so that the work, $\int F dx$, can be calculated approximately by a sum.

This paper derives from the instruction given by DANIEL BERNOULLI to his nephews.

two great papers of EULER written in 1771 and 1774.] Over forty years had passed since he had composed the first treatise organizing and unifying the theories of elastic and flexible lines as they were then known (E8, described above, pp. 148—150). *The genuine principles of the doctrine of equilibrium and motion of flexible or elastic bodies*[1]) treats the same class of problems. [First, EULER recasts the whole body of known results on elastic and flexible bodies in the simple, direct way made possible by "the first principles of mechanics" (above, § 35).] As explained at the outset, "we are still far distant from a complete theory, sufficient to determine the shape of flexible surfaces and bodies," and in this paper perforce he rests content with a "more accurate" study of "simple threads, whether perfectly flexible and elastic, as they have been treated up to now by the geometers. But since the numerous solutions . . . that may be found here and there are derived from principles either too special or not sufficiently clear and perspicuous, I will take pains to explain so lucidly the true and general principles . . . that not only the figures of equilibrium but also the [finite] motion of such bodies may be determined."

[Second, this paper revives and greatly extends JAMES BERNOULLI's careful distinction between the *laws of mechanics* and the *constitutive equations* defining particular kinds of continuous bodies (above, pp. 105—108). It is EULER's achievement to obtain the *general equations of equilibrium and motion of a deformable line* in the plane, independent of any special hypothesis regarding the material of which it is composed and of any assumption of small deformation.]

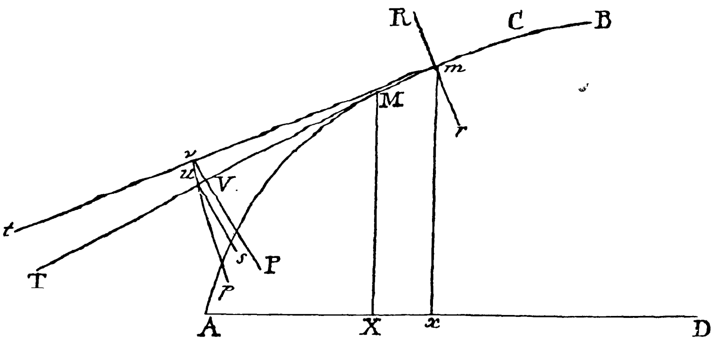

Figure 106. EULER's diagram for the general line stress in a rod (1771)

The "General Problem" 1
with which the paper begins is to establish the equations of equilibrium for a [plane] curve subject to arbitrary forces [acting in its plane. As to be expected from EULER's successful treatment of perfect fluids[2]), he foreshadows the stress principle] by considering the curve BMA to be cut in imagination into two pieces BM and MA, then replacing the action of the part MA on the part BM by that of a force and a moment (Figure 106). The force, [which is the *stress resultant*,] 7
he splits into a tangential component T and a normal component V; the latter is regarded

1) E410, "*Genuina principia doctrinae de statu aequilibrii et motu corporum tam perfecte flexibilium quam elasticorum*," Novi comm. acad. sci. Petrop. **15** (1770), 381—413 (1771) = Opera omnia II **11**, 37—61. Presentation date: 14 January 1771.

2) *Cf.* pp. LXXXI—LXXXII of my Introduction to L. EULERI Opera omnia II **12**.

8—10 as acting at the point V, a distance v along the tangent from M, so that $\mathscr{M} = vV$. [While in former treatments the total force and moment were calculated,] EULER now demands that the resultant force and moment *on the element ds* shall vanish. The assigned tangential and normal forces per unit length acting upon ds are F_t and F_n, the radius of curvature is r, and $ds = r d\varphi$, φ being the complement of the slope angle. [Under the tacit assumption that if the part BM is cut away its action on AM is equivalent to that of $-T$ and $-V$ acting at M,] EULER resolves the forces acting at the point $s + ds$, denoted by
11 m in the figure, into components tangential and normal to the tangent at M. The vanishing of tangential force, normal force, and moment then lead to the following *exact statical equations* in intrinsic form :

$$\frac{dT}{ds} + V\frac{d\varphi}{ds} = -F_t\,,$$
$$\frac{dV}{ds} - T\frac{d\varphi}{ds} = -F_n\,, \tag{562}$$
$$\frac{d\mathscr{M}}{ds} - V = 0\,.$$

[Thus EULER tacitly assumes that the rod is not subject to any couples applied along its length.]

29 If $F = 0$, by elimination of $d\varphi/ds$ between $(562)_1$ and $(562)_2$ we obtain

$$T^2 + V^2 = C^2,\ T = C\cos\varphi,\ V = C\sin\varphi, \tag{563}$$

where a constant of integration has been set equal to zero by choice of the line from which the angle φ is measured. [This is a statical theorem : *When a line is subject to terminal loads only, the stress resultant is constant along it.*]

12—22 The perfectly flexible case is obtained by setting $V = 0$; (92) follows at once. The equations for the general elastica are obtained by adjunction of the hypothesis (69). [It is clear that all differential equations refer to the *deformed* band; when $\mathscr{B}$, the absolute elasticity, is not constant, it will generally be an assigned function of S, the arc length in the undeformed band, though in most problems treated in the eighteenth century the elastica was taken as inextensible, $s = S$.]

There is also an analysis of the initially straight band of uniform elasticity. [Nothing new regarding the elastic curves themselves is to be expected. What is sought is the *unification* of the theory of the elastica with the general theory based upon the full statical equations (562). In particular, EULER determines the tension T and the shear resultant
13 V.] Substitution of the elastic hypothesis (89) into $(562)_3$ and then into $(562)_1$ leads to an equation for T which may be integrated; elimination of T by $(562)_2$ then yields a differential equation for the slope,

$$\mathcal{B}\frac{d^3\varphi}{ds^3} + \tfrac{1}{2}\mathcal{B}\left(\frac{d\varphi}{ds}\right)^3 = -\frac{d\varphi}{ds}\int F_t ds - F_n\,, \tag{564}$$

including all known cases of the initially straight elastic bands and of flexible lines. When $F_n = F_t = 0$, the equation for T becomes 28

$$T = B - \tfrac{1}{2}\mathcal{B}\left(\frac{d\varphi}{ds}\right)^2 = \mathcal{B}\frac{d^3\varphi}{ds^3}\Big/\frac{d\varphi}{ds}\,. \tag{565}$$

The equation $(565)_2$ may be integrated once. Putting $u = d\varphi/ds$, we may write the result in the form

$$ds = \frac{2du}{\sqrt{C + \frac{4B}{\mathcal{B}}u^2 - u^4}}\,. \tag{566}$$

Putting $(563)_1$ into (89) also yields an equation which can be integrated; the result is 30

$$r = \frac{\sqrt{\mathcal{B}}}{\sqrt{B - 2C\cos\varphi}}\,. \tag{567}$$

Since $dx/ds = \sin\varphi$ and $r = ds/d\varphi$, we may integrate (567) and obtain 31—32

$$x = \frac{\sqrt{\mathcal{B}(B - 2C\cos\varphi)}}{C} + \text{const.} \tag{568}$$

Take $x = 0$ at a point where the curve is normal to the axis of x; by rearrangement of constants we get

$$\cos\varphi = 1 - \frac{x}{a} - n\frac{x^2}{a^2}\,,$$

$$V = \frac{2n\mathcal{B}}{a^2}\sin\varphi\,, \qquad T = \frac{2n\mathcal{B}}{a^2}\cos\varphi\,, \qquad \mathcal{M} = \frac{\mathcal{B}\sqrt{4n + 1 - 4n\cos\varphi}}{2n\sin\varphi}\,, \tag{569}$$

$$\frac{dy}{dx} = \frac{(a^2 - ax - nx^2)\mathcal{B}}{\sqrt{2a^3x + (2n-1)a^2x^2 - 2nax^3 - n^2x^4}}\,.$$

Writing (567) in terms of the constants a and n, we obtain 33

$$r = \frac{a}{\sqrt{4n + 1 - 4n\cos\varphi}} = \frac{a}{\sqrt{1 + 8n\sin^2\frac{1}{2}\varphi}}\,. \tag{570}$$

Eliminating $\cos\varphi$ by $(569)_1$ yields

$$x = \frac{a^2}{2nr} - \frac{a}{2n}\,, \tag{571}$$

"a remarkable property common to all elasticas." [This is of course the law (171) that was used as the starting point in EULER's characterization of elastic curves.]

The "second general problem" is to find the equations of motion. The method used is 34—40 the balance of linear momentum, according to EULER's "first principles" (above, § 35). To the actual forces acting upon ds are to be added "the forces instantaneously required to produce the motion," *viz* [in general units] $-\sigma\dfrac{\partial^2 x}{\partial t^2}$ and $-\sigma\dfrac{\partial^2 y}{\partial t^2}$. Thus from $(562)_{1,2}$ we obtain

$$(572)\qquad \begin{aligned} \frac{\partial T}{\partial s} + V\frac{\partial \varphi}{\partial s} &= \left(-F_x + \sigma\frac{\partial^2 x}{\partial t^2}\right)\sin\varphi + \left(-F_y + \sigma\frac{\partial^2 y}{\partial t^2}\right)\cos\varphi\,,\\ \frac{\partial V}{\partial s} - T\frac{\partial \varphi}{\partial s} &= \left(-F_y + \sigma\frac{\partial^2 y}{\partial t^2}\right)\sin\varphi - \left(-F_x + \sigma\frac{\partial^2 x}{\partial t^2}\right)\cos\varphi\,. \end{aligned}$$

The equation of moments, $(562)_3$, remains unchanged, because the assigned forces do not occur in it.

42 To get the usual equation for the vibrating string, set $F_x = F_y = V = 0$ and assume $\partial^2 x/\partial t^2 = 0$, $\cos\varphi = 0$. From $(572)_1$ follows $T = \text{const.}$; "thus during the motion the tension of the thread is assumed to be kept constant." With $\cos\varphi = \dfrac{\partial y}{\partial s}$ and $\sin\varphi = 1$, $(572)_2$ reduces to the usual equation (251) with s replacing x.

43 To get the equation for small oscillations of an elastic band, we proceed as above except that $V = \mathcal{B}\dfrac{\partial^2\varphi}{\partial s^2} = -\mathcal{B}\dfrac{\partial^3 y}{\partial s^3}$, so that

$$(573)\qquad \frac{\partial T}{\partial s} + \mathcal{B}\frac{\partial^2 y}{\partial s^2}\frac{\partial^3 y}{\partial s^3} = 0\,,\qquad -\mathcal{B}\frac{\partial^4 y}{\partial s^4} + T\frac{\partial^2 y}{\partial s^2} = \sigma\frac{\partial^2 y}{\partial t^2}\,.$$

The integral of $(573)_1$ is

$$(574)\qquad T = B(t) - \tfrac{1}{2}\mathcal{B}\left(\frac{\partial^2 y}{\partial s^2}\right)^2.$$

Eliminating T "leads to a differential equation of fourth order such as has been found by those who have treated this problem in greater detail." [This equation is

$$(575)\qquad \mathcal{B}\frac{\partial^4 y}{\partial s^4} + \tfrac{1}{2}\mathcal{B}\left(\frac{\partial^2 y}{\partial s^2}\right)^3 + \sigma\frac{\partial^2 y}{\partial t^2} = B\frac{\partial^2 y}{\partial s^2}\,.$$

So far as I know, it had not been derived previously. EULER's formula (565) shows that the tension is of two kinds: The part $B(t)$, an arbitrary uniform tension independent of the transverse motion and possible because longitudinal inertia and elastic force has been neglected, and a pressure $\tfrac{1}{2}\mathcal{B}\left(\dfrac{\partial^2 y}{\partial s^2}\right)^2$ arising from the transverse acceleration. When both these tensions are neglected, as is usual, (575) reduces to (273); if $\mathcal{B} = 0$, (575) reduces to (251).

While no new special problems are solved by this memoir, its value for co-ordination of the whole theory of deformable lines is great. More than this, it achieves a major step toward *the concept of stress* by supposing that *the action of one part of the line upon the other is* *equipollent to a force and a couple* and in recognizing that when the line is not perfectly flexible, *a cross force is required*. Thus while the concept of shear strain remained unformulated, the simplest case of *shear stress* at last takes its place in the equations of mechanics.

Moreover, in respect to the laws of mechanics this paper marks a turning point, for it is the first work on deformable continua in which *the principles of linear momentum and moment of momentum appear on a par*, independent and separately necessary. Thus is closed the dichotomy signalled by JAMES BERNOULLI's "two keys" (above, p. 89). While in his first attempt EULER had shown that the balance of moments, in the integral form (91), suffices to derive equations of flexible lines as well as of the elastic band, the part to be taken by the balance of forces in the general theory, and, in particular, in the theory of the elastica, remained mysterious. Here it is made plain that *neither principle, by itself, suffices* except in special cases. This is the result of years of trial and reflection[1]). EULER's final formulation of the general principle of moment of momentum has been described elsewhere[2]).

But EULER remained unsatisfied. In the paper just described, written in his sixty-fifth year, he had finally shown how everything concerning deformable lines follows from *differential statements* of the principles of linear momentum and moment of momentum. But what of the old integral equation (91) that had served him so well for nearly half a century?] The purpose of his next paper, *On the two methods of determining the equilibrium and motion of flexible bodies and on their extraordinary agreement*[3]), is to obtain *integral equations* expressing the same mechanical principles. [While he phrases many of his results here in

1) We mention some of the many examples that may be noticed.

1. In E174, from 1744, the principles of linear and angular momentum are invoked independently to derive general equations of motion for a system of linked bars (above, pp. 223—229).

2. On pp. 268—269 of Notebook EH5 (c. 1750), EULER gives "another method for finding the curves of flexible wires. This method is HERMANN's" [and JAMES BERNOULLI's] (*cf.* above, pp. 81—87). Then he solves the same problem by balance of moments and compares the two methods.

3. After calculating on p. 181 of Notebook EH8 (1759—1760) the moments acting on a line subject to arbitrary load, on p. 181a EULER obtains results equivalent to (576) with $\boldsymbol{F}$ replaced by $-\sigma\,\partial^2 x/\partial t^2$ and with $\mathcal{M}$ given by the elastic hypothesis (89). "This method is to be preferred to that in which the tensions of the thread are considered, since the tension cannot be applied to the elasticity."

4. On p. 35 of Notebook EH7, just following a remark dated 26 April 1763, EULER states a theorem: For a body in general, equilibrium of forces does not suffice; equilibrium of moments is necessary as well. The latter principle is stated in the form $\int(\boldsymbol{r}\times d\boldsymbol{f})=0$, where $d\boldsymbol{f}$ is the element of applied force.

2) See § IX of my "*Neuere Anschauungen über die Geschichte der allgemeinen Mechanik*," Z. angew. Math. Mech. **38**, 148—157 (1958).

3) E481, "*De gemina methodo tam aequilibrium quam motum corporum flexibilium determinandi et utriusque egregio consensu*," Novi comm. acad. sci. Petrop. **20** (1775), 286—303 (1776) = Opera omnia II **11**, 180—193. Presentation date: 31 October 1774.

4—6, 24—25 terms of elastic bands, his ideas are purely statical, and we shall so express them.] The statical notion leading to (91) is

$$-\int dy \int F_x ds + \int dx \int F_y ds = \mathcal{M} \,. \tag{576}$$

When $\mathcal{M} = \mathcal{B}\dfrac{d\varphi}{ds}$, (576) yields (91), which suffices to determine the shape of the band, but "the accompanying . . . features, such as the tension . . . and the normal force . . ., cannot be found in this way, which defect I have tried to supply by the other method . . ."

We are now to show that the new method, expressed by (562), also implies (576).
13—15 We multiply $(562)_1$ and $(562)_2$ by $\cos\varphi$ and $\sin\varphi$, respectively, add and subtract, and integrate, obtaining

$$\begin{aligned} T\cos\varphi + V\sin\varphi &= -\int F_x ds \,, \\ T\sin\varphi - V\cos\varphi &= -\int F_y ds \,, \end{aligned} \tag{577}$$

[generalizing (39)]. Hence

$$\begin{aligned} T &= -\cos\varphi \int F_x ds - \sin\varphi \int F_y ds \,, \\ V &= -\sin\varphi \int F_x ds + \cos\varphi \int F_y ds \,. \end{aligned} \tag{578}$$

16 Substituting $(578)_2$ in $(562)_3$ and integrating yields (576). Euler explains that integrals such as $\int F_x ds$ represent the total force and thus "such finite forces as act upon the end
17 of the band can be included" [*i. e.*, the integrals are taken in the Stieltjes sense]. The equilibrium of each element of the band requires that the forces T, V representing the action of one part on the other change in sign but not in magnitude when the parts are interchanged. [That is, if $\boldsymbol{S}_+$ is the stress vector $T\boldsymbol{t} + V\boldsymbol{n}$ representing the action of the material to one side of a given point on that to the other, while $\boldsymbol{S}_-$ results when the roles of the two sides are interchanged, then Euler asserts that

$$\boldsymbol{S}_+ + \boldsymbol{S}_- = 0 \text{ .]} \tag{579}$$

23 Euler shows that the moment $\mathcal{M} = vV$ may be replaced by the action of two forces at arbitrarily assigned points on the tangent. These forces remain finite and non-vanishing even if $v = \infty$, $V = 0$ [*i. e.*, in the case of a couple].

59. Coulomb's introduction of interior shearing stress (1773). [We have seen above, p. 113, that Parent in 1713 had conceived the interior shearing stress in a beam, but he was unable to use it, and no notice was taken of his work. Some five years after Euler had published his general equations (562) for deformable lines, which rest upon his introduction of the cross force or resultant shear force V,] there appeared an important memoir of Coulomb, *Essay on an application of the rules of maxima and minima to some statical problems relevant to architecture*[1]), where, among other things, we find the reintro-

1) "*Essai sur une application des règles de maximis et minimis à quelques problèmes de statique,*

duction and fruitful use of interior shear stress. COULOMB states that this material had been composed for his own use some years before, [and there is every reason to believe his work independent of EULER's, since it is different in character and in content]. First, V
COULOMB tests specimens of "a fine grained, homogeneous white stone" for rupture: "I wished to see if in breaking a solid of stone by a force directed along its plane of rupture the same force would have to be employed as when breaking it . . . by a stress[1]) perpendicular to that plane (Figure 107). [Thus COULOMB compares *breaking in tension* with *breaking in shear*.] For the former, he obtains a value of 215 lbs./sq. in., [this being the first occasion on which an experimenter reports a result in terms of stress rather than force,] and for the latter, 220 lbs./sq. in. "I have repeated this experiment several times, and I have found almost always that a greater force is needed to break the solid when the force is directed along the plane of rupture than when it is perpendicular to this plane. Nevertheless, since this difference is here but $\frac{1}{44}$ of the total weight and is often even less, I have neglected it in the following theory."

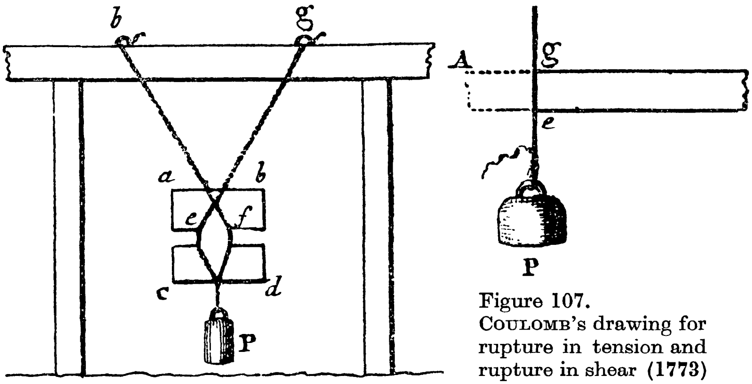

Figure 107.
COULOMB's drawing for rupture in tension and rupture in shear (1773)

[Thus COULOMB assumes that rupture occurs, other things being equal, when a certain *maximum stress* is attained.] To visualize the effect of the bending moment, COULOMB VII
says "it is plain . . . that all the points of the line AD (Figure 108) resist in such a way as to hinder the weight from breaking the solid; that, consequently, an upper portion AC of this line is stressed by a traction along QP, while the lower part is stressed by a pressure along $Q'P'$." The curve $BMCe$ is the curve of tensions PM, $P'M'$; [no curve is drawn to represent the variation of the shear stress MQ, $M'Q'$, but COULOMB plainly allows it to vary arbitrarily]. COULOMB then applies the principles of statics to the portion of the beam from AD on. If we take the x co-ordinate as vertical, y as horizontal, then $dx = Pp$, and if we write T_x for COULOMB's PM, $P'M'$ and T_y for his MQ, $M'Q'$, then his results are

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relatifs à l'architecture," Mém. math. phys. acad. sci. divers savans [7] (1773), 343—382 (1776). I have found helpful the account of this paper given by TIMOSHENKO, §§ 12, 14, and 15 of *op. cit. ante*, p. 11.

1) From this point onward I translate the French "effort" as "stress", but the reader is not to infer therefrom that a precise definition has been given by COULOMB or any other old author.

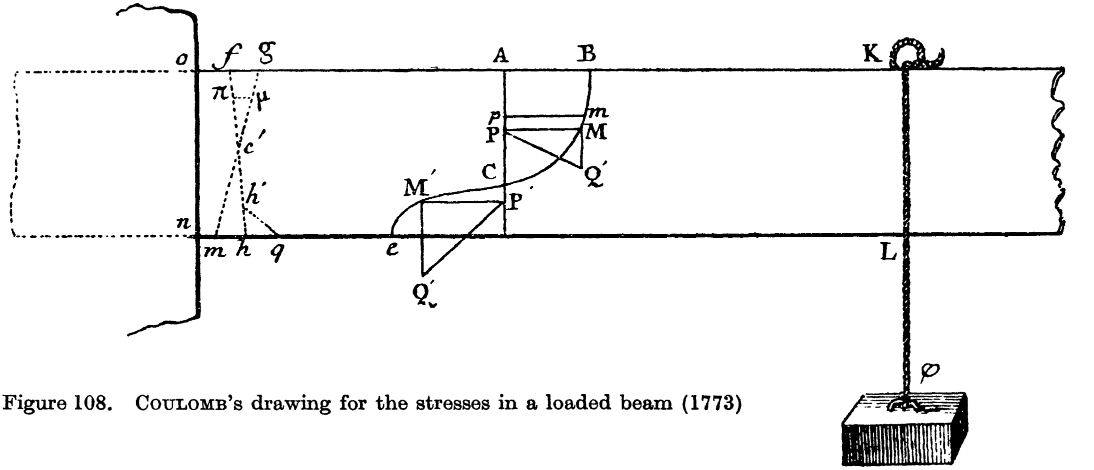

Figure 108. COULOMB's drawing for the stresses in a loaded beam (1773)

equivalent to[1])

$$
(580)\qquad \begin{aligned}
&\int_0^D T_y\,dx = 0\ ,\\
&\int_0^D T_x\,dx = \varphi\\
&\int_0^D x T_y\,dx = (l-y)\,\varphi\ ,
\end{aligned}
$$

where $l - y$ is the distance from the section AD to the line KL along which the load φ acts. COULOMB interprets $(580)_1$ as a statement that "the area ABC of the tensions equals the area CeD of the pressures." [The moment relation $(580)_3$ is only VARIGNON's "fundamental rule" (58), which is due in principle to LEIBNIZ and which all writers on this problem had used explicitly or implicitly. The equation of horizontal forces $(580)_1$ is PARENT's rule (above, p. 113),] which COULOMB interprets [just as had PARENT] : *The area under the curve of tensions must vanish.* [What is new is the recognition that *there must be shearing stress T_x in order to support the weight.* PARENT had mentioned the possibility of shearing stress but had not given it prominence or stated any condition regarding it. The system (580) constitutes a fundamental case of the *stress principle*, which is to be formulated in general terms by CAUCHY in 1822[2]).]

In the case of a "perfectly elastic" body, "that is, one which when loaded along its length is compressed or extended proportionally to the force which compresses or dilates it," COULOMB considers fg to be the elongation of the topmost fibre, mh the contraction of the lowest. Since the areas of the two triangles fge and emh must be equal, we have $fe = \frac{1}{2} fh$,

1) In terms of the breadth B and CAUCHY's stress tensor t_{ij}, we have $T_x = Bt_{yx}$, $T_y = Bt_{yy}$.

2) *Cf.* my note, "*Zur Geschichte des Begriffes ‚innerer Druck‘*," Phys. Blätter **12**, 315—326 (1956).

[*i. e.*, as PARENT had shown, if HOOKE's law holds, *the neutral line must be the middle line*,] and the relation $(580)_3$ gives[1])

$$\frac{1}{6}D^2(T_y)_{\max} = l\varphi \ . \tag{581}$$

[This result is equivalent to the special case of PARENT's formula (71) when $D_t = \frac{1}{2}D$; *cf.* also the remarks of JOHN III BERNOULLI (above, p. 389).] If, on the other hand, the solid remains rigid up to rupture, and if one assumes the body turns about h as a fulcrum in breaking, then $T_y = \text{const.}$, and $(580)_3$ yields

$$\frac{1}{2}D^2T_y = l\varphi \ , \tag{582}$$

[equivalent to GALILEO's formula (11); *cf.* the work of VARIGNON, above, p. 104]. COULOMB decides to apply (582) to his experiments. For the stone which broke in tension or shear at about 215 lbs./sq. in., (582) predicts that a beam of dimensions $D = 1''$, $B = 2''$, $l = 9''$ should break when subjected to a terminal load of 24 lbs., but in fact 20 lbs. suffices. "Therefore, in the breaking of stone, one cannot suppose either that the stiffness of the fibres is perfect, or that the fulcrum is precisely at h." [In fact, the hypothesis $T_y = \text{const.}$ contradicts the condition $(580)_1$ unless, as suggested by JOHN III BERNOULLI (above, p. 389), we suppose there is a suitable concentrated force (infinite stress) along the lowest fibre. Notice also that COULOMB expressly rejects HOOKE's law; if we accept COULOMB's hypothesis that rupture occurs when a certain maximum stress is attained, then in fact his experimental values conform more nearly to GALILEO's formula than to the result (581) inferred from HOOKE's law.]

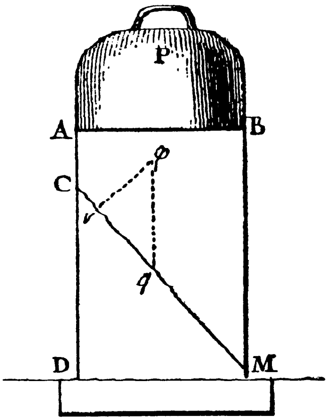

Figure 109. COULOMB's drawing for rupture in compression through failure in shear (1773)

To construct a model for the rupture of a pillar in compression, COULOMB divides it by a plane CM making an arbitrary angle x with the horizontal and supposes the upper part free to slide over the lower (Figure 109) except for the force of the shear stress upon it. Calling this stress σ, if it is to equilibrate the weight P we have $\sigma \cdot (\text{area of } CM) = \sigma A \sec x = P \sin x$, where A is the area of the base. Hence VIII

1) Recall that $T_y = Bt_{yy} = BE\epsilon_y$, where ϵ_y is the strain of the longitudinal fibre.

An attempt to locate the neutral line is made in §§ IV—VI of LAMBERT's paper of 1777, cited above, p. 325. LAMBERT takes the law of tension for the fibres of a bent beam as a power series in the elongation. Calculating the moment about the neutral line, he asserts that this moment must be a minimum, "since the beam bends in the fashion that is *easiest of all*." There results an equation for determining the position of the neutral line; for small bending, it turns out to be the central line. In § XVIII LAMBERT remarks that EULER "left undecided" the question of the neutral line, but LAMBERT's other remarks here show that he failed to follow EULER's derivation of (87).

$$\frac{P}{A} = \frac{2\sigma}{\sin 2x} \ . \tag{583}$$

Thus if a material fails when a certain shear stress σ is reached, then in order to prevent failure it is necessary that

$$\frac{P}{A} < \frac{2\sigma}{\sin 2x} \quad \text{for all } x \ . \tag{584}$$

The right-hand side is a minimum when $x = 45°$, and hence $P/A = 2\sigma$. [As a theory of failure, this is not very impressive, but we recognize the result as the first occurrence of the theorem of HOPKINS[1]).] If we take account of frictional force, which according to AMONTONS is proportional to the normal force, in place of (583) we get

$$\sigma A \sec x + \mu P \cos x = P \sin x \ , \tag{584A}$$

where μ is the coefficient of friction. The minimum of P is now attained when

$$\tan x = \frac{1}{\sqrt{1+\mu^2} - \mu} \ . \tag{585}$$

For bricks he has found that $\mu = \frac{3}{4}$; this gives $\tan x = 2$ and $P/A = 4\sigma$, and this [mere curve-fitting] agrees fairly well with MUSSCHENBROEK's results on the collapse of square brick pillars[2]). However, COULOMB regards this agreement as accidental since MUSSCHENBROEK asserted that a column does not break until it starts to bend and found that the buckling load is given by (94), while COULOMB's theory yields a breaking load that is proportional to the area but independent of the length. [Since his theory neglects deformation altogether,] it is easy for COULOMB to find "the height to which one can raise a tower without its falling by its own weight" and to solve some related problems. [COULOMB does not mention any other theories of collapse, but he must surely know EULER's theory, and he makes it plain that he considers any dependence of the strength of a column on its length to be wrong; apparently he considers only such materials as masonry and brick.

This memoir of COULOMB has been praised very highly. On the basis of it TIMOSHENKO[3]) asserts, "No other scientist of the eighteenth century contributed as much as

1) *I. e.*, the extreme shear stresses occur across the planes making equal angles with the principal axes of stress, and the magnitudes of these extremal shears are $\pm \frac{1}{2}(t_1 - t_2)$, $\pm \frac{1}{2}(t_2 - t_3)$, $\pm \frac{1}{2}(t_3 - t_1)$, where the t_i are the principal stresses.

V. §§ 4—5 of W. HOPKINS, "*On the internal pressure to which rock masses may be subjected, and its possible influence in the production of the laminated structure,*" Trans. Cambr. Phil. Soc. 8 (1844—1849), 456—470 (1847).

2) Where these experiments are reported I do not know. The work of MUSSCHENBROEK we have described above (p. 153) refers to wooden struts.

3) § 12 of *op. cit. ante*, p. 11.

COULOMB to the science of mechanics of elastic bodies." Opinion aside, this estimate is misleading, since, as we have seen, there is scarcely a hint of elasticity in the paper[1]). In one respect COULOMB's work, since it reverts to purely statical arguments in the manner of GALILEO, LEIBNIZ, and VARIGNON, is a step backward. In another, however, it is of the very greatest importance, since it reintroduces PARENT's concept of interior *shear stress*, essential to any complete theory of deformable bodies. This aspect justifies the more definite praise of ST. VENANT[2]), which is restricted to § VII: "It is only in our century that his paper, which in the three pages entitled *Remarks on rupture* contains so many things on this subject, has finally been studied and understood."]

60. EULER's formal definition of the modulus of extension and final scaling laws for the frequencies and buckling loads of straight rods (1774, 1776). Only in the last of his papers on vibrating rods[3]), written in 1774, does EULER at last derive scaling laws for the frequencies as functions of the cross-section. Here he takes the absolute elasticity $\mathcal{B}$ as bc^4, "where b is a quantity depending upon the nature of the material of the rod" and c is "the diameter of the area." [This is of course consistent with $(87)_2$, but it is not clear what EULER means by c.] At first he takes $A = c^2$ and finds that for a given mode

(586) $$\nu \propto \frac{c}{l^2}$$

in rods made of the same material and having similar cross-sections. [The general law which follows at once from $(87)_2$ and (136) is

(587) $$\nu = \frac{\zeta^2}{2\pi} \cdot \frac{k}{l^2} \sqrt{\frac{EA}{\sigma}} = \frac{\zeta^2}{2\pi} \cdot \frac{k}{l^2} \sqrt{\frac{E}{\varrho}} \,,$$

1) We have discussed only that part of its contents referring to problems which, if properly treated, would involve deformation. The remainder of the paper concerns two problems of pure statics: the pressures exerted by earth masses and the stability of arches composed of finite rigid bodies. Much of this is described by TIMOSHENKO, *loc. cit.*

COULOMB deals with only one case of truly deformable media, *viz*, the arch of infinitesimal links, or, in other words, the perfectly flexible line. He derives the differential equation immediately (§ XVII) from the observation that the resultant force on any finite section is parallel to the tangent at the end; as he says, this generalizes the result of GREGORY (above, p. 80) and yields the appropriate special case of EULER's result (92). He is just in remarking that his method is entirely different from EULER's, since in fact it is JOHN BERNOULLI's (above, p. 75).

2) Quoted by TODHUNTER, § 117 of *op. cit. ante*, p. 11.

3) §§ 4 and 45—46 of E 526, cited above, p. 326. EULER uses here the special units he has used since 1750; *cf.* pp. XLII—XLIV of my Introduction to L. EULERI Opera omnia II 12.

In all of EULER's earlier treatments of the subject, including even E 443 (described above, pp. 323—325), which was written in 1772, he avoided mentioning the dependence of frequency upon the cross-sectional form or area. In these earlier papers he made it plain that for each band the modulus $\mathcal{B}$ was to be determined from experiment by use of (137) or of one case of (136).

where k is the radius of gyration of the cross-section about the central line and ζ is the appropriate root of the appropriate characteristic equation.] EULER says that for a rectangular beam we should take $c = D$, and the breadth plays no part; thus a non-square rectangular beam has two different sequences of frequencies, according as the vibrations occur parallel to one face or the other. For a circular beam, $c = \frac{3}{4}d$, where d is the diameter. [These results give the correct scaling laws, but I cannot understand what EULER now means by c.]

We have seen that the concept of a stress-strain relation, as distinct from a formula for force as a function of elongation, may be traced back to JAMES BERNOULLI (above, p. 106); also, that an elastic constant having the dimensions [Force]/[Area] and denoting the stress produced by a definite strain occurred explicitly in EULER's unpublished work of 1727 (above, p. 145) and in papers published by EULER and by JORDAN RICCATI in 1766 and 1767, respectively (above, pp. 384 and 388). [In effect, the modulus of extension, now called "YOUNG's modulus", was in use, but its importance had not been grasped.] In 1776
15 EULER introduced it in precisely its modern sense[1]). To discuss the elongation of a column, EULER considers "a little cylindrical or prismatic wand from the same material." For a given attached weight, "the elongation φ . . . is proportional to the length f of the rod," so that if we put $\varphi = f\delta$, "there will then be a relation between the attached weight P and the letter δ, so that there is no further need to bring the length f into the calculation.
16 It is also plain that when the weight P increases, the letter δ ought to increase also, until finally the wand breaks. But so long as the elongations are small enough, it cannot be doubted that the value of the letter δ will be proportional to the weight P, since in all tiny changes of this kind the effect is always proportional to the cause. Finally, it is also evident that if the wand were twice as thick, then twice as great a weight would be required for producing the same elongation," so that the weight P is always proportional to
17 g^2, where g^2 is the thickness [*i. e.* area]. In order to eliminate the thickness, "instead of the weight P we may conveniently substitute the weight of a volume of the same material, which therefore may be represented by a like wand whose length is p, so that $P = pg^2$, that is, so that P equals the weight of a column made of the same material whose base is g^2 and whose altitude is p." The assumed proportionality of P to δ then assumes the form

$$p = h\delta \,, \tag{588}$$

"where h is a certain length, which will be the same for all wands made of the same material, since it depends neither on the length f nor on the thickness g^2. Thus we shall be able to regard this length h as a true measure of the tenacity or firmness of the material, what-

1) In E 508, cited above, p. 358.

ever be the case in question, so that a certain determined length h belongs to each and every material."

[As he usually did, EULER is measuring weights in units of length, so that in general units $p = P/(\varrho g)$ and $h = E/(\varrho g)$, where E is what is now called "YOUNG's modulus." That is, EULER sees that *a relation between stress and strain* is independent of the dimensions of the specimen, while a relation between force and extension is not. EULER is thus the first to grasp the importance of fully isolating *an elastic property of a material* as distinct from elastic properties of particular specimens of a material. It is most regrettable that YOUNG's name has been attached to the modulus E, since YOUNG's concept, while worded similarly to EULER's, is more primitive as well as somewhat vague and does not enjoy independence from the specimen[1]). If any name in addition to EULER's were to be associated justly with the modulus of extension, it should be JAMES BERNOULLI's.]

With this clear concept in hand, EULER is then able, at last, to give a clear and general 18—20

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1) "The modulus of elasticity of any substance is a column of the same substance, capable of producing a pressure on its base which is to the weight causing a certain degree of compression, as the length of the substance is to the diminution of its length." This definition is given in § 319 of "*Mathematical elements of natural philosophy,*" *A course of lectures on natural philosophy and the mechanical arts*, London (1807), 2, 1—86 = *Misc. Works* 2, 129—140. LOVE says of YOUNG's definition, "This introduction of a definite physical concept, associated with the coefficient of elasticity, which descends as it were from a clear sky on the reader of mathematical memoirs, marks an epoch in the history of the science"; see p. 5 of Vol. 1 of *A treatise on the mathematical theory of elasticity*, Cambridge (1892), p. 4 of the later editions. I hope I may be forgiven a pun: YOUNG's epochal sky, as usual, was not clear, for he had beclouded EULER's enlightenment.

TODHUNTER, § 139 of *op. cit. ante*, p. 11, says of YOUNG's work on elasticity, "The whole section seems to me very obscure like most of the writings of its distinguished author; among his vast attainments in sciences and languages that of expressing himself clearly in the ordinary dialect of mathematicians was unfortunately not included. The formulae of the section were probably mainly new at the time of their appearance, but they were little likely to gain attention in consequence of the unattractive form in which they were presented." In fact, most if not all of the contents of YOUNG's Sect. IX, "Of the equilibrium and strength of elastic substances," consists in rephrasing of results derived by EULER or JAMES BERNOULLI or DANIEL BERNOULLI.

PEARSON, footnote to § 137 of *op. cit. ante*, p. 11, remarks that YOUNG defines his modulus as a *volume*. This is not true: Perhaps following EULER, YOUNG defines his modulus as a *column*, *i. e.* as a mass of material, but unlike EULER he does not specify that it shall be a column of unit base. Thus, as was remarked by PEARSON, E is the quotient of the *weight* of YOUNG's own modulus by its cross-sectional area. Such a measure of elastic force, since it recognizes the concept of extensional strain but not that of stress, was used, as we have seen, by virtually every writer on elasticity in the eighteenth century, and YOUNG deserves no notice whatever for using it in his turn, some decades after EULER had replaced it by the more useful modulus still employed today.

While PEARSON, § 75 of *op. cit. ante*, p. 11, remarks that EULER's h in the paper we are discussing "is the constant now termed the modulus of elasticity," he does not see fit to mention this in his discussion of the inadequate definition of YOUNG.

derivation of the basic formula (87), for the case of a straight beam[1]). [The idea, of course, is the same as that in his papers of 1727 and 1760 (above, pp. 144, 388). As in all of EULER's
24 work, the neutral line is taken tacitly as being on the concave side of the beam.] From $(87)_2$ it follows[2]) that the various formulae for the buckling loads of beams of similar cross-section of typical linear dimension d always fall into the form

$$(589) \qquad P_c \propto \frac{EI}{l^2} \propto \frac{Ed^4}{l^2} \ .$$

[This is the first appearance of the full correct scaling law for buckling; *cf.* (426).]

—28 EULER compares the values of $h\left[=\frac{E}{\varrho g}\right]$ as determined by the buckling load and by elastic extension. For the former, he uses the experimental data of MUSSCHENBROEK on rupture of wooden sticks $\frac{1}{2}''$ or $0.7''$ square and $4'$ long, concluding from the results that $h = 2774980'$. [If we assume the density of the wood to be 40 lbs./ft.³, then this statement is equivalent to $E = 7.7 \times 10^5$ lbs./in.², a respectable value.] EULER infers that if a load equal to the breaking load were applied in tension, the resulting strain ϵ would be 0,00069. [This seems to be the first occurrence of numbers indicating that very considerable stresses,
29 such as those occurring at rupture, may produce only minute strains.] EULER cites MUSSCHENBROEK as asserting that according to his experiments, the breaking loads for some woods vary as d^4, for others, such as oak, in a lesser ratio (see above, pp. 152—153). EULER therefore recommends that proper corrections be inferred from "numerous experiments".

Writing in 1778, EULER expresses a view toward rupture of columns which had been adopted tacitly in his earlier work and is often shared today[3]): "... it is convenient to

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1) *Cf.* also § 21 *et seqq.* of E 510 (cited above, p. 363), where EULER writes $\mathcal{B} = A^2 e$, where "e is a line proportional to the absolute elastic force."

The discussion of the law of bending given by JORDAN RICCATI in 1782 (§§ III—IV of *op. cit. ante*, p. 328) rests upon a somewhat obscure application of HOOKE's law to the fibres of the bent beam. RICCATI obtains results equivalent to

$$\mathcal{B} \propto EAD^2 \ ,$$

where E, the "rigidity" [*i. e.* "YOUNG's modulus"], depends only on the material. This is correct, but it falls short of (87). RICCATI seems to be unaware of the work of EULER and JOHN III BERNOULLI on this problem, since in 1782 he criticizes EULER for taking $\mathcal{B} \propto d^3$, while from 1760 onward EULER had corrected this to $\mathcal{B} \propto d^4$ (*cf.* above, pp. 347, 388 and the text on this page).

2) In §§ 21—23 EULER finds that $\mathcal{B} = \frac{1}{3} ED^3 B$ for a rectangular cross-section, $\mathcal{B} = \frac{5}{64}\pi E d^4$ for a circular one, but these results are not new, having been obtained earlier by PARENT, by EULER himself, and by JOHN III BERNOULLI.

3) § 3 of E 510, cited above, p. 363.

Cf. the remark of v. MISES, *op. cit. ante*, p. 212, that a small increase in load beyond the buckling load causes a large deflection of the beam. Thus "... a rod subject to compressive load almost always breaks as soon as the buckling load is exceeded."

replace a column in imagination by an elastic band of the same stiffness, for it is always possible to conceive of an elastic band that resists bending as much as a column resists breaking, the whole difference being that the elastic band is really bent, while a column acted upon by the same forces will break."

[We have seen that the full scaling law (587) for transverse vibrations of bars follows at once from combination of two of EULER's results obtained at different periods, but he himself derived only the more special[1]) rule (586). After years of uncertainty regarding the correct dependence of $\mathcal{B}$ on the cross-sectional form, using sometimes correct formulae and sometimes incorrect ones[2]), he finally decided in 1776 that his old formula $(87)_2$ was correct. His last paper on the vibrations of rods, however, had been written two years before.]

61. COULOMB's experiments on torsion. [We have seen that in 1764 EULER by consideration of a rather precarious discrete model had been led to infer that the torque of a twisted rod is proportional to the sine of the angle of torsion. This work attracted no notice.] The last contribution to the laws of elasticity in our period is made by COULOMB's experimental researches on torsion. These are first reported in his essay for the Paris prize of 1777, *Researches on the best means of making magnetic needles* . . .[3]). Chapter III is called "Experiments and theory on the force of torsion of hairs and silk threads . . ." In the first experiment, a copper ball is hung from a silk thread. The torsional vibrations are found to be isochrone, even when the amplitude is as much as six or seven full turns. Hence "the forces of torsion . . . are necessarily proportional to the angle of torsion."

According to the second experiment, the tension of the thread, caused by the suspended 44
weight, "does not at all influence the force of torsion. One must remark nevertheless that if the weight of the body is very much increased, and if the hairs or silk threads are ready to break, this same law does not hold exactly. The force of torsion seems then much diminished, the oscillations are no longer isochrone, the times of the large ones then being much

1) The rule (586) seems to be limited to beams of similar cross-section, while (587) exhibits the precise attribute of the cross-section that is relevant. *E. g.*, the breadth of a rectangular section is irrelevant.

2) For example, the discussion of the dependence of $\mathcal{B}$ on B and D is better in E 303, written by 1762 and containing ideas of 1727, than in E 526, written in 1774, while shortly before writing E 303 EULER had inclined toward the rule $\mathcal{B} \propto d^3$, which derived indirectly but ultimately from GALILEO and was supported, if also indirectly, by MUSSCHENBROEK and other experimenters.

3) "*Recherches sur la meilleure manière de fabriquer les aiguilles aimantées* . . .," Mém. math. phys. divers savans 9, 165—264 (1780).

According to HOPPE, *op. cit. ante*, p. 11, COULOMB's law of torsion was given or suggested by J. MICHELL, *A treatise of artificial magnets*, Cambridge, Benthem, 1750, 81 pp., but neither in this booklet nor in MICHELL's papers in Phil. trans. London have I been able to find a word concerning torsion.

greater than those of the small ones. It happens in this case that the thread, from too great tension, loses its elasticity, much as a band retains its spring only so long as it is bent less than a certain amount." [*I. e.*, for moderate tensions the torsional moment is independent of the amount of tension, but large tension diminishes the torsional rigidity and also invalidates the linear response.

In summary, with the exception just noted, we have

$$\mathcal{M} = -n\theta \,, \tag{590}$$

where θ is the angle of torsion. Hence the equation of motion for the suspended body is[1])

$$I\ddot{\theta} = -n\theta \,, \tag{591}$$

where I is the moment of inertia of the body. Hence the frequency of torsional oscillations is

$$\nu = \frac{1}{2\pi}\sqrt{\frac{n}{I}} \,. \tag{592}$$

These formulae are discussed in more detail in COULOMB's next paper, but also here all of his inferences from experiment rest upon use of (592). No static measurements are reported.]

45 In the third experiment, "the forces of torsion . . . for equal revolutions are in inverse
46 ratio to the lengths of the hairs." In the fourth experiment, COULOMB's attempts to determine "how the diameter of the hairs influences the force of torsion" are inconclusive and hence are not reported in detail, but he finds "generally enough" that the force of torsion varies as d^3, [but this is incorrect, as COULOMB will shortly learn].

The subject is developed further in COULOMB's *Theoretical and experimental researches on the force of torsion . . .*[2]). [This paper inaugurates a new style in the subject, relating at length the numerical results inferred from a series of repetitions of the same experiment. However valuable this may be from the standpoint of scientific method, it makes for dull
IV reading.] COULOMB writes that "the forces of torsion are proportional to the angle of torsion, a supposition in accord with experiment when the amplitude of the angle of torsion
V is not too great." [Thus he has found a second circumstance that may invalidate (590)[3]).]

1) It is perhaps from this equation that LOVE, p. 4 of *op. cit. ante*, p. 403, concludes that COULOMB supposed the torsional rigidity n to be proportional to the polar moment of inertia of the cross-section. No trace of this ridiculous statement, which would make an I-beam stiffer in torsion than a circular rod of the same cross-sectional area, is to be found in either of COULOMB's memoirs on torsion.

2) "*Recherches théoriques et expérimentales sur la force de torsion, et sur l'élasticité de fils de métal: Application de cette théorie à l'emploi des métaux dans les arts et dans différentes expériences de physique: Construction de différentes balances de torsion, pour mesurer les plus petits degrés de force. Observations sur les loix de l'élasticité et de la cohérence*," Mém. acad. sci. Paris 1784, 229—272 (1787). Read in 1784.

3) In § VIII COULOMB attempts to determine the oscillation when the force of torsion is $n\theta + R(\theta)$, where R is some function such as $\mu\theta^m$.

To test (592) and determine the coefficient n, COULOMB measures the frequency of IX—X
oscillation in thirteen circumstances for wires of iron or brass of different size and length
and subject to different stretching weights. He infers that the vibrations are isochrone if XI
the angle of torsion is not too large, and the frequency is independent of the tension in the
wire, provided it suffices to keep the wire taut. "Nevertheless, by many experiments made
with very great tensions relative to the force of the metal, it seems that great tensions
diminish or change a little the force of torsion. One perceives in fact that as the tension increases, the wire elongates, its diameter diminishes, which should decrease the period of
oscillation." [While plausible, this explanation is insufficient. COULOMB has observed an
effect of non-linear elasticity related to the POYNTING effect.] "The force of reaction of XII
torsion ought to be . . . in inverse ratio to the length of the wire . . .," and that this is so,
COULOMB verifies by experiment. To consider the effect of the diameter, he says that "the XIII
moment of the reaction of torsion should increase with the thickness of the wire in three
ways." First, in a wire doubly thick there are "four times as many parts stretched by the
torsion"; second, "the mean extension of all these parts will be proportional to the diameter"; third, "the mean lever arm relative to the axis of rotation" is proportional to the
diameter. "Thus we are led to believe, according to the theory, that the force of torsion . . .
is proportional to the fourth power of the diameter," and this, too, COULOMB verifies by
experiment. His "general result," then, is XIV

(593) $$\mathcal{M} = \mu \frac{d^4}{l} \theta ,$$

"where μ is a constant coefficient which depends on the natural stiffness of each metal."
COULOMB determines [in effect] numerical values of μ for iron and brass, with the result XV—XVI
$\mu_{\text{iron}} / \mu_{\text{brass}} \approx 3\frac{1}{3}$.

In a long series of experiments COULOMB finds that when a wire is twisted more than XX—XXVIII
a certain angle, [the elastic limit in torsion,] "displacement of the center of torsion"
[*i. e.* permanent set] results. This set is "rather irregular" until a certain greater angle is
reached, after which it is about the same for all angles, until the wire breaks. The "reaction
of torsion," *i. e.* the constant μ in (593), is little changed. Measurements of the decreases of
amplitude in successive vibrations show that these are unaffected by the size and shape of
the suspended body and hence are not due to air resistance but to "the imperfect elasticity
of torsion" (§ II); for small enough amplitudes, they are proportional to the amplitudes.
Also, if a wire is twisted initially far beyond the elastic limit in torsion, the material be- XXX
comes susceptible of far greater twisting before it breaks. This greater hardness can be
removed by annealing.

On the basis of these results, COULOMB decides that "the constituent parts of . . . a XXXI metal have an elasticity that can be regarded as perfect . . ., but they are joined together only by coherence, a constant quantity absolutely different from elasticity." [Perhaps he means that a body has an elastic modulus and an ultimate shear stress.] "In the first degrees of torsion, the constituent parts change their shape and are extended or compressed, but the points where they adhere to each other do not change about, since the force necessary to produce these first degrees of torsion is less considerable than the force of adherence. But when the angle of torsion becomes sufficient that the force with which these parts are extended or compressed equals the coherence that unites the constituent parts, then those parts can separate or slide upon one another . . . But if this slipping . . . causes the body to compress, the extent of the points of contact increases, and the extent of the elastic range increases also. However, since the constituent parts have a definite shape, the extent of the points of contact can increase only to a certain degree, beyond which the body breaks . . . What proves still more that the cause of elasticity must be distinguished from coherence is that one can vary the coherence arbitrarily by the amount of heat treatment, but this leaves the elasticity unaltered."

XXII A wire that has been twisted until it nearly breaks can be magnetized to a greater degree than an unworked one.

XIII To confirm his ideas of elasticity and coherence, Coulomb experiments with the deflection of a steel beam by a terminal load. Heat treatment has no effect whatever on the elastic deflections. A cold worked bar remains perfectly but not linearly elastic up to rupture; the same holds true of a bar that has been tempered, but it will bear a much greater load before breaking; a bar that has been heated white hot will admit very great deflections for imperceptible increase in load beyond a certain limit and will take on a permanent set.

[Thus Coulomb is so taken with the concept of *shear stress* that he wishes to found a theory of materials upon it. It is not only because of his limited mathematical ability that such a theory is beyond him, however, but even more because he fails to introduce the concept of *shear strain*[1]). That Coulomb does *not* attempt to explain torsion in terms of extension shows his insight, at least, to be sound, but, since he makes no analysis of the deformation, his theory of torsion remains necessarily a disconnected, isolated theory of a single phenomenon.]

1) The account of Coulomb's work given by Love, p. 4 of *op. cit. ante*, p. 403, is false.

Part V. Evaluation

62. An evaluation from the eighteenth century: Lagrange's Méchanique Analitique (1788). Several parts of Lagrange's celebrated treatise[1]) concern the problems whose development we have traced. Not only is the *Méchanique Analitique* often taken as a final summary and authority for what was known concerning mechanics before 1800, but also its historical sections constitute the first history of the mechanics of continuous bodies and, for mechanics more generally, include most of the references consulted by Mach, thus being, doubly, the ultimate source of most of the historical beliefs commonly infused along with instruction in mechanics today.

In Lagrange's history of statics there is nothing concerning concrete problems or Part I, Sect. I
continuous bodies. Neither here nor in any later passage is there *a single attribution* for any of the static problems we have followed in the foregoing pages.

In contents, however, Lagrange's entire statics consists in application of the prin- Sect. V, § II, ¶ ¶ 11—28
ciple of virtual work to obtain equations for systems slightly more general than those Euler had treated by direct methods or by the principle of minimum energy. The choice of work functions is made easy by Euler's researches on the latter principle (above, p. 218). Lagrange obtains the differential equations of a linked system in space and determines
afterward the reactions against the constraints, *i. e.*, the tensions. A case of interest is that 19
of three masses when the middle one is free to slide along the cord connecting the other two.
There follows a similar treatment of a continuous line subject to arbitrary forces; several § III
kinds of end conditions, some new, are included. Among the examples is that of a catenary 38—41
on an arbitrary curved surface, an elegant problem "which would perhaps be difficult to treat by the ordinary principles of mechanics." The possibility of extensile elasticity is included.

Lagrange gives also a treatment of the skew elastica[2]). Some formal manipulations § III, ¶ ¶ 43—47 43

1) *Méchanique Analitique*, Paris, Veuve Desaint, 1788. I take no account of the extensive changes made in the second edition (2 vols., 1811, 1815 = Œuvres **11**, **12**), since these reflect to some extent the influence of a new style and of new developments occurring after the close of the period I have set, not arbitrarily, for study. An example of such a change is given above, p. 295, footnote.

2) The variational principle, of course, is evident from Euler's principle (197A), the essential thing being to calculate the virtual work done by a moment. While Euler had faced this problem squarely (above, p. 218), Lagrange avoids it: In Sect. V, § II, ¶ 26 he writes, "Suppose the rod be elastic in the point where the second body is, so that the distances [between the bodies] are constant, but the angle formed by the lines of these distances is variable, and that the effect of the elasticity consist in augmenting this angle ... Let us call the elasticity E, and the exterior angle according to which it acts, e; it is easy to conclude from what we have established in Sect. II that the moment of the force [*i. e.* virtual work of the moment] must be represented by $E de$ ..." Going back to Sect. II, we find nothing relevant. All that is given there is the definition of the "moment" [virtual work] of

I do not understand lead to [EULER's] formula (518), in a form similar to (550), for the curvature of a skew curve[1]). The resulting differential equation of the elastica is

$$(594) \qquad \boldsymbol{F} - (\lambda \boldsymbol{R}')' + (\mathcal{B}\boldsymbol{R}'')'' = 0 \ ,$$

where λ is a multiplier. Integration, vector multiplication by $\boldsymbol{R}'$, and a second integration lead to [EULER's] form (519)[2]).

Part II, Sect. I LAGRANGE's history of dynamics mentions the paper E 40 as if it did no more than apply JOHN BERNOULLI's method for finding the center of oscillation of a rigid body. Then, "It would take too long to speak of the other dynamical problems that exercised the wits of the geometers ... before the art of solving them was reduced to fixed rules," and LAGRANGE mentions condescendingly "those problems that Messrs. BERNOULLI, CLAIRAUT, and EULER proposed to each other" concerning linked or otherwise constrained systems. D'ALEMBERT's *Traité de Dynamique*, 1743, "put an end to this kind of challenges, by offering a direct and general method," *etc.*

Sect. V, § III, ¶ 27 35 LAGRANGE himself obtains the general equations of motion of a discrete system by use of the "Lagrangean equations". He treats the small motion of a weighted string hung up

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"powers" [forces] $P, Q, R, \ldots$ as being $P dp + Q dq + R dr + \ldots$, where $p, q, r, \ldots$ are "straight lines ... placed in the directions of these powers."

1) In my copy an old hand has written, "But is not this angle [of contact] *double* in a curve of *double* curvature?" This query is natural since there is no mention of the osculating plane or of any idea equivalent to it. As always, there are no figures.

I take this occasion to give notice that my copy, which I intend to dispose of in a way such as to make it permanently available, is of uncommon historical value by reason of hundreds of annotations made by a careful student whom I judge to have been a German of about 1820. These annotations, besides queries and explanations, give cross-references, references to other literature of the period, and corrections of many errors, most of which remain in the second edition and in the reprint in the *Œuvres*.

2) To derive the form (594), conversely, from EULER's form, we begin with the statical equations generalizing EULER's system (562) to three dimensions

$$\text{(A)} \qquad \boldsymbol{S}' + \boldsymbol{F} = 0 \ ,$$

$$\text{(B)} \qquad \boldsymbol{M}' + \boldsymbol{R}' \times \boldsymbol{S} = 0 \ ,$$

where, as in all work of the period, the applied couple $\boldsymbol{L}$ is assumed to be zero. Now identically

$$\begin{aligned} \boldsymbol{S} &= \boldsymbol{R}'\boldsymbol{R}'\cdot\boldsymbol{S} - \boldsymbol{R}' \times (\boldsymbol{R}' \times \boldsymbol{S}) \ , \\ &= \boldsymbol{R}'\boldsymbol{R}'\cdot\boldsymbol{S} + \boldsymbol{R}' \times \boldsymbol{M}' \ , \end{aligned}$$

by (B). Since (519) implies that $\boldsymbol{M}' = (\mathcal{B}\boldsymbol{R}'')' \times \boldsymbol{R}'$, we have

$$\begin{aligned} \boldsymbol{S} &= \boldsymbol{R}'\boldsymbol{R}'\cdot\boldsymbol{S} - \boldsymbol{R}' \times (\boldsymbol{R}' \times (\mathcal{B}\boldsymbol{R}'')') \ , \\ &= \boldsymbol{R}'\boldsymbol{R}'\cdot\boldsymbol{S} + (\mathcal{B}\boldsymbol{R}'')' - \boldsymbol{R}'\boldsymbol{R}'\cdot(\mathcal{B}\boldsymbol{R}'')' \ , \\ &= -\lambda\boldsymbol{R}' + (\mathcal{B}\boldsymbol{R}'')' \ , \end{aligned}$$

say, where $-\lambda \equiv \boldsymbol{R}'\cdot(\boldsymbol{S} - (\mathcal{B}\boldsymbol{R}'')')$. Substitution into (A) yields (594).

at one end; of a string of this kind held taut by a weight M attached to a ring that can 36
slide smoothly on a vertical rod, and here he obtains the solution for the case when M is
infinitely greater than the sum of the other weights; of the loaded string (*cf.* above, pp.
265—269); of freely linked rods; of rods with elastic joints. He ends by saying, "this sub- 37—
ject, at bottom, is merely curious," but he goes on to obtain the equations (500) for finite 41
motion of a free flexible line in space; and he discusses the vibrating string and the heavy 42
chain. "These different examples include nearly all the problems that the geometers have solved on the motion of bodies or of a system of bodies. We have selected them on purpose so that the reader may the better judge the advantages of our method, in comparing our solutions with those to be seen in the works of Messrs. Euler, Clairaut, d'Alembert, *etc.*, in which the differential equations are reached only by arguments, constructions, and analyses that are often rather long and complicated. The uniformity and the speed of progress of [our] method are what should principally distinguish it from all the others . . ."

Comparing Lagrange's treatise with the facts presented in the foregoing pages, we see that, as far as concerns our subject, his histories are worse than none, and as for his presentation of the results, it would scarcely seem possible to write so many largely correct pages yet give the reader so little profit. He who would learn the ideas of mechanics in the Age of Reason and the phenomena the theories were capable of predicting must look elsewhere than in Lagrange's book.

But this criticism grows from an attempt to share the common esteem of the *Méchanique Analitique* as a great treatise similar in scope to Newton's *Principia*. This, however, it is not, nor, it seems, did Lagrange so intend it. In the famous preface we read, "No figures will be found in this work. The methods I present here require neither constructions nor geometric or mechanical arguments, but only algebraic operations, subject to a regular and uniform process." As this sentence indicates, the work is limited, with few exceptions, to finding the differential equations of problems that fall within the scope of Lagrange's method, and this suffices to explain, on the one hand, the absence of concrete results such as determination of the shape of the elastic curves or of the frequencies and modes of a vibrating bar, and, on the other, the absence of any mention of the concepts such as stress, strain, elastic modulus, neutral line, *etc.*

Once we agree to look upon the *Méchanique Analitique*, not as a treatise on rational mechanics but rather as a monograph on one method of deriving differential equations of equilibrium and motion, it becomes a successful and sometimes elegant work. Its usefulness can be appreciated only by a person who has learned mechanics already and hence has some idea of the significance and source of the work functions Lagrange rather arbitrarily introduces.

Granted its scope, estimates of Lagrange's method must remain a matter of taste.

Although LAGRANGE himself never succeeded in deriving by it anything of any consequence which had not previously been established by EULER's direct methods, it must be conceded that the concept of the potential function, while fruitless in our period of study, led in the next century to new discoveries, particularly to GREEN's and KELVIN's concept of a conservative elastic material.

In LAGRANGE's own work is no trace of any new concept of mechanics. Rather, the *Méchanique Analitique* reflects the extreme formalism of the moribund *ancien régime.* Mechanics, however, will not be reduced to differential algebra[1]). To a reader of 1958 as well as one of 1788, almost any handful of pages from NEWTON's *Principia* contains more of mechanics than LAGRANGE's whole treatise. Despite D'ALEMBERT and LAGRANGE, the mechanics of NEWTON as reformulated and extended by the BERNOULLIS and EULER somehow made its way into the next century. This we now seek to explain.

63. Some later evaluations: CHLADNI (1802), T. YOUNG (1807), LOVE (1892). A juster estimate, though restricted to the domain of vibrations, was given by CHLADNI in 1802[2]). As those "who have contributed most to the knowledge of vibratory motions," CHLADNI names DANIEL BERNOULLI, EULER, LAGRANGE, LAMBERT, and RICCATI. [If it seems unfair that he passes over D'ALEMBERT, recall that CHLADNI is an experimenter, while D'ALEMBERT did not obtain a single result that might be compared with experiment.] For the last three he names, CHLADNI reports only generalities[3]). But he recognizes "DANIEL BERNOULLI on account of his researches on the vibrations of air in organ pipes and wind instruments, on the vibrations of a rod, which he was the first to discover, on the vibrations of a string, and on the composition of several kinds of vibration . . ." As for EULER, "Some of his writings of little use to acoustics are mentioned everywhere and are much better known than some of his much more instructive papers. In his *Attempt at*

1) It will be recalled that LAGRANGE attempted similarly to reduce analysis to differential algebra. LAGRANGE's talent for algebra was undoubtedly great, but in respect to fundamental questions of analysis or mechanics his work does not attain the logical and conceptual standards of his great predecessors. Also, the proportion of non-trivial error in LAGRANGE's calculation is high compared with other major mathematicians'. This body of error seems to have attracted little notice, so that LAGRANGE is generally given credit for having solved several problems on which his work is largely or totally wrong.

It is time for a reappraisal of the works of the French mathematicians, a reappraisal constructed, in defiance of the *généralités* from the obituaries and the descendents of the obituaries, upon critical study of the work done. I am confident such a reappraisal would much reduce the importance of D'ALEMBERT and LAGRANGE, would yield a more realistic view of LAPLACE, MONGE, and FOURIER, would raise CLAIRAUT and POISSON to their just level, and would reveal CAUCHY as the towering giant of his age and nation.

2) Pp. IV—V of *Die Akustik,* cited above, p. 329.

3) LAGRANGE is an "honorable veteran . . ., useful for the higher mechanics and analysis . . ., [who] deserves recognition also in several domains of acoustics . . ."

a new theory of music, one of his earliest writings, and also in his *Letters to a German princess* occur various things not in accord with nature; for example, his sequence of twelve tones... is not usable in practise, and the manner in which he measures by degrees the greater or lesser consonance of tonal ratios fails, for the most part, of confirmation by experience[1]). On the other hand, in several lesser known papers . . ., cited on various occasions in this book, he has made known very many theoretical discoveries fully agreeing with experiment on the vibrations of strings, rods, air, *etc.*, so that it would be most unjust to lay upon a man who has done so much the least reproach because of some few isolated incorrect assertions, many of which he himself later corrected, and others, such as the determination of the vibrations of a ring or a bell . . ., which he would probably have corrected, had he lived longer . . . Rather, we should accept his many contributions with thanks and respect."

[It is curious that even in 1802 EULER was better known in mechanics for his few errors than for his many discoveries.]

An estimate representing a different viewpoint toward mechanics, that of the English, who had done nothing in rational elasticity or the theory of vibrations since the work of TAYLOR in 1713, is furnished by the famous "Catalogue" of T. YOUNG, published in 1807[2]). This is an eccentric performance indeed. With a great show of learning, some thousands of works on the most miscellaneous subjects are listed. The elaborate classification bears so little relation to the true contents of the papers that one cannot be sure what is cited and what is not. For acoustics the list is fairly complete, but in regard to elastic and flexible systems more generally, it is nearly vacuous. For work on the catenary prior to 1743, only GREGORY is cited! There seems to be nothing on the position of the neutral line or on torsional elasticity. How little YOUNG understands all work on strength since GALILEO'S, excepting only COULOMB's, is shown by the summary on pp. 173—174. It is curious that he includes, though without attribution, JOHN III BERNOULLI's dubious theory of the rupture of a beam of triangular cross-section.

YOUNG marks with an asterisk a relatively small number of works having "superior merit and originality." For our subject, these are

1. ARISTOTLE (no reference)
2. MERSENNE's *Harmonie Universelle*
3. GALILEO's *Discorsi*

1) Such matters pertain to music rather than to science and thus are primarily expressions of taste or tradition. We remark, however, that CHLADNI's opinions are not shared by all. First, EULER's measure of accord is praised by HELMHOLTZ, *loc. cit. ante*, p. 124, who considers it just except that it disregards combination tones, which had not been observed when EULER wrote (*cf.* above, p. 271). Second, EULER's scale of twelve tones is used in some modern music.

2) Pp. 87—520 of *op. cit. ante*, p. 403.

4. Lagrange's first two papers on the vibrating string
5. Ellicott on resonance
6. Riccati's paper on the vibrations of bars
7. Coulomb's work on strength (but in reference to walls and architecture)
8. Fuss on frameworks
9. Chladni's *Entdeckungen*

An obelisc, denoting work which is "erroneous or unimportant", is put against

1. LaHire's paper of 1709
2. Euler's paper E 136 on the propagation of pulses

Dozens if not hundreds of Euler's papers are listed, but no notice is given to them[1]). The author awarded the most asterisks of all is Robison, an influential Scottish engineer and prolific writer of surveys for the *Encyclopaedia Britannica.*

It is an entirely different matter in the lectures preceding the catalogue. There, with the excuse that the catalogue follows, few references are given. The content, as far as our subject is concerned, consists in obscure and often defective proofs of propositions translated into Young's dialect[2]) from writings of Euler.

Thus it cannot be claimed that Young had not seen Euler's papers or, in respect to acoustics, that Young did not appreciate the results. Quite aside from his obvious favor toward utilitarian studies, it is difficult to conjecture any excusable reason for what Young did.

Since the great developments in the theory of elasticity were to come in France and in England, it was most natural that the next generations of researchers should look to the treatises of Lagrange and Young as summaries of preceding achievement[3]). Thus arose

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1) It is curious to note the works of Euler on other subjects to which Young gives an asterisk:

E 33, on music
E 96, on machines
E 116, on oars
E 179, on Segner's wheel
E 194, on machines
E 249, on gears
E 260, (false) theory of fluid friction
E 289, Rigid Bodies
E 330, on gears
E 342, Integral Calculus

2) *Cf.* footnote 1, p. 403.

3) Specifically, the next great advances in continuum mechanics were made by Fresnel, Cauchy, Navier, and Poisson in the period 1820—1845; these men were trained on Lagrange's *Méchanique Analitique*. The new British school, initiated by Kelland, Green, Stokes, and Kelvin

the gross neglect of nearly all of the work of the BERNOULLIS and EULER. Much of what they achieved remained unknown and was rediscovered; since most of what was transmitted by LAGRANGE and YOUNG was presented with no reference to its discoverers, it was taken by later students as common knowledge or attributed to one or the other of those two writers. The one major exception is the work on simple modes of vibration, which passed into the German literature through the just if summary description of CHLADNI.

The famous "Historical Introduction" to LOVE's *Treatise*[1]) deserves remark because it has been read more than has any other historical evaluation written in the last half century. It rests heavily on TODHUNTER & PEARSON's *History*, criticized many times in the foregoing text, but LOVE seems to have consulted some of the sources, since he adds some errors of his own[2]). Despite these specific faults, LOVE's essay displays a hearty enthusiasm for the older writers and is a worthy effort toward a commendable purpose: We are surprised to find such an attempt at all, especially when it concludes that[3]) "the history of the mathematical theory of Elasticity shows clearly that the development of the theory has not been guided exclusively by considerations of its utility for technical Mechanics. Most of the men by whose researches it has been founded and shaped have been more interested in Natural Philosophy than in material progress, in trying to understand the world than in trying to make it more comfortable."

LOVE finds HOOKE's law the greatest landmark in our period. After mentioning GALILEO's work as raising the question of the neutral fibre, LOVE asserts that MARIOTTE located it as the central fibre and that COULOMB found its "true position", though he does not specify where that position is. COULOMB's theory of beams "is the most exact of those which proceed on the assumption that the stress in a bent beam arises wholly from the extension and contraction of its longitudinal filaments, and is deduced mathematically from this assumption and HOOKE's law." A partly false description of COULOMB's work on shear and torsion follows[4]).

JAMES BERNOULLI's paper of 1705, DANIEL BERNOULLI's variational principle, and EULER's determination of elastic curves and buckling loads are summarized. LAGRANGE

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from 1828 onward, drew its inspiration mainly from the French writers just named and from FOURIER, as did the later Italian school, while the German revival found most of its reference material in French and English works rather than in any indigenous literature.

1) Cited above, p. 403. In the first edition a historical introduction appears at the beginning of each volume. In the later editions the two are condensed into one. The description of the older work is better in the first edition, particularly in Vol. 2 (1893).

2) In particular, those in respect to COULOMB's work; *cf.* above, pp. 406—408.

3) This passage first appears in the second edition (1906).

4) *Cf.* our footnotes 1, p. 406, and 1, p. 408. *Cf.* also LOVE's estimate of YOUNG, quoted above, p. 403.

is said to have determined the strongest form of column, but we are not told what that form is.

After a few words on the BERNOULLI-EULER theory of transverse vibrations of rods, EULER's faulty theory of bells and JAMES II BERNOULLI's faulty theory of plates are described in greater detail.

This is all that LOVE reports concerning our period for study. He concludes, "At the end of 1820 the fruit of all the ingenuity expended on elastic problems might be summed up as—an inadequate theory of flexure, an erroneous theory of torsion, an unproved theory of the vibrations of bars and plates, and the definition of YOUNG's modulus. But such an estimate would give a very wrong impression of the value of the older researches. The recognition of the distinction between shear and extension was a preliminary to a general theory of strain; the recognition of forces across the elements of a section of a beam, producing a resultant, was a step towards a theory of stress; the use of differential equations for the deflection of a bent beam and the vibrations of bars and plates, was a foreshadowing of the employment of differential equations of displacement; the NEWTONian conception of the constitution of bodies, combined with HOOKE's law, offered means for the formation of such equations; and the generalization of the principle of virtual work in the *Mécanique Analytique* threw open a broad path to discovery in this as in every other branch of mathematical physics. Physical Science had emerged from its incipient stages with definite methods of hypothesis and induction and of observation and deduction, with the clear aim to discover the laws by which phenomena are connected with each other, and with a fund of analytical processes of investigation."

While nearly every specific historical statement LOVE makes is misleadingly inaccurate if not false, and while his fund of sources is miserably meagre, his general conclusions are much the same as those any intelligent reader would draw from the fuller material made available in the present work. In the next and final section, rather than repeating these generalities, I draw up an organized list of facts in summary of the successes and failures of the great researches which have spoken for themselves in the foregoing pages.

64. A modern evaluation. We give separate summaries for *analysis*, for *geometry*, and for *mechanics*, and in each of these we list not only positive achievements and clear failures but also the steps toward *concepts and methods* that became fruitful in the next century or even later.

I. Analysis. Prior to 1730, researches on continuum mechanics applied mathematical techniques already developed in other subjects, notably in geometry and in the mechanics of point masses. Starting with the researches on vibrating systems by DANIEL BERNOULLI and EULER, this situation was completely inverted. From then on until the end of the century, *continuum mechanics gave rise to all the major new problems of analysis.*

First, the theory of vibrations of an elastic band led at once to EULER's *general solution of linear differential equations with constant coefficients* (1739). Before this, "BESSEL's equation", both for real and for purely imaginary argument, had been set up and solved, and by the end of the century much of its formal theory had been completed by EULER: the analytic and logarithmic solutions, "POISSON's integral", complete asymptotic expansions, characterization of the reducible cases, calculation of roots.

The problems of the vibrating string of non-uniform density and of the buckling of a column of varying cross-section led to a study of the general linear differential equation of second order, but little regarding the nature of its solutions was learned.

The problem of *proper frequencies* and *proper functions*, opened by DANIEL BERNOULLI's work on the heavy hanging cord (1733), was solved in several typical cases. The mechanical context made it plain that the frequencies should be real and distinct, and this was asserted both for the function $J_0(x)$ and for the "LAGUERRE polynomials", but proof was lacking except in the case of the four simple transcendental equations arising in connection with the vibrations of a rod. In these cases and also for the function J_0 it was found by trial that the number of roots of the proper functions increases with the index; *i. e.*, that the proper functions are *oscillatory*. While EULER solved correctly two formidable problems for the buckling of a heavy vertical column (1776—1778) as well as calculating the proper functions and proper frequencies for square and circular membranes (1759), these cast no further light on the general theory.

Totally lacking was any hint of the *orthogonality* of the proper functions; hence *not a single expansion*, and *a fortiori*, *not a single general solution* of an initial value or boundary value problem in terms of proper functions was obtained, nor can it justly be said that such solutions were foreshadowed, unless the unsupported claims of DANIEL BERNOULLI (1753 onward) may be so interpreted.

The one exception was the *discrete* problem of the loaded string, leading to a finite set of differential-difference equations of second order. In a major special case, the problem of initial values was solved by EULER (1748); his *explicit solution* was extended to the *fully general initial value problem* by LAGRANGE (1759—1761). Both EULER's method and the second and simpler of LAGRANGE's two methods rest on the *orthogonality relations* for finite trigonometric sums and solve incidentally the problem of *trigonometric interpolation* for equidistant data[1]). These results were not appreciated until well into the next century.

The problem of determining the behavior of the *elliptic functions* of real argument was

1) EULER's solution is for the case when the function is zero at every point except one of the ends, where it is given an arbitrary value.

raised by JAMES BERNOULLI's elastica (1694) and was solved completely by EULER (1743). All essential properties of the inflectional and non-inflectional forms were rigorously determined. Through accidental discovery of a special property of the rectangular elastica (1738), EULER was led ultimately to the addition theorem for elliptic functions (1775).

The phenomenon of buckling caused EULER to study a non-linear problem of proper numbers and to prove that, for this problem, those numbers are the same as their counterparts for the linearized problem (1743). The entire relation between solutions of the corresponding linear and non-linear problems may be read off from his results; in particular, interpretation of them by LAGRANGE (1770, 1773) gave the first example of *bifurcation of equilibrium*. These ideas were to remain scarcely noticed for a century. The related question of *stability* was not raised in our period.

Variational techniques, particularly the formalism of LAGRANGE (1760), were exercised on the equations of motion of complex systems, but these latter cannot be said to have had any major influence on the theory itself.

But the great gift of continuum mechanics to analysis is *the theory of partial differential equations*. While partial derivatives had occurred here and there before 1745, a *calculus of partial derivatives* was lacking. This was supplied by EULER in a series of papers on mechanical subjects (1748—1766)[1]), in which changes of all variables, inversion of partial derivatives, and manipulation of functional determinants were explained, so that by 1788 it could fairly be said that some dozen mathematicians could use the new calculus[2]), though it remained the most abstract domain of pure mathematics.

The first partial differential equation subjected to intensive study[3]) is D'ALEMBERT's *wave equation* (1746). D'ALEMBERT obtained a solution in terms of two functions that he

1) For the most part, on hydrodynamics. See also E 44, "*De infinitis curvis ejusdem generis. Seu methodus inveniendi aequationes pro infinitis curvis ejusdem generis*," Comm. acad. sci. Petrop. 7 (1734/35), 174—189 (first pagination), 180—183 (second pagination) (1740) = Opera omnia I 22, 36—56. Presentation date: Prior to 12 July 1734. This is the first paper on partial differential equations. It concerns the system $\frac{\partial z}{\partial x} = P$, $\frac{\partial z}{\partial y} = Q$. The main theorem, given in § 6, is $\frac{\partial^2 z}{\partial x \partial y} = \frac{\partial^2 z}{\partial y \partial x}$.

2) The list is nevertheless short: EULER, D'ALEMBERT, LAGRANGE, LAPLACE, MONGE (with reservations), COUSIN, and a few younger men. EULER's attempts to gain interest for the new theory by questions proposed for the Petersburg prizes seem to have evoked some response but scant understanding.

3) EULER had derived a partial differential equation for a geometrical problem in 1734 and CLAIRAUT had derived the condition of integrability for a differential form in 1740; see p. XXI of my Introduction to L. EULERI Opera omnia II 12 and footnote 6, pp. LXXXIV—LXXXV of Vol. II 13. D'ALEMBERT derived the equation for the uniformly heavy cord in 1743, and EULER derived a complicated system for finite motion of the taut string in 1744 (above, pp. 226—228). However, these earlier researches cannot fairly be said to give a just idea of what the theory was to become.

regarded as in some degree arbitrary. The wave equation is of hyperbolic type[1]). EULER saw at once that it has non-analytic solutions (1748), the singularities of which are determined by the boundary, and, eventually, solutions representing *travelling disturbances* (1764—1765) which are *reflected* from the boundaries; this observation led him to the *method of images*. He created the concept of piecewise smooth *function of a real variable* (1748 onward) and showed great skill in joining together functions defined over finite intervals so as to obtain the *explicit solutions* of many initial value problems of considerable generality. EULER insisted that *discontinuous initial data*, resulting in *moving singularities*, are admissible (1760). He realized that the differential equation need not be satisfied at boundary points, and he proposed smoothing processes and other devices so as to incorporate polygonal solutions within the formal structure of differential and integral calculus. In one case (1760), he replaced a requirement of continuity by an integrated or smoothed condition.

None of this work of EULER's met any immediate response. In fact, it was at first rejected by all other mathematicians, on insubstantial grounds; as later experience showed the need for non-analytic solutions, the formalistic views held at about the time of his death allowed use of such solutions on equally insubstantial grounds and with little or no thought regarding their meaning. EULER's concept of real function came into general use only in the early nineteenth century, and his view of discontinuous solutions of hyperbolic equations was not adopted until RIEMANN's time.

EULER directed nearly all his researches toward solutions in *arbitrary functions*. While he derived many infinite classes of equations for which such solutions can be exhibited[1]), and while in some cases his methods have since proved useful and have been rediscovered in recent times, this approach we now consider of minor importance.

No general solution was obtained by any other method in our period. In particular, the method of *separation of variables*, introduced by D'ALEMBERT and employed frequently by EULER, remained abortive from lack of expansion theorems. The proper functions for vibration problems remained isolated, *special* solutions, which could indeed be superposed to form more general ones, but no rational method for adjusting the coefficients was known.

To justify some of the formal procedures used, nothing more than a *uniqueness theorem* was needed, but of such a theorem there is no trace.

II. Geometry. Much of the differential theory of skew curves was created by EULER

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1) A few results concerning elliptic equations were obtained in the course of hydrodynamical researches, but these give little insight toward general problems. For these and other researches on partial differential equations, see my Introductions to L. EULERI Opera omnia II **12**, **13**.

so as to study the bending of an elastica in space (1774—1775). In particular, he introduced the fundamental magnitudes of second order, the osculating plane, and the binormal. He used vectorial concepts fluently.

Research on curved surfaces in our period does not appear to have grown from mechanical problems[1]).

The remarkable results in pure mathematics we have seen to have arisen from problems of flexible or elastic bodies in the period from 1730 to 1780 are the almost single-handed creation of EULER[2]).

III. Mechanics[3]). The mechanics of flexible or elastic bodies grew up about four very old problems:

1. The vibration of a string, deriving from classical antiquity
2. The equilibrium of an elastic band, first mentioned by JORDAN DE NEMORE (13th C.), who claimed to prove that a terminally loaded band takes on the shape of a circular arc
3. The catenary problem, attacked by LEONARDO DA VINCI (before 1520) in terms of a discrete model
4. The breaking of a beam, first studied by GALILEO (c. 1606, published 1638).

GALILEO's derivation of *scaling laws*, right or wrong, for rupture and vibration, served as a persuasive example of what can be gotten from mechanical theories of materials; such results have been expected as a part of the product of every theory of media from that day to this.

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1) *Cf.* A. SPEISER, Vorwort des Herausgebers, L. EULERI Opera omnia I 28.

2) The important results by others may be listed:

1. DANIEL BERNOULLI's perception of the sequence of proper functions and proper numbers for oscillatory systems
2. D'ALEMBERT's solution of the wave equation in partially arbitrary functions
3. LAGRANGE's completion of EULER's results on finite trigonometric interpolation

3) This summary is limited to mathematical theory. A list of the main experimental results prior to 1727 was put in footnote 2, p. 139. Those between 1727 and 1788 were:

1. MUSSCHENBROEK's discovery of the law of buckling in compression (1729)
2. DANIEL BERNOULLI's rough confirmation of the existence, frequencies, and nodal positions of the simple modes as predicted by his theories of the oscillation of weighted strings, continuous heavy ropes, and rods (1733—1742)
3. COULOMB's laws of linear and non-linear torsion (1777, 1784)
4. JORDAN RICCATI's and CHLADNI's detailed confirmation of the BERNOULLI-EULER theory of free transverse oscillations of bars (1782, 1787)
5. CHLADNI's figures of nodal patterns and tables of tones of free vibration of circular and square plates (1787)

Note that there was a gap of nearly fifty years, 1729—1777, in which no result of any importance in our subject was *discovered* by experiment.

The concept of *perfectly flexible line* led to the first successful solutions of problems of finite deformation: the suspension bridge (BEECKMAN [?] (1615), HUYGENS (1646), PARDIES (1673)), the catenary (LEIBNIZ, JOHN BERNOULLI, HUYGENS (1690)), and others. JOHN BERNOULLI (1690—1691) was the first to calculate correctly the *resultant force* acting on a differential element of a curve. JAMES BERNOULLI derived two forms of the *general differential equations* for a flexible line subject to arbitrary loading (1691—1704). He gave four approaches to the problem: balance of forces in rectangular co-ordinates, balance of forces intrinsically resolved, balance of moments, and the principle of virtual work. Concerning static deflection of a flexible membrane, nothing was done.

The correct differential equation for equilibrium of a uniform *elastic band* subject to terminal load was derived and reduced to quadrature by JAMES BERNOULLI (1691). DANIEL BERNOULLI obtained the linearized solution for nearly normal load (1735). The quadratures for finite deflection were evaluated explicitly, and the possible bent forms were characterized and classified exhaustively by EULER (1743), who gave some solutions also for naturally curved rods and for an inverse problem.

EULER was led to recognize the phenomenon of *buckling* of an uniform column and to derive a formula for the *exact critical load* which applies in all cases (1743). He himself discussed the pinned-pinned and clamped-clamped cases (1743, 1778), and the clamped-free case was remarked by JOHN III BERNOULLI (1766). While LAGRANGE (1770) noticed the multiplicity of bent forms corresponding to higher critical loads, no theory or even conjecture as to which form is assumed was brought forward. Using the linearized theory, EULER calculated the first few critical loads and bent forms for a *heavy uniform vertical elastica* clamped at its base and free at its top, and he calculated also the first critical load for such a column when it is pinned at its top (1778). Earlier EULER had obtained the critical loads for various columns of *non-uniform cross-section* (1757); from his result for the conical case, it follows that the strongest conical frustum of given volume is the cylinder, and that a pointed column has no strength. LAGRANGE gave an incorrect analysis, leading to an incorrect result, regarding the strongest form of column of given height and volume (1773).

Historians of the last century put upon HOOKE's *linear elastic relation* (1675) an emphasis which reflects the predilection of physicists of the same period for linear theories. HOOKE's law was known to all researchers in the eighteenth century but, from the contrary evidence of experiments, was given little credence. As JOHN II BERNOULLI (1736) and EULER (1742, 1776) observed, on general grounds such a linear formula may be expected as an approximation valid for small elongations according to virtually any elastic theory, and this view is held today. Almost all work was directed toward problems of *large deflection*. The distinction between large deflection and large strain was not grasped.

Much of the early theory, following the precedent set by GALILEO, was compared with experiments on rupture rather than on elastic deformation; it was shown conclusively that HOOKE's law does not hold all the way up to failure. Theory, however, had provided a result which might have been, but was not, brought out in decisive confirmation of HOOKE's law for then immeasurably small strains. This is EULER's final scaling formula for the frequencies of transverse oscillation of rectangular bars (1776), which was amply tested and found good by CHLADNI (1787), for this formula rests essentially upon the fact that the flexural stiffness of bars of like material is proportional to D^3B, which, as EULER had shown, is a consequence of the linear law for the interior tensions[1]).

The first hint that very great stress may produce but tiny strain comes at the end of our period with EULER's remark that for wood a tensile stress equal to the buckling stress in compression would produce a strain of only 0,07% (1776). Special non-linear elastic relations were proposed by JAMES BERNOULLI (1695) and others, but were given no support. Fundamental studies sought to *avoid any specific elastic hypothesis.*

Only in this light can we understand the researches on the *neutral fibre*, the existence of which was implied by BEECKMAN (1620) and several later authors. JAMES BERNOULLI's attempt to provide a theory for locating the neutral fibre without assuming any particular law of variation of the tensions over the cross-section is a failure (c. 1696). PARENT gave a correct condition (1713): The area under the curve of tensions must equal the area under the curve of pressures, as was rediscovered by COULOMB (1773). Both JAMES BERNOULLI and PARENT saw that according to HOOKE's law, *the neutral fibre must be the central fibre*, but PARENT regarded the resulting formula for the breaking strength as unsubstantiated by experiment, hence the supposition (*i. e.* HOOKE's law) as not right. These profound researches have been little understood by historians, who often imply with condescension that later work in three-dimensional elasticity in some measure improves upon them, but this is not true at all, for ST. VENANT's theory, too, since it rests on HOOKE's law, places the neutral fibre at the line of centroids, precisely the conclusion that JAMES BERNOULLI and PARENT struggled to avoid! JOHN III BERNOULLI found fair agreement between theory and MUSSCHENBROEK's experiments when the tension is linear and the neutral line is the central line (1766); COULOMB, contrariwise, inferred that, just before rupture, more of the beam is in tension than in compression (1773).

JAMES BERNOULLI tried to prove that the location of the neutral fibre is immaterial (1705), but this was recognized at once as false. While the position of the neutral fibre has a marked effect on the stiffness of a given cross-sectional form, it has none whatever on

1) It has not been proved that no other law would yield this formula, but a general stress-strain relation implies that $\mathcal{M} = E B r^2 f(D/r)$, whence a more complicated law of vibration may be expected to follow.

the totality of elastic curves obtainable with varying loads. Thus EULER's persistent error in placing the neutral fibre on the concave side does not invalidate any of his results concerning elastic curves. Furthermore, these results are in no way revised by the three-dimensional theory of ST. VENANT, which leads to the differential equation of the linearized BERNOULLI-EULER theory for the curve, though allowing in the solution an extra arbitrary constant, which represents the rotation of the cross-sections. In regard to problems of *finite deflection* of beams, toward which nearly all the researches of the eighteenth century were directed, the three-dimensional linear theory of the nineteenth century is a retreat.

LEIBNIZ showed how to integrate the tensions over the cross-section of a loaded beam so as to calculate the moment (1684); after an abortive attempt by JAMES BERNOULLI (c. 1696), EULER succeeded in taking into account the curvature as well as the tension, thus *deriving JAMES BERNOULLI's law of bending from HOOKE's law* for the longitudinal elongation of the fibres (1727, 1760, 1774).

That *strain*, or change in length per unit length, rather than mere elongation should appear in the law of elastic stretching was understood by BEECKMAN (1630). In GALILEO's theory of rupture (1638) there appears, in effect, a constant or modulus having the dimensions of *stress*, or force per unit area; however, it is the breaking stress in tension rather than an elastic stress. But it is in a deep work of JAMES BERNOULLI (1705) that we encounter a real framework for one-dimensional theories of elasticity: *For a given material, stress is a function of strain.* Eschewing, as usual, any particular elastic law, JAMES BERNOULLI does not actually introduce an elastic coefficient, but if we translate his words into equations, we are led to the *tangent modulus* of non-linear elastic laws.

The importance of an *elastic modulus*, independent of the form of the particular specimen of elastic material, was not felt so long as only strictly one-dimensional problems were considered; it came into its own with EULER's derivations of the law of bending from the law of extension, mentioned above. While what is now called *YOUNG's modulus* was introduced in this earliest work of EULER (1727) and was used occasionally by him and by others in later years, it was only in 1776 that he gave a formal definition and explanation of it as *the elastic stress that would produce unit elastic strain* in any specimen of a given material.

The greatest quantity of definite results was obtained in *linearized vibration problems.* After notable progress by BEECKMAN (1614—1615), TAYLOR finally succeeded in deriving from a dynamical theory the formula for the fundamental frequency of a vibrating string (1713). Through experience with special cases[1]), DANIEL BERNOULLI (1733 onward) came to believe that a vibrating system has as many independent kinds of harmonic oscillation

1) The weighted string (1733), the heavy hanging cord (1733), the transverse motion of an elastic bar (1735).

as it has degrees of freedom. In these simple modes, all members of the system occupy their equilibrium positions simultaneously, and each vibrates at the same frequency, the *proper frequency* of that mode. For a given system, the proper frequencies are discrete, and a mode corresponding to a greater proper frequency has more *nodes* than does one corresponding to a lesser one. The overtones of sounding bodies arise from the higher modes. Any number of these modes, with any small amplitudes, may be set into vibration *simultaneously*, nor do they interfere with one another (1741). BERNOULLI asserted that *any vibratory motion may be resolved into component simple modes* (1753 onward), but he gave no means of effecting such a resolution and no proof, either mathematical or physical, that it is possible. While his idea was rejected by all contemporary theorists, it found favor with M. YOUNG, CHLADNI, and other experimenters later in the century, and in the hands of FOURIER and other mathematicians of the next it was developed into a method of great value.

EULER, LAMBERT, and JORDAN RICCATI calculated the proper frequencies and nodal ratios for the simple modes of the six kinds of transverse vibration of bars and for the vibration of a heavy hanging cord with more than sufficient accuracy (1743—1782).

All of DANIEL BERNOULLI's work and the earliest work of EULER was done without differential equations of motion. The historical evidence shows that NEWTON's principles, however comprehensive they seemed to MACH, did not put into the hands of NEWTON's successors concepts sufficient to set up *the equations of motion for compound or continuous systems*. Through what now seems laborious groping from case to case, EULER finally succeeded in 1744 in determining the general equations of motion for the loaded string and for the chain of rigid links. In 1748 he calculated *all the proper frequencies* for longitudinal vibration of an elastic cord loaded by n equal and equidistant weights, or, equivalently, for *small transverse oscillation of the loaded string*, a problem which had been solved in special cases by HUYGENS (1673) and JOHN BERNOULLI (1727). EULER solved the initial-value problem when only the end mass is displaced (1748); LAGRANGE then solved the general initial-value problem (1759). These results furnished justification of DANIEL BERNOULLI's views, but only for the particular discrete systems considered.

The *first partial differential equation of motion*, that for small displacement of the uniformly heavy cord, was published by D'ALEMBERT in 1743. By a passage to the limit, EULER found correct but complicated partial differential equations for *finite motion* of the continuous string (1744). Several authors had come close to mechanical principles sufficient to determine the small motion of a string, and in 1746 D'ALEMBERT derived the *linear wave equation*, essentially by exploiting one of the intermediate results published by TAYLOR. EULER devoted a long series of researches to this equation (1748—1765). He solved the *general initial-value problem* and proved that *all solutions are periodic* with the period T of the lowest mode; that certain special solutions, among which are the higher modes, have periods which are submultiples T/k of that period; that a necessary and sufficient condition for such a solution is that it have $k-1$ *nodes;* that the speed determined by the fundamental period is also the *speed of propagation* of pulses in both directions down the string; that such pulses are *reflected*, inverted in form, from the ends. He justified one aspect of DANIEL BERNOULLI's postulated principle of coexistence by the derived *principle of superposition*, applicable whenever the governing partial differential equations are *linear*. EULER and DANIEL BERNOULLI found solutions also for several kinds of non-uniform strings.

LAGRANGE attempted to derive EULER's solution for the uniform string by a passage to the limit from the solution for the loaded string (1759). While his analysis is faulty, it can be corrected.

As a result of the accumulated mechanical experience of the preceding seventy years, EULER in 1750 proposed his *first principles of mechanics*, which form the turning point for methods of setting up the *equations of motion*. Here, for the first time, what are now called "NEWTON's equations" are laid down as the first law governing all mechanical problems, whether discrete or continuous. In a form often used by EULER, they allow us to derive equations of motion from equations of equilibrium by adding the *reversed accelerations* to the assigned forces per unit mass; this is one of the three different laws now called "D'ALEMBERT's principle."

These "first principles" enabled EULER to write down at once the partial differential equations governing the motion [1]) of every system for which the statical equations were then known: the elastic band, the heavy cord, the flexible line, *etc.*, and in 1759 he obtained the equations of small motion of a *flexible membrane.* From 1750 onward, but not before it, each mechanical problem becomes *a problem in the theory of differential equations*, ordinary or partial according as the number of degrees of freedom is finite or infinite, of second order in the time.

DANIEL BERNOULLI conjectured the form of the elastic potential or *stored energy* of a band and predicted that it should be an extreme in equilibrium (1738); EULER verified this (1738, 1743). In his studies connected with the principle of least action, EULER extended BERNOULLI's principle to the case when distributed forces act on the band (1748); to this end, he identified the stored energy as the work done by the bending moment. JAMES RICCATI (c. 1754) suggested that at least a part of the work done in deforming any elastic body is stored, available for reversing the deformation, but he gave no theory. LAGRANGE in his researches on the principle of least action formulated a variational principle for the motion of loaded or continuous perfectly flexible lines (1761), but only in 1788 did

1) Every student of modern mechanics is aware of the pitfall here, but it does not come up in our history.

he succeed in including flexural elasticity in his formalism. While LAGRANGE's approach did not lead to anything in our period of study, it proved of great use in the nineteenth century for work on conservative systems. GREEN was to found his theory of finite elastic strain upon LAGRANGE's methods, applied to a general stored energy, and the credence given to variational principles in modern physics is extreme. CAUCHY, however, entirely ignoring LAGRANGE's approach, built his theories of materials upon direct laws of force, as EULER had done before him. While modern physics has followed the tradition of LAGRANGE, modern continuum mechanics works almost exclusively in the tradition of EULER and CAUCHY.

Historians often refer to the faulty theory of vibration of plates proposed by JAMES II BERNOULLI (1787), sometimes also to EULER's faulty theories of vibration of curved rods (1760, 1774). These theories deserve no notice, being simply wrong. Correct mechanical principles were at hand and were applied correctly. The failures here reflect no lack of "physical intuition" but only *insufficient geometry:* in the one case, a differential description of the small deflection of an inextensible curve, in the other, a proper theory of the curvature of a surface.

But of the developments in our period, the most important are those pointing toward *the general theories of stress and strain*. The steps are short and far apart. GALILEO's hint at the tension in a suspended rope carrying a weight (1638) was generalized by PARDIES' assertion that when a flexible string is hung up by two points, the force exerted by one part upon its neighbor acts along the tangent (1673). This is the *tension* of a perfectly flexible line, explicitly recognized and clearly defined at last by JAMES BERNOULLI (c. 1696) and HERMANN (c. 1712). Still far ahead lies the *interior stress*. A single remark of PARENT in 1713 shows that he realized that such stress need not be normal to the surface on which it acts; *i. e.*, *shear stress* is possible, but this remark lay unnoticed.

The concept of *shear force* was evolved by EULER in 1771 after nearly fifty years of struggle to gain a theory based on the balance of forces yet general enough to include both the elastica and the catenary. In 1728 he had achieved a unification in terms of the balance of moments, but only by two pieces of luck: In the perfectly flexible case, balance of moments implies balance of forces, and in the case of the elastica, the additional information supplied by the balance of forces is not necessary to determine the form of the curve. Finally he saw that the action of one part of a plane deformable line upon its neighbor is equipollent to a force and a couple. The force, which is the modern *stress resultant*, is a vector *not necessarily tangent* to the line. With this concept, EULER achieved the *general statical equations* for a plane deformable line (1771).

In the next year COULOMB revived PARENT's concept of shear stress and actually wrote the three conditions of equilibrium for the stresses acting upon a cross-section, but

COULOMB's work, like PARENT's, came nowhere near a theory of elasticity, since deformation was neglected. COULOMB showed that the *maximum shear stress* in simple tension is that which acts upon a plane making an angle of 45° with the direction of tension.

Still more important for the future was EULER's concept of *fluid pressure* in hydrodynamics. There the stress vector appears within three-dimensional bodies of arbitrary form, but is restricted to be normal to the surface on which it acts[1]).

Thus all the elements of the theory of stress in three dimensions had been created:

1. The dimensions of stress are [force]/[area];
2. Stress is defined upon an imagined boundary dividing the material into two parts;
3. Stress is a vector or vector field equipollent to the action of one part of the material upon another;
4. The direction of the stress vector, as far as purely mechanical principles are concerned, is not restricted;

but they appeared in different contexts, in each of which at least one of the four is violated by additional restrictions.

Not only stress but also a *general theory of strain* is needed for theories of materials. While *extensional strain,* a simple concept, the growth of which we have already followed, is an element in such a theory, so also is *shear strain*, not a trace of which occurs in the researches we have studied. Neither physical intuition nor experiment was what was needed here; rather, as both EULER and CHLADNI said, it was *want of differential geometry* that blocked the way to theories of deformable surfaces and solids. While in logical order the geometry of motion is preliminary to the mechanical laws which select one motion out of all the possible ones, in the history of mechanics correct dynamical principles have nearly always preceded the *kinematical analysis* necessary to exploit them.

Here we meet a surprise, for the six components of the three-dimensional infinitesimal *strain tensor*, including the *shear strains,* were introduced and carefully interpreted by EULER in 1766—not in connection with elasticity, where they were needed, but in a research on the kinematics of fluids, where they served no immediate end[2]).

Thus the material was all at hand. This in no way lessens the originality of CAUCHY, who within forty years of EULER's death was to create the whole general structure of continuum mechanics. Rather, to reforge the tradition of his forebears is the greatest originality a man can have. This EULER proved in respect to JAMES BERNOULLI, and this CAUCHY is to prove in respect to EULER.

1) Development of this concept occupies a major part of my Introduction to L. EULERI Opera omnia II 12.

2) *Cf.* p. XIII of the Introduction to L. EULERI Opera omnia II 13.

For the results in mechanics, I have thought it superfluous to estimate the relative achievements of the various geometers. But such an estimate may be inferred from one of a somewhat different kind. If we except a few pages each from HUYGENS' notes and from the first memoirs of LAGRANGE and COULOMB, everything of permanent value known in 1788 may be learned from EULER's papers, which incorporate, simplify, and deepen the results of all earlier researches.

In surveying all these brilliant individual achievements in the theories of flexible or elastic bodies, we are driven to ask why, when EULER had succeeded in 1752 in creating a general theory of perfect fluids in three dimensions, nevertheless after many more years he failed to reach a general theory of elasticity. Making fair allowance for the greater complication of elasticity in some regards, I cannot believe it to have been the main reason. Recall that, with some exceptions, special theories of fluids led only to error or frustration. To succeed in hydrodynamics, the only hope lay in abandoning a one-dimensional approach. But for elastic or flexible bodies, one-dimensional theories led to one triumph after another. It was the *brilliant successes* of the special theories that blocked the way to the general theory, for nothing is harder to surmount than a corpus of true but too special knowledge. What CAUCHY was to achieve was *sufficient distance* from all this material, both in theory and in experience, to cast aside the accidents and draw out the essentials.

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INDEX OF NAMES