SCIENCE AND VALUES

Patterns of Tradition and Change

edited by

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Introduction

A variety of entrenched traditions in recent Western thought have favored the assumption that science is a thing apart, as unique in its special methods as in its remorselessly cumulating results. As orthodox religion decayed, natural science was an obvious surrogate. From Comte to Carnap, major Western philosophers have sought to define and defend those characteristics which made science the peculiarly correct repository for Western ambitions. Though more vigorous than most in his formulations, George Sarton was merely applying common intellectual assumptions when he argued that "the history of science is the history of mankind's unity, of its sublime purpose, of its gradual redemption". Today we enjoy neither such faith, nor the assumptions on which it was based. It thus becomes a considerable challenge to move on from those tacit beliefs which surrounded the history of science in its earliest years as a professional enterprise. To disenchant our understanding of science, and to see it as a natural cultural phenomenon, may well prove as difficult in practice as it is desirable in theory.

The seventeenth century has long been at the focus of historical enquiry into science. Initially this was because "the scientific method" seemed to be the enduring product to which "the scientific revolution" gave rise. More recent studies have shown the notion that there was one revolution to be as unsatisfactory as the idea that any unambiguously identifiable method or methods emerged from the intellectual turmoil of the period. Yet the myth lives on that there is one homogeneous product, "science", of which the roots may be studied and the stock grafted onto other less fortunate nations. Social scientists, historians of ideas and students of modernization have all continued to accept the belief, long after its intellectual roots have withered. This is in part because the notion of science as a culture-free and somehow timeless enterprise continues to serve a complex of social needs within modern Western Civilization. But it is also because we still lack studies which illustrate the varieties of natural knowledge as a cultural activity.

It is our hope that these present essays may serve as one modest beginning toward the task of understanding natural knowledge as a cultural enterprise.

The task inevitably requires cooperative and comparative study. Attention to the way in which Western science was received by non-Western cultures offers one of the most fruitful opportunities for analyzing the cultural dimensions of that science. Other possibilities of particular promise include closer attention to the way in which the values, meanings and functions of Western science have themselves shifted in the last two centuries as urbanization, industrialization and professionalization have transformed the cultural meanings of natural knowledge. A closer attention to the political functions served by the rhetoric of current scientific enterprise may also throw light on the evolving character of that enterprise.

The essays that follow seek to pursue such avenues. They do so in ways which are necessarily as limited as they are varied. Any attempt to impose a common methodology would be as premature as it is alien to our intention. The need at present is for a range of methods and approaches, as we seek to understand the several roles that natural knowledge has played in different cultures and periods. The contributions in this volume thus range from general studies of the reception of modern Western science in China and Japan, to detailed examination of subjects as varied as the values of American chemists and the attitudes of British administrators. What all the contributors have in common is a belief that comparative historical study of natural knowledge in social context is a prerequisite to any full appreciation of the possibilities and limitations of scientific understanding.

It is a pleasure to record that the contributors also share the memorable experience of a week-long seminar at the Van Leer Jerusalem Foundation in August 1970, at which the first drafts of these present essays were subjected to common critical analysis. Through formal and informal discussion ideas were clarified, problems identified, and strategies agreed. The delights of intellectual exchange within a new and challenging field would have been enough. The addition of the cultural associations of Jerusalem, the unruffled efficiency of the staff of the Van Leer Foundation, and the contagious intellectual enthusiasm of its director, Dr. Yehuda Elkana, explain both why the seminar was truly memorable, and why the Foundation is rapidly becoming a major focus for the sort of comparative studies represented in these present essays.

30th July 1972 The Editors

The Industrial Revolution and the Image of Science

THE INDUSTRIAL REVOLUTION AND THE IMAGE OF SCIENCE

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If you turn to Samuel Johnson's great dictionary of 1755, and look up the word *SCIENCE,* you will find it defined as "1. Knowledge. 2. Certainty grounded on demonstration. 3. Art attained by precepts, or built on principles. 4. Any art or species of knowledge. 5. One of the seven liberal arts." The relationship (if any) between science and the experimental study of nature is left unstated. The image is rather that of certain knowledge, obtained by solitary and reflective activity. In harmony with this image Johnson defines a philosopher as "A man deep in knowledge, either moral or natural." And the particular term naturalist describes "a person well versed in natural philosophy."

As these definitions make transparently clear, not only was the very name and function of the scientist not yet invented, but science in the sense we know and use the term was unfamiliar to the English-speaking world of the mid-eighteenth century. Natural knowledge certainly existed, and one could discuss the rather doubtful legitimacy of its claim to be considered as "certainty grounded on demonstration" and thus to be admitted to the lofty realm of science. That natural knowledge constituted and defined science not even its most zealous advocates would claim. In like manner the period's philosophers or men "deep in knowledge" certainly included many (among them Johnson himself) "well versed in natural philosophy". But the professional norms, occupational structures, values, goals and rewards associated with the scientist were as unknown as the word.

If we move on some three generations, the mood has changed. In 1834 we find William Whewell decrying "the want of any name by which we can designate the students of the knowledge of the material world collectively." It is of some significance that Whewell went on to point out how "this difficulty was felt very oppressively by the members of the British Association for the Advancement of Science, at their meetings at York, Oxford, and Cambridge, in the last three summers. There was no general term by which these gentlemen could describe themselves with reference to their pursuits. Philosopher was felt to be too wide and too lofty a term, and was very properly forbidden them by Mr. Coleridge...*savans* was rather assuming, besides being French instead of English; some ingenious gentleman [Whewell himself] proposed that, by analogy with artist, they might form scientist..."[1]

So much for scientist. What of science? Move on another four decades and listen to Norman Lockyer's chillingly prophetic statement that "Science [is] simply the employment of means adequate to the attainment of a desired end, whether that end be the constitution of a government, the organization of an army or navy, the spread of learning, or the repression of crime... The same method is necessary to raise, organize and equip a battalion, as to perform a chemical experiment." Thomas Henry Huxley makes the same point in mellower language when he asserts that science is "nothing but *trained and organized common sense.*"[2] Not certain knowledge, but not just common sense.. Rather trained and organized common sense, the common sense of the scientist. The picture suggests professionalism, limited goals, utilitarian emphases and specific technical concerns. The structure and function of natural knowledge have been transformed, and with them the image of science itself.

It is the reasons for and the implications of this transformation that I wish to discuss. What were its causes, its content and its significance for our own understanding of the present nature and possible futures of science?

* * *

Let us leave this immediate question for a minute, and turn to a broader discussion of natural knowledge, viewed historically. Within the Western tradition (and it is only within the Western tradition that, for good or ill, modern science has developed) we can perhaps discern three fundamental transformations. The first is primarily mental and psychological—the intellectual revolution of the seventeenth century in which a new confidence was acquired as to the cultural worth of mounting a sustained cognitive enquiry into the workings of nature. The second is primarily social—the professionalizing reorganization centered in the century after 1760, which gave the enquiry into nature not only new goals but new structures, as exemplified in the need to invent the word scientist. The third, which began around 1914, is the one we live at the center of—the technocratic reappraisal. This latest change in the image and purpose of natural knowledge incorporates several of the cognitive and professional norms of the two prior transformations. What makes the present reappraisal peculiarly technocratic is the full conscious realization of the fact that, in the hands of the expert, natural knowledge is now a fundamental key to military power, national survival, the increase of wealth, and perhaps even to life, liberty and the pursuit of happiness.

The first of these three great transformations of natural knowledge, the intellectual revolution of the seventeenth century, has been the subject of considerable study. Its outline is now well understood.[3] Even so there is still much lively debate and lingering discussion over particular details. The second transformation, the professionalizing reorganization is as yet but dimly apprehended,[4] while the third, the technocratic re-evaluation, is almost completely neglected by competent historians of science. This technocratic re-evaluation quite obviously requires attention. However there is something to recommend first devoting our efforts to an understanding of the prior and still neglected professionalizing reorganization of, say, 1760 to 1870. And it is this second of the three fundamental transformations of natural knowledge and its seldom explored relationship to the industrial revolution that is my concern here.

* * *

The continuing existence in Western culture of a relationship between the desire for natural knowledge, and a desire to exploit nature for technical ends, is obvious. Which is not to say it is well understood, or even much studied. Indeed the particular and crucial relationship between the industrial revolution and the professionalizing reorganization of science has never been carefully examined. Yet it is not lacking in easy and confident generalizations. To the question of what, if anything, was the relationship between science and the industrial revolution, at least three different answers have been given: the later-Victorian, the Marxist and the idealist.[5] Curiously, all three answers reduce to one common belief.

The later Victorians were extremely proud of their own considerable technical achievements. These achievements were, they felt, superior to those of any previous age, and superior at least in part because of their scientific basis. The close relationship between science and technology was itself but one aspect of their own ascendancy over previous ages. They confidently asserted that before the middle of the nineteenth century science and technology were essentially unrelated enterprises. As Samuel Smiles so vigorously phrased it: "One of the most remarkable things about engineering in England is, that its principal achievements have been accomplished, not by natural philosophers nor by mathematicians, but by men of humble station, for the most part self-educated. The educated classes of the last [i.e. the eighteenth] century regarded with contempt mechanical men and mechanical subjects...engineering was thought unscientific and ungenteel."[6] What Smiles asserted, Arnold Toynbee implied by discreet omission. His classic *Lectures on the Industrial Revolution in England* (Oxford, 1884) found the very mention of science unnecessary, such was its supposed irrelevance to his theme. And just as Smiles' assertions set the tone for much subsequent writing in the history of technology, so Toynbee provided a model which was to mold the work of successive economic historians.

If the later-Victorians, and those who still follow the patterns of argument they developed, were sure of the irrelevance of science, it was far otherwise with the followers of Karl Marx. To the Marxist, important changes in science are plainly relatable to the industrial revolution but as consequence rather than cause or concomitant part of that revolution. For instance J.D. Bernal writes of the industrial revolution that "science still remained largely what it had become in classical times, a somewhat esoteric part of the framework of belief erected in the interests of the ruling classes: it was part of the ideological superstructure. Effectively, it had contributed nothing to industry." While science was not a contributing cause, it was soon to be transformed as a direct *consequence* of the industrial revolution and thereby to become "one of the major elements in the productive forces of mankind." To the Marxist, science and the industrial revolution are thus inseparably bound together, but in chronological sequence rather than contemporary interaction. The practical result is that Marxist and later-Victorian agree: the science of the period may safely be neglected when the industrial revolution itself is under discussion, and vice versa.

Paradoxically the idealists also endorse this conclusion, while disavowing both the later-Victorian and the Marxian forms of the argument. The leader of the idealist school, Alexander Koyré, was himself quite explicit that "our 'idealism' is nothing else than a reaction against the attempts to interpret, or misinterpret, modern science....as a promotion of arts and crafts, as an extension of technology, as an *ancilla praxi.*" In his mirror-image stance, Koyré therefore reversed the Marxist insistence on the primacy of the material over the spiritual. To him "science, the science of our epoch like that of the Greeks, is essentially *theoria,* a search for the truth....an inherent and autonomous development."[7] And of course the inevitable consequence is that to historians of Koyré's persuasion, the industrial revolution is once again a subject safely neglected.

I want to remark later on the images of science, and the assumptions about "pure" and "applied" science, contained in these three historiographic positions–the late Victorian, the Marxist and the idealist. But for now let us concentrate on the historiographic issue. To reject the remarkable consensus that exists between economic historians, historians of technology and historians of science, between Marxists, idealists and empirics–to reject this consensus does perhaps seem foolhardy. Nonetheless I want to argue that science and the industrial revolution can and should be studied together, to their mutual profit. To see them as totally separate is wrong–as wrong as would be the assertion of any simple causal relationship. The manifold connections between industrial growth and scientific change are not easy to elucidate. Even the present socially stratified system of science is ill-understood, let alone the ways in which it has changed with time. However we shall never achieve any adequate understanding of the professionalizing reorganization of science, until we make the deliberate effort to see that reorganization in relationship to the cultural, social, economic, political and technological elements with which it was so closely interwoven.

* * *

If we are to achieve any adequate discussion of science and the industrial revolution, some arbitrary limitations and preliminary clarification are needed. Thus—as should by now be apparent—my bias is hopelessly British. The industrial revolution demands discussion at least in European context.[8] How much more so the scientific enterprise! But my comments will be restricted to Britain. This is partly because some such limitation is required if one is to make any impact on so complex a problem, partly because the industrial revolution was itself initially restricted to Britain.

Even granted these restrictions, we still confront a situation of daunting complexity. The professionalizing reorganization that led from men "well versed in natural philosophy" to scientists displaying "trained and organized common sense", has many facets. The transformation of the actual institutional structure of the scientific enterprise is one. The changing social class of the scientific practitioner, and the changing patterns of financial support, are others. Allied shifts in social status and social function may be discerned, along with changes in the epistemological meaning attributed to the very theories constituting scientific knowledge. These would seem to be the main factors in that professionalizing reorganization of science which was both a function of and influence on the contemporaneous industrial revolution. Let us now examine some of these factors, in hope of gaining an insight into the changing nature and image of natural knowledge.

* * *

In 1844 that perceptive observer of the contemporary scene, Benjamin Disraeli, remarked that "what art was to the ancient world, science is to the modern: the distinctive faculty. In the minds of men the useful has succeeded to the beautiful....rightly understood, Manchester is as great a human exploit as Athens."[9] In thus linking science, utility and Manchester, Disraeli was highlighting the changes in the structure and function of natural knowledge which had taken place during the industrial revolution.

Consider first the institutional structure of natural knowledge in 1760. The task is simple—the whole British Isles contained only twelve institutions which believed natural knowledge to be even marginally within their concerns. The oldest, the Royal College of Physicians of London (founded in

1518), contented itself with the occasional endowed lecture, the erratic maintenance of its library and the vigorous prosecution of unlicensed physicians. Committee meetings, dinner parties and other socializing activities for its eminently clubbable gentlemen were clearly its major concern. The eighteenth was after all the century of the club, the coffeehouse, the clique, the claque and the coterie, and London was their British center.[10] The status and function of the college is thus wholly understandable–for a physician was above all a gentleman, able to wait on his wealthy patients without social embarrassment, if without trained expertise.

Where the College led, the Royal Society of London for the Improving of Natural Knowledge (to give the full title) amiably followed. To its credit, the Society never entirely lost sight of its ostensible purpose of improving natural knowledge. Neither did it neglect its more immediate and pressing function as a meeting place for gentlemen. Characteristic of its mid-eighteenth-century concerns is the way that in 1750 its associated dining club, the Society of the Royal Philosophers, busied itself in laying down such formalized rules of procedure as that "any Nobleman or Gentleman complimenting the Society annually with venison, not less than a haunch, shall, during the continuance of such annuity, be deemed an honorary member." The innovation was a resounding success. Indeed it was soon decided that "the giver of a turtle should also enjoy the rights and immunities of a venison donor."[11] Such amiable camaraderie and epicurean regulations should not be lightly dismissed. The club format was peculiarly well adapted to the needs and social functions of natural knowledge at this period. Thus one of the more significant new institutions, the Society of Civil Engineers, began in 1771 as a gentlemen's dining club, consciously modelled on that of the Royal Philosophers.[12]

The Society of Arts, launched in a London coffeehouse in 1754, displayed a more immediate and pressing interest in its stated goals. But premiums and propaganda were its favored means, technical ingenuity its end. Natural knowledge was peripheral, in a way that the inevitable dinners, nobility and clubbable gentlemen were not. In the provinces the Spalding Gentlemen's Society and the Peterborough Society flickered with spasmodic light.[13] Only in Edinburgh did the conjunction of capital city, teaching university and active medical faculty lead to hints of new ambitions and new forms. There a Medical Society and a Philosophical Society jostled with the College of Physicians and the College of Surgeons in the competition for place and status. An active pursuit of natural knowledge was one of the more fortunate side effects. Glasgow enjoyed only a combined Faculty of Physicians and Surgeons, while in Dublin the College of Physicians and the Dublin Royal

Society sponsored some modest activity.[14]

To the best of my knowledge these twelve groups constituted the total of organized societies with even the remotest interest in science. Among them only the Royal Society had natural knowledge as its ostensible focus. Otherwise medicine was the vital sustaining force, surpassed only by the impulse to be clubbable itself. The provinces were scarcely represented. Arts and manufacture provided a very minor theme. Even agriculture was unimportant in organizational terms. Natural knowledge was at best part of the usual equipment of the cultured and leisured gentleman, at worst but one of a host of excuses for convivial activity. Interest in science by those of lower social status—for instance the itinerant lecturers and instrument makers—was already considerable, but completely lacking institutional focus.[15]

Now consider the picture in 1870. Our 12 societies have grown to 125. Of these no less than 52 are in the English provinces. 19 in some way concern technology, 6 are directly agricultural. Medicine no longer dominates, but does offer 16 societies. 15 societies may be classed as "general scientific", but 59 are restricted to particular sciences or areas of science.[16] In 1760 there was but one general scientific society (the Royal Society) and none of a specialist nature. If the foundation and survival of societies is any guide (and what better criterion is there of the life and vitality of the scientific enterprise?) the century following 1760 was one of enormous growth and vigor. It was also the century in which British science entered seriously into the life of the provinces, shook off the dominance of medicine, began a career of specialization, came to know a new relationship with manufacturing and agriculture, and became both fully institutionalized and fully professionalized.

* * *

With such dramatic changes in the organizational structure of natural knowledge, it is not surprising that we may trace significant shifts in the social class of its practitioners. Venison, turtle, polite medicine and Anglican graces were increasingly replaced by Quaker thee's and thou's, the discussion of steam pressures and the systematic purchase of books and apparatus. Firm information on the social background of the membership of any of the 125 societies in existence in 1870 is easy to obtain in theory, but non-existent in practice. Among the very few attempts made, the rather differently conceived enquiry of Nicholas Hans yields interesting figures.

Investigating a somewhat arbitrarily selected group of 680 natural philosophers of the seventeenth and eighteenth centuries, he found that of those born before 1665, 52% were of upper class background. For those born in 1706-25 (that is, of middle age in 1760), the figure falls to 24%. For those born in 1766-85 (that is, entering science in the period of the Napoleonic wars) it is close to 18%. Over this whole time span, the percentage of natural philosophers recruited from the lower classes doubles from 12 to 25%, while the middle class contribution takes firm command, moving from 36 to 57%. If we extend the analysis into the nineteenth century, we find that of those born in 1826-45, no less than 85% are of middle class origins. The upper class contribution has dropped to 4%, while the lower class contribution has fallen back to 11%.[17] These statistics are not definitive but suggestive and indicative in the way they illustrate the growing attractiveness of natural knowledge to the provinces, to the middling classes of society, and (for a limited period) to the ambitious and talented member of the urban masses, determined to rise he knows not how. John Dalton, Humphry Davy and Michael Faraday provide immediate examples of each type.

Each of the last three I have named made a full-time career in science, at a time when such a career knew no public definition. Thus consider John Dalton in 1790, an ambitious and frustrated schoolmaster of 24, writing to friends for advice: "Though I doubt not but my inclination would yet adapt itself to any business that promised to be of advantage, yet it seems natural to turn to such wherein literary or scientific knowledge is requisite." What were such businesses? Not those of the scientist or the natural philosopher, let alone the physicist or the chemist. Instead his concern was the two professions open to a Quaker–those of the lawyer and the physician. Unhappily for Dalton "the great objections are the expense at first, and the uncertainty of getting business afterward." For just those reasons, Dalton's friends discouraged his plan to study medicine at Edinburgh (his uncle brusquely declared that the role of physician or lawyer was quite beyond him but that "thou mightest, perhaps, be able, with a little capital and great industry, to establish thyself....in the humbler sphere of apothecary or attorney.")[18] Dissuaded from his larger hopes, Dalton instead found fame, security and a modest wealth in science. He did all these things in Manchester–a new town–and more especially in its Literary and Philosophical Society, one of the new institutions reflecting and creating new values, uses and ideologies for natural knowledge. Dalton's career thus invites further investigation and remark.

The Manchester Literary and Philosophical Society, England's oldest continuing scientific society apart from the Royal Society of London, was founded in 1781.[19] The first, it was also the foremost of the rash of such societies founded in the growing manufacturing centers of England as the industrial revolution progressed. Boldly provincial, progressive and technological in its rhetoric, it nourished creative science of the highest caliber, of which John Dalton's work is the best-known but by no means the solitary example. While Dalton was ultimately to bring great prestige to the Society, the Society in its turn played an earlier and critical role in his intellectual development.

The "Lit and Phil" offered legitimation, audience, encouragement and reward to the fledgling scientist, at a time when science still enjoyed little public recognition as a *profession.* Not only did the Society provide an extensive and up-to-date library, a vehicle of publication (the *Manchester Memoirs,* which were eventually to contain 26 of the 117 papers Dalton read before the Lit and Phil) and, from 1800, a home for Dalton's apparatus and experimental labors. It also offered critical encouragement and personal reward. This last may be seen objectified and institutionalized in John's path through member to secretary to vice-president (1808), and finally to president (1817) – in which last capacity he ruled the Society firmly but efficiently for the remaining 27 years of his life.

If the Manchester group provided the essential environment for the flowering of Dalton's abilities, other scientific societies were more peripheral to his life. Dalton showed considerable reluctance to be a candidate for election to the Royal Society. In 1810 he rebuffed Davy's approaches, and he was finally elected in 1822 only when some friends proposed him without his knowledge. He submitted but four papers to its *Transactions.* Though awarded one of the first two Royal Medals in 1826, in recognition of his chemical atomic theory, he appears to have been almost completely indifferent to the Society's affairs. This indifference contrasts sharply with his attitude to other groups whose socializing functions were more clearly subordinated to the recognition of professional merit and the promotion and dissemination of science. As early as 1822 Dalton found time to visit Paris, and formally take his seat as corresponding member of the French Académie des Sciences. In 1831 he was active as a founding member of the British Association for the Advancement of Science. Yet it was only in 1834, when he himself was at last enjoying widespread social recognition as the archetype of the new breed of scientists, that Dalton finally condescended to sign the register and formally take his place as an F.R.S.[20]

Dalton's earlier indifference reflects the gulf in social class and professional stance between the provincial teacher committed to his science as a means of self-definition, and the still largely amateur, cosmopolitan and dilettante orientation of the Royal Society. In this respect one might usefully contrast Dalton's struggles to establish himself with Sir Roderick Murchison's entry onto a distinguished scientific career: "In the summer following the hunting season of 1822-3, when revisiting my old friend Morritt of Rokeby, I fell in with Sir Humphry Davy, and experienced much gratification in his lively illustrations of great physical truths. As we shot partridges together in the morning, I perceived that a man might pursue philosophy without abandoning field-sports; and Davy....encouraged me to come to London and *set to* at science....[and] said he would soon get me into the Royal Society...." Davy was as good as his word, rapidly gaining Murchison an F.R.S. not because of "the amount or value of his scientific work" but simply because "he was an independent gentleman having a taste for science, with plenty of time and enough money to gratify it."[21] It is understandable how Murchison and Dalton might not feel at ease in the same society, though in fact both became fully-committed professional scientists.

The changing social status of science, and the tensions and opportunities endemic in the professionalizing scientific enterprise, are also illuminated by the career of Humphry Davy. If science was itself slowly declining in status, it clearly offered an ever more accessible escalator to entrepreneurial spirits who saw their opportunity in its changing nature. Davy's giddy rise from Cornish obscurity through mind-expanding gases and a brilliant shower of electric sparks to fame, fortune, a baronetcy and the Presidency of the Royal Society, is the most vivid illustration of this truth.[22] Natural knowledge, whether as fascinating lectures for fashionable ladies or earnest exhortations on prosperity and manufactures for portly businessmen, was clearly finding a new relevance and a new role. But as it did so, the upper classes increasingly abandoned the cultivation of science to the provincial and Dissenting, to the earnest and professional, to the unspeakable middle class.

Consider the composition of the Royal Society itself. Between 1800 and 1830 the percentage of members who might conceivably be classed as "scientific" rose from 28.6 to 32.3. A cause of congratulation to the reform minded, the narrow legalists and the historian of science, even if not sufficient for the visionary Charles Babbage. Of more concern and significance in contemporary eyes was the ominous fall in the percentage of peers from 11.2 to 9.5. By 1860 reform had done its work, 52.6% of the fellows being scientific, but only 4.6 being peers.[23] Men of standing were no longer able or willing to be associated with something as narrow and professional as "trained and organized common sense." The Royal Society of London might from henceforth promote natural knowledge with a vigor unknown since its earliest days: but only at the price of altering its own social class and social function in a painful and belated adjustment to the new realities of science.

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As the scientific enterprise was taken over by the middle classes, so the main themes in its public justification moved from polite curiosity and natural theology to a concern with immediate technological utility. In a certain sense, natural knowledge has always been justified in terms of the utility it offers both its adherents and the wider society. But the earlier "utility" of awe, wonder, contemplation and a buttressing of accepted religious values was a far cry from the direct technical utility favored by the advocates of "trained and organized common sense."

As early as 1791 the propagandizing poet of this new utilitarian rhetoric would envisage how

> "Soon shall thy arm, unconquer'd steam! afar
> Drag the slow barge, or drive the rapid car;
> Or on wide-waving wings expanded bear
> The flying-chariot through the fields of air
> –Fair crews triumphant leaning from above,
> Shall wave their fluttering kerchiefs as they move
> Or warrior-bands alarm the gaping crowd,
> And armies shrink beneath the shadowy cloud."[24]

In more sober and practical fashion the Manchester Literary and Philosophical Society would early decide that "a gold medal, of the value of seven guineas, be given to the author of the best experimental paper on any subject relative to arts and manufactures." And its Secretary, Thomas Henry, would instruct the members how "several branches of natural philosophy seem peculiarly adapted to fill up the vacant hours in which the tradesman can withdraw from his employments....[and] supply him with a kind of information which may turn to good account, by furnishing him with the means of extending his commercial concerns, and conducting them to greater advantage; of improving...manufactures...or inventing new fabrics, which may give additional life and spirit to trade."[25] In such ways did the Manchester Society in the 1780's announce those utilitarian themes which came to legitimate professional science. Not that natural theology was completely abandoned. The middle-classes also found its appeal peculiarly congenial–hence the furore over Darwin. But utility became a dominant means of justification and a stock argument for financial support as natural philosophy contracted into organized common sense.

Many of the organizers and promoters of new scientific institutions and schemes earnestly and naively believed in their direct technological utility: witness Count Rumford's visionary plans for the Royal Institution. In the early triumphs of the industrial revolution–especially steam power and chlorine bleaching–some saw convincing proof of the rich dividends available to investors in natural knowledge.[26] Others were more cautious in their actions, if equally enthusiastic in their rhetorical exclamations. The clamorous assertion of the utility of abstract research was especially marked among those men of lowlier origin whose futures were wholly dependent on the successful institutionalization and public support of the rapidly-expanding scientific enterprise. Dalton with his laboratory at the "Lit and Phil," Davy at the Royal Institution, Andrew Ure with his manifold publishing ventures, Friedrich Accum as entrepreneur of scientific supplies – such obvious examples could be multiplied indefinitely. Whether or not "the rich are different from us" solely in their possession of money was at best an academic question to these precariously employed practitioners of science. And the social status, social functions, ideologies and values associated with the pursuit of natural knowledge changed only in so far as an industrializing nation both provided employment opportunities for and demanded the services of these fledgling scientists.

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Noble Lords, their physicians, minor clergy and country gentry – in the mid-eighteenth-century all could indulge their desire for clubbable life and their whim for natural knowledge without benefit of salaries, standards, demands for productivity or any such vulgar paraphernalia. The collections of a Banks and the munificence of a Sloane could well support whole fractions of the national scientific enterprise. *Ad hoc* government grants – given by men of secure social position to men equally secure – took care of occasional major expenses. Examples may be found in the running of the Royal Observatory, the commissioning of voyages of exploration, the determination of longitude or the observation of the transits of Venus. Gresham College and Oxbridge chairs were available for the odd gentleman who was more, or perhaps less, committed to natural knowledge. The style of support was monopolistic, paternalistic, fragmentary: a style which acted to reinforce the polite, learned and cultured tone of natural knowledge. In the secrecy of his study Isaac Newton might draw up a plan for a Royal Society endowed with ample government funding and exact organization charts, but even he knew it was but an idle dream.[27]

One of the most interesting aspects of British science is how long this amateur and gentlemanly style persisted, while new personnel, institutions and activities grew and flourished, always supplementing but before the mid-nineteenth century rarely supplanting the older forms. Holding Oxbridge chairs, serving on Government advisory boards and commissions, belonging to the Royal Society: all supposed a degree of leisured grace quite alien to the newer practitioners of the industrial revolution period. Symptomatic of the cleavage is the first meeting of the British Association for the Advancement of Science. But seven representatives from London, two from Oxford and none from Cambridge joined the three-hundred-odd professionally-oriented "provincials" in their brash, ungentlemanly scheme to organize a pressure-group for the better organization and endowment of science. When the viability of the Association became apparent, the older elite quickly moved in and took over, quietly eliminating such ideas as that of organized state support, with its prospect of unwelcome legislative interference.[28]

Despite this opposition to the too rapid and explicit professionalization of science, the range, type and extent of public support steadily increased throughout the nineteenth century. One need only instance the Mechanics Institutes, the University of London and the Redbrick colleges, the funds collected and disbursed by the British Association, Government grants administered via the Royal Society, the support of research in private and hospital medical schools, Kew Gardens and the Natural History Department of the British Museum, the voyages of exploration, the meteorological activity funded through the Royal Artillery, the Ordnance and Geological surveys and the ever-expanding empire of the Astronomer Royal.[29] Such growth inevitably fostered increasing specialization, and the emergence of narrower professional norms.

After the first early euphoria, the interest of the industrialists declined. The return on abstract research was simply too low to excite the continuing large-scale investment demanded of any manufacturer, at a time when British products enjoyed an effortless command of world markets. The slow and reluctant accomodation of gentlemanly philosophers to the new world of science did not prevent the gradual decline in social prestige of natural knowledge. A new and artificial distinction between "pure" and "applied" science might enable the London and Oxbridge elite to salvage some self-respect from the inroads of the newer scientists and their technological orientation. But the inescapable truth was that the rewards of professional science were unavoidably limited, even if middle class. As Charles Babbage put it in 1851: "The estimate which is formed of the social position of any class of society, depends mainly upon the answer to these two questions: – What are the salaries of the highest offices to which the most successful may aspire? What are the honorary distinctions which the most eminent can attain?...the highest position a man of science can attain, and that but very rarely, is a baronetcy; ...the highest salary is about £1,000 a year. When this is compared with the most successful prizes in the army, the navy, the church, or the bar, it shows at once the inferior position occupied by science."[30]

By the mid-nineteenth century British science had become middle-class, professional and technologically utilitarian in rhetoric if not in practice. It is a paradox that as the interest of industrialists waned, the appeal to utility grew correspondingly more urgent in the continuing struggle to finance and equip the scientific enterprise. Justus Liebig correctly observed of the British scene at this time that "only those works which have a practical tendency awake attention and command respect, while the purely scientific works, which possess far greater merits, are almost unknown... In Germany it is quite the contrary."[31] The determination of such spokesmen as Lockyer and Huxley to stress the pragmatic, professional and practical aspects of natural knowledge thus takes on a different hue. For good or ill, natural knowledge as a suitable pursuit for gentlemen had been overtaken by the idea of trained and organized common sense working for national, technological and utilitarian ends. The image of science had been transformed, and in that transformation we may see some dim foreshadowings of our present civilization and its discontents.

FOOTNOTES

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1 *The Quarterly Review,* 51 (1834), 58-61 as quoted in Sydney Ross, "*Scientist:* The Story of a Word", *Annals of Science,* 18 (1962), 71-72.

2 Norman Lockyer in *Nature,* 2(1870), 449 as quoted in George Haines IV, *Essays on German Influence upon English Education and Science, 1850-1919* (Connecticut College Monograph No. 9; 1969), p. 53: Thomas Henry Huxley in *Science and Education. Essays* (New York: D. Appleton & Co., 1901), p. 45.

3 See for instance A.R. Hall, *The Scientific Revolution. 1500-1800.* (2nd ed., London: 1962).

4 But see E. Mendelsohn, "The Emergence of Science as a Profession in Nineteenth-Century Europe" in *The Management of Scientists* (Boston, Mass.: 1964), pp. 3-48.

5 A more extended historiographical discussion than is possible here may be found in my articles "Science: Has its Present Past a Future?", *Minnesota Studies in the Philosophy of Science,* 5 (1970), 112-133 and "Science, Technology and the Industrial Revolution", *History of Science,* 2 (1970), 76-89.

6 Quoted by E. Robinson and A.E. Musson, *James Watt and the Steam Revolution* London: Adams and Dart. 1969, p. 1.

7 See J D Bernal, *Science in History* (London: Watts & Co., 1954), p.385 and A. Koyré in *Scientific Change,* ed. A.C. Crombie(London: 1963), pp. 852, 856.

8 As in D.S. Landes, *The Unbound Prometheus* (Cambridge: 1969).

9 Quoted from *Coningsby, Or the New Generation,* with an introduction by W. Allen

(London: John Lehmann, 1948), p. 148. Disraeli's novel was originally published in 1844.

10 See e.g. M.D. George, "London and the Life of the Town", in *Johnson's England,* ed. A.S. Turberville (Oxford: 1933); G.N. Clark, *A History of the Royal College of Physicians of London,* 2 vols. (Oxford: 1964-66), esp. ch. 27.

11 [Anon.], *Sketch of the Rise and Progress of the Royal Society Club* (London: 1860), pp. 20-21.

12 See T.E. Allibone, "The Club of the Royal College of Physicians, The Smeatonian Society of Civil Engineers and their Relationship to the Royal Society Club", *Notes and Records of the Royal Society of London,* 22 (1967), 186-192.

13 See D. Hudson and K.W. Luckhurst, *The Royal Society of Arts. 1754-1954* (London: 1954); H.J.J. Winter on the Peterborough and Spalding Societies in *Isis,* 31 (1939), 51-59 and *Archives Internationales d'Histoire des Sciences,* 3 (1950), 77-88.

14 See, *inter alia,* J. Gairdner, *A Sketch of the Early History of the Medical Profession in Edinburgh* (Edinburgh: 1853); A. Duncan, *Memorials of the Faculty of Physicians and Surgeons of Glasgow. 1599-1850* (Glasgow: 1896); H.F. Berry, *A History of the Royal Dublin Society* (London: 1915).

15 See for example F.W. Gibbs, "Itinerant Lectures in Natural Philosophy", *Ambix,* 8 (1961), 111-117 and E.G.R. Taylor, *The Mathematical Practitioners of Hanoverian England,* 1714-1840 (Cambridge: 1966).

16 A full listing is not possible here, but some examples may be given. Typical of the "general scientific" societies are the Manchester Literary and Philosophical Society (1781), the Royal Society of Edinburgh (1783), the Newcastle Literary and Philosophical Society (1793) and the Philosophical Society of Glasgow (1802). Generally at a slightly later period we have the foundation of such specialist societies as the Royal Astronomical Society (1820), the Zoological Society of London (1826), the Berwickshire Naturalists Club (1831), the Yorkshire Geological Society (1837), and a whole rash of local natural history groups. Agriculture is represented by, for instance, the Royal Highland and Agricultural Society of Scotland (1784), the Royal Horticultural Society (1804) and the Yorkshire Agricultural Society (1837). Technology was the focus of many societies formed in the 1850's and 60's, as the Incorporated Society of Engineers (1854), the Manchester Association of Engineers (1856), the Institution of Gas Engineers (1860), and the Royal Institute of Chartered Surveyors (1868). Detailed study of the temporal, geographic and interest shifts in the formation of societies would obviously be rewarding. The overall statistics were calculated from ***The Yearbook of Scientific and Learned Societies of Great Britain and Ireland*** (London, 1884 and subsequent years).

17 N. Hans, *New Trends in Education in the Eighteenth Century* (London: 1951), pp. 32-33. Hans' figures are derived from entries in the *Dictionary of National Biography,* with some additions. In fact his statistics cannot easily be extended, as the basis of his procedure is not adequately explained. The 1826-45 figures cited here are therefore based on an analysis of men cited in the *Dictionary of National Biography,* whose names begin with the letters A and B. Obviously these tentative results should be viewed with caution. A thorough analysis, refining and replacing Hans' statistics, would be of great value.

18 Quoted from H. Lonsdale, *The Worthies of Cumberland. John Dalton* (London: 1874), pp. 75-77.

19 There is no adequate history of the Society, but see R.A. Smith, *A Centenary of Science in Manchester* (London: 1883).
20 For further details see my *John Dalton* (Cambridge, Mass.: 1972), *passim.*
21 A. Geikie, *Life of Sir Roderick I. Murchison,* 2 vols. (London: 1875), vol. 1, pp. 94, 129.
22 See Sir Harold Hartley, *Humphry Davy* (London: 1966), and the review in *Science,* 160 (1968), 870.
23 These figures are from the tables in Sir Henry Lyons, *The Royal Society. 1660-1940.* (Cambridge: 1944), Appendix II.
24 From Erasmus Darwin's *The Botanic Garden* (1791) as quoted in W. Eastwood, ed., *A Book of Science Verse* (London: 1961), p. 96.
25 *Memoirs of the Manchester Literary and Philosophical Society,* (2nd ed.; London: 1789), vol. 1, pp. xv and 19.
26 See T. Martin, "Origins of the Royal Institution", *British Journal for the History of Science,* 1 (1962), 49-63; A.E. Musson and E. Robinson, *Science and Technology in the Industrial Revolution* (Toronto: 1969), *passim.*
27 See Sir David Brewster, *Memoirs of the Life, Writings and Discoveries of Sir Isaac Newton,* 2 vols. (London: 1855), vol. 1, pp. 102-104.
28 See *North British Review* (American edition), 9 (1850-51), 126-158, and A.D. Orange, "The British Association for the Advancement of Science: The Provincial Background," *Science Studies, 1* (1971), 315-329.
29 Full details cannot be cited here. See however J.B. Morrell, "Individualism and the Structure of British Science in 1830", *Historical Studies in the Physical Sciences,* 3 (1971), 183-204.
30 C. Babbage, *The Exposition of 1851* (London: 1851), pp. 191-193.
31 Quoted from George Haines IV, *German Influence Upon English Education and Science, 1800-1866* (Connecticut College Monograph No. 6; 1957), p. 53.

Science and Social Values in 19th Century America:
A Case Study in the Growth of Scientific Institutions

SCIENCE AND SOCIAL VALUES IN NINETEENTH-CENTURY AMERICA: A CASE STUDY IN THE GROWTH OF SCIENTIFIC INSTITUTIONS

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In any culture some values favor, others retard or positively oppose the development of science. Even within Western Europe, national and religious differences have been seen as peculiarly immanent in shaping variant patterns of scientific and technological growth. Hesitant mid-twentieth-century experience with social engineering in the so-called developing nations has simply dramatized the intricate relationship between social values, institutional forms and the growth of a scientific community. The following pages represent a case study in the role of social values in the creation of a particular scientific and technological institution – the agricultural experiment station – in a particular developing society, the United States in mid-nineteenth century. A small group of European-trained chemists shaped the movement for experiment stations and the body of this paper analyzes the shared assumptions and sources of emotional assurance which motivated these young men.[1]

The beginnings of agitation for the creation of experiment stations in the United States can be traced to the 1850's, and in particular to the ideas and efforts of a group of young chemists who studied together in Germany in the mid-1850's. The most articulate and tenaciously entrepreneurial of these German laboratory companions were Evan Pugh of Pennsylvania and Samuel William Johnson of New York State. Upon his return to the United States, Pugh guided the development of Pennsylvania's agricultural college into an early model for other such institutions before his premature death in 1864. Johnson returned to New Haven, an eventual professorship in Yale's Sheffield Scientific School, and leadership in the establishment of America's first agricultural experiment stations. Because of their importance as institution builders, I have singled out Pugh and Johnson for somewhat more detailed discussion. There existed, nevertheless, a revealing similarity in the shared values and experiences which shaped the motivational structure of all the Americans who studied chemistry in Europe during the 1850's – and a particular unity of commitment among some half dozen who were to make a career in agricultural chemistry.[2]

None of these young men came from particularly wealthy backgrounds; one at least, Evan Pugh, from relatively humble origins.[3] All had to overcome formidable obstacles in acquiring an education and in establishing a scholarly career. Though chemistry had, in the 1840's, come to replace geology as the generation's "glamour" science, it offered at best a problematical career in a society still unwilling to recognize and support the professional research scientist. When these young men boarded ship for study in Europe, it constituted a very real act of faith.

In mid-nineteenth century, most practicing American scientists served as college teachers; such positions entailed enormous teaching burdens and assorted pastoral duties. Research was never assumed to be a condition of employment. Most American professors, as a student chemist writing from Berlin expressed it, "are worked to death and many [never] in the course of their lives publish one single original paper or contribute one single new fact to science." Even the politically compromised desks in the government's patent office were an extremely desirable place for a young chemist; such posts paid better than almost any professorship and work ended at three, allowing "time enough for study or research."[4] Not surprisingly, there were no provisions for formal graduate training. Until the late 1840's, advanced training in chemistry was to be found only in a handful of private analytical laboratories, ad hoc, expensive, and often inadequate by contemporary European standards. Even Yale's Analytical Laboratory – opened in 1847 and a genuine and important forerunner of true graduate training – was in form not much different from competing private analytical laboratories; fees for analyses and the tuition of private students provided its only income.[5] And, despite America's theoretical acceptance of the self-made man, scientists complained again and again that material success alone seemed to determine such social acceptance. A European student, one American explained by way of contrast, "may dress in the coarsest & cheapest garb & . . . be admitted to that society for wh. his intellectual powers fit him." American attitudes provided a dismaying contrast. Despite such consistent discouragement, however, a growing number of American would-be chemists were – by the early 1850's – in residence in European laboratories. What, one cynic among them wondered in 1853, would become of them when they returned to the United States? Even the ordinarily sanguine Samuel W. Johnson could joke that a rich wife was a young chemist's only hope for success.[6]

Why then did these Americans make the difficult decision to study abroad? Even more specifically in terms of our case study, how did those among them concerned with the growth of agricultural chemistry justify their commitment? There were, it seems to me, four principal sources of motivation and legitimization and I should like for the moment simply to enumerate them, then elaborate these aspects of their thought in somewhat greater detail. First, many sought an avenue for the fulfillment of individual aspirations in a role seemingly untainted by the demeaning compromises and materialistic standards of the business world. A second and related factor was the stimulus provided by the energies of evangelical pietism. The decades between the 1820's and '50's had been marked by a mood of intense, even millennial enthusiasm. Men accepting this strenuous faith had necessarily to pursue their chosen vocation with an intense seriousness of purpose – and for a few Americans the career of scientist seemed a higher vocation than that normally undertaken by ambitious young men of their generation. Thirdly, all shared an unquestioning faith in the unambiguous virtue of progress – and, as we shall emphasize, distinctions between the material and spiritual aspects of economic, technological, and scientific progress were almost never made. A shared nationalism also legitimated what might have been seen as elitist and personal ambitions. Within the assumptions of this nationalism, moreover, agriculture played a particularly significant role; providing technological and thus economic aid to American farmers was a goal of more than temporal dimensions, for the individual landowning farmer had come to assume a pivotal role in the accepted structure of America's virtue-embodying social order. What I am suggesting then is a configuration of logically distinguishable, yet emotionally consistent – indeed synergistic – assumptions and sources of emotional reassurance. Such a world view served to legitimate personal ambition and thus the desire for both institutional innovation and intellectual achievement.

In examining the careers of America's pioneer agricultural chemists, perhaps the most striking similarity is their religiosity. Even though they came from widely varied backgrounds (Johnson, for example, was a Congregationalist, Pugh a Welsh Quaker), they all adhered to a peculiarly evangelical and intensely pietistic faith, their lives necessarily consecrated to a worthy stewardship. Samuel Johnson, for example, worried frequently about the vitality and strength of his commitment to science. "My studies have been profitably and steadily pursued," he wrote home from Yale in 1850, "and the way seems open for further prosecution. Yet I have sorrow that some hours have not been well spent, – that the motive of my industry and zeal in study, has not been a higher one, such as my Heavenly Father could approve. But I have a greater joy. It is that I am determined through Christ who strengtheneth me to walk in all the ordinances of the Lord, blameless." On another occasion, Johnson wrote to his father that

> Since I left home 2 months have elapsed. 6 times 2 months is a year, 20 or 30 is all I can hope to live – Have I time to do more than my duty to God? Have I time to jest and trifle when so short a time only separates me from the presence of my God?

Precious hours could not be wasted; the discipline of the laboratory was a necessary improvement of whatever skills God had granted. It was only natural that Johnson's brother-in-law should have encouraged him in his scientific vocation with the injunction that "agricultural science is your missionary field and you are responsible to the amount of some talents for its cultivation."[7] Johnson was not atypical. Pugh was even more ascetic in his personal habits; he detested all artificial stimulants, tea and coffee as well as alcohol and tobacco. His meticulously kept European journal provides a good many expressions of muscular indignation at German beer drinking and casual hygienic attitudes; such habits implied no enviable state of spiritual health.[8] Similarly, John P. Norton, co-founder of Yale's Analytical Laboratory and Johnson's teacher, also worried about the strength of his vocational commitment. God has been good to me, he confided to his journal in 1846 as he contemplated a second European study trip, though I have done little to deserve it. "May he keep me from all evil ways during this second absence, and may I be led to improve my time so that I shall be fitted to do much on my return." When, on another occasion, his child lay seriously ill, Norton reflected again in his journal:[9]

> This uncertainty of life ought to prove to me also a solemn warning and to remind me how imperfectly I am performing any one particular of my duties, and how many things I leave undone.

Obviously there is much of the conventional in such rhetoric; yet in its pervasiveness and intensity one senses genuine emotional conviction. It implied that scholarship would be undertaken with an unswerving seriousness of purpose; and it goes far to explain why men such as Norton, Johnson, and Pugh would have embarked upon that very act of faith with which they mounted the gangplank for their European apprenticeship.

This pietistic commitment to science as vocation was not, of course, limited in mid-nineteenth-century America to would-be agricultural chemists. It marked the careers of many other scientists as well. Edward Hitchcock, for example, professor, geologist, and probably the most widely read expositor of natural theology in his generation, could conclude in examining the state of his spiritual health that scientific pursuits were indeed "a means of personal sanctification."[10] It was, in many ways, the similarity rather than the difference between scientific and religious values which made it natural for many Americans to move fluidly from one intellectual realm to another.

The scientific vocation provided many of the spiritual compensations demanded by men of this generation. It represented no conflict in life-style with that of the traditional religious leader, but served rather as an alternative, offering many of the values embodied in the ideal type of the spiritual teacher. It was a role in which success could be achieved not in terms of demeaning material standards, but as a result of contributing to human knowledge – and thus, they never doubted, to human welfare and morality. Pugh, for example, explained to his journal that he knew "no higher standard of greatness (other conditions such as morality, etc., being equal) than that of being a great scientific man." More poignant are the words of a contemporary unable for personal reasons to enter upon the life of science.

> But after all it is something to be conscious of such a life. It is right to feel oneself allied to a higher order of beings by holding in common with them faculties which the base votaries of Mammon all around us do not possess, and for possessing which they despise us. They will brand me a *visionary* and cast me from the pale of their fellowship – nay this is daily done and I am made to feel the biting pangs of their sarcasms...

"The pursuit of knowledge," as Samuel W. Johnson explained,

> furnishes its own exceeding great reward, independent of the voice of human flattery. Yet reputation is not to be slighted, for where well-founded it enlarges the field of usefulness and enables its possessor to wield a mighty influence upon the minds and hearts of his fellow men.

The inspiring lives of great scientists, Johnson explained, encouraged him "to tread cheerfully the path of science, though alone and exposed to the sneers of the vulgar and ignorant."[11] Images of isolation and moral heroism appear with illuminating frequency in the writings of would-be American scientists in this generation.

The self-contained and protectively removed quality of the scientific confraternity must too have seemed attractive for at least a few lonely and introspective young men; the life of science promised a secure and legitimate identity. It was an identity, moreover, in which one's hopes for achievement could be defined in terms outside those of the local community. One senses in America's sprawling, scattered, and relentlessly masculine society a group of young men who sought contacts outside the frustrating, perhaps even threatening routine of daily existence. Let me simply quote a few wistful phrases from a letter exchanged between two young midwestern botanical correspondents: "I enclose my photograph," one wrote, "do you think we will do to be intimate friends?"[12]

Religion and the peculiar qualities of the scientific role were not alone in shaping the configuration of values and assumptions which motivated our protagonists. They also shared a similar attitude toward progress, an attitude central in their commitment to applied science. As workers active in a field potentially relevant to human needs these young Americans assumed that their work would have a moral and social significance – and not simply prove a source of personal intellectual satisfaction.

The vast majority of nineteenth-century Americans never doubted that human beings had progressed and that this progress – inevitably – subsumed dimensions both moral and material. It was inconceivable to them that the steam engine and morality were not somehow interconnected. It was unthinkable that the failure of, let us say, the Burmese to produce such artifacts was not somehow related to their lack of evangelical Christianity. Progress and technology were not only integral but justifying elements in the widely accepted vision of America's higher moral order.

Improvements in man's material comfort created precisely those conditions in which his moral and spiritual health might improve; or so it seemed. Even so careful and pious a thinker as Evan Pugh entertained no reservations. Scientific progress, he explained to a farm audience, is certainly not of a "higher character than moral and religious advancement. It only stands," he elaborated, "in such relation to these as does the engine upon the railroad to the human freight which it hurls along the iron track."[13] Derailments did not enter into this design and the destination was never in doubt. Let me refer to the sentiments of Samuel W. Johnson's father, a pious and prudent New York State farmer. "It is right and proper," he urged his son,[14]

> that every one should be well employed in doing and being useful, in bettering the condition of our fellow-beings in the concerns of the present life, in making improvements. In many things surely this is an age of improvements. The steamboat was a wonderful achievement in 1807. . . . But who thought of five hundred or more persons in ten or twenty coaches flying on iron rails at the rate of 30 or 40 miles an hour without horse or mule, but more than twenty years ago was that event consummated. Then the electric telegraph soon followed,. . .It would seem that the Lord is lavishing temporal blessings in great abundance upon Christendom, and particularly upon that part now Protestant. "Has God so dealt with any other people?"

That men who lived in increasing material comfort would also live more piously was an assumption so visceral that the vast majority of mid-nineteenth-century Americans never realized it was an assumption – let alone questioned it.

So ingrained was this faith in the ability of science and technology to improve man's spiritual condition, that it remained ineluctably alive while the explicitly spiritual energies which inspired the generation of the 1850's began to recede. Thus Johnson's most successful student, W. O. Atwater, a leading nutritionist and prominent Methodist layman, could in 1893 justify his devotion to nutrition investigations by declaring that "the time has come when we must get at the physical basis of human living if we are going to make the best provision for intellectual and moral progress." From such a position it demanded no great leap of intellectual evolution to arrive at the characteristic – if vulgar – sentiment of an agricultural college dean who explained in 1912 that "efficiency and morality may not be synonymous, but they are mighty good chums."[15]

The worship of productivity as the essence of and index to progress and the infusion of this assumption with an aura of nationalism and morality has persisted into the present with a dismaying weight of moral inertia. The generation of the 1850's played an unavoidable role in forging the habit of seeing all social and economic problems as solvable through the deus ex machina of increasing productivity – a position which has conveniently obviated the need to examine social policy alternatives.

In this complex of attitudes toward science and human progress, a vigorous nationalism assumed a natural place. To the earnest young advocates of agricultural science in the 1850's, America's peculiar virtues were unquestionable – despite the powerful counter-attraction of European culture and learning. Their ambivalence toward Europe was particularly marked in relation to Germany. Its government seemed despotic, its common people ignorant, impious, and tradition-bound. Though American students might concede that German pure science led the world, their own American countryman seemed far more skillful and ingenious in the application of science and technology to the improving of man's lot.[16] Even more important to many Americans was the vast difference between German and American moral expectations. A Göttingen friend of Johnson and Pugh observed of German students that their "only pleasure seems to be in drinking beer, smoking pipes, and fighting duels." A generation later – in the 1870's – Americans continued to express a similar ambivalence toward things German. Henry Rowland, Johns Hopkins physicist, confessed, for example, that:

> It is only since coming to Europe that I have been able to understand my own countrymen and appreciate their good qualities. . . . I believe that I can say with pride that there is not a more moral people on earth than our own, and this will account for some of our social habits which I often see criticized.

Religion seemed dead in Germany, mere ritual and social convention. Perhaps this secularism, another agricultural chemistry student at Gottingen wrote in 1870, explains "an intellectual development in the upper classes that stands in marvelous contrast with the beastiality of all classes."[17]

Yet many Americans did develop a warm feeling for some at least of their German hosts, their drinking and good fellowship, their hospitality, their relaxed attention to eating, to music and literature. And, of course, German cultivation of the sciences found no parallel in America. The German professor, moreover, in his dedication to research and apparent disdain for material goals provided an appropriately ascetic model for idealistic young men. Students recalled too their shared hardships, their scrimping to buy books, their vacation-time sightseeing and hiking, their crude efforts at cooking. Not surprisingly, almost every American who studied chemistry in Germany in the 1850's strained his – or his family's – financial resources to the utmost in an effort to extend his stay.

But none, so far as the available evidence indicates, even contemplated the possibility of leaving their native soil for Germany or any other country. Would-be agricultural chemists – whose particular ambitions I should not like to lose sight of in delineating the more pervasive attitudes in which they shared – felt not the slightest temptation to deny their social responsibility. Evan Pugh, for example, though engaged in important research at England's Rothamsted experimental farm, explained to Johnson that "they have offered me $500 to stay next summer but I feel that I must get home. There is a field there upon which the harvest is great and the laborers are few." Johnson too, though anxious to extend his European stay, felt that he ought "to go home and put a shoulder to the wheels of progress in my young native land with all her youthful stains vastly more glorious than the monarchies of Europe."[18]

But before following these student travelers to their North American homeland, let me – at the risk of seeming obvious – reemphasize that their European years in sum intensified original motivations and imparted a new unity of vision to disparate views. The shared experience of alienness in a civilization to which one maintained a consistently ambivalent position, combined with the peculiar ideological regalia of the German academic world, only strengthened commitments both to science and to the role of purveyors of science in service to society. (A reciprocal ambivalence toward things American would only have provoked guilt and thus a renewed activism.) Perhaps most important, the German experience gave to American students a particular body of techniques and concepts, knowledge which at once justified and – in a sense – constituted the peculiar status of the man of learning. One cannot well disentangle the consciousness of adherence to a discipline from the specific techniques and ideas which constitute the intellectual content of that discipline at any moment in time.

Once he had accepted the values of the world of academic science, the American scholar could measure achievement primarily in terms of acceptance as a creative scholar by his disciplinary peers. Such acceptance was, of course, based on research and publication. Thus American chemists necessarily returned to their native land not only with the reformer's zeal – but with a blueprint to guide them. The need for adequate laboratory facilities and research time dictated a specific program for institution building. Conditions appropriate for research and publication were thus always an organic part of the demands for agricultural education and experimentation formulated by this handful of German-trained chemists. In agriculture, moreover, the legitimacy of such demands was generally underscored by their unquestioned conviction that only first-rate research would prove ultimately most beneficial to the agricultural producer; there could be no conflict between science pure and applied.

Pugh and Johnson as well as a number of their friends had been particularly impressed during their student years by Germany's infant network of agricultural experiment stations. To these young men committed to help in bettering man's lot, the need for improving American agriculture was especially pressing, not only because of the farmer's place in the pantheon of national values, but because of a shared faith that chemistry could and would be readily applicable to farm problems. Thus the establishment of agricultural experiment stations on the German model promised not only the opportunity to contribute personally to a growing research area – but to do good in a more general and fundamental way. For some Americans at least, the entrepreneurial impulse could manifest itself in only limited and morally suitable contexts.

As early indeed as 1854, Pugh and Johnson already planned a campaign of educational and scientific reform. In discussing the possibility of establishing an agricultural school in Pennsylvania, Pugh warned Johnson, "It still may be best to *'compromise'* matters; and after the thing is once going and, its operations acquire the confidence of the public and those interested in its maintenance, &c., it may be made what (and *all* what) we would want it to be." A year later Pugh wrote again, urging Johnson to cultivate the "really scientific agriculturists" as a necessary first step in creating a base of support for their reforms.

> I don't doubt but that if one got into a place where the arrangements were not the best in the world for the promotion of Ag. science he might *bend* matters gradually

> into a proper course. One must first gain the confidence of interested persons, and then *influence* over them follows.[19]

Allies in editorial circles and in state and local agricultural societies were necessary paths to such influence – and both Pugh and Johnson wrote and spoke widely, assiduously cultivating personal contacts. Their themes were predictable enough.

Agriculture, they reiterated, must be made rational and scientific. Only trained scientists could ultimately guarantee true progress; the farmer's fear of the "mere theorist" was sadly misplaced. Standards in agricultural education and research would have to be raised and only through agricultural colleges and related experiment stations could these goals be achieved. As early as the mid-1850's, both Pugh and Johnson saw experiment stations as a necessary component in a proposed agricultural college system. (To be realized in the Morrill Act of 1862.) By the late 1850's and early 1860's, Pugh in particular began to emphasize the need for Federal support if experiment stations – and the research opportunities they implied – were to become a reality. Pugh had become frustrated after years of lobbying with his state's unsympathetic legislature. "I have spent the whole vacation," he wrote in 1861, "dogging at our legislature for money."

> I have been put off, trifled with, cheated, deceived and humbugged in a great variety of ways till now the session is nearly to a close and yet not one dollar voted... I am a little blue about it. Blue because all my vacation was wasted with those legislator blockheads – blue because honesty has not availed us in a righteous cause.

Proper agricultural research, Pugh reassured Johnson during the Civil War, could never be undertaken until government paid

> back a tythe of what it already owes science in order to carry them out. But my dear fellow get at it, starve along as best you can and I will point at you starving when the proper time comes to lay the question of a *station* before Uncle Sam. I am satisfied the old man will help you just as soon as he arranges matters on his cotton estates.[20]

In *all* education, Pugh and Johnson urged farmers and legislators, especially in agriculture and industry, science would have to constitute the essential substance. To establish an agricultural college and not place it under the guidance of a man with the most advanced scientific training would be to create a watch, as Pugh put it, without a main-spring. There was no doubt in his mind that he and other German-trained scientists should and would perform this function. Not surprisingly, when Pugh was called in 1857 to assume presidency of Pennsylvania's infant agricultural college – christened at first the Farmer's High School – he began almost immediately to offer advanced and specialized analytic training. Both in public and private – though in somewhat different terms – Pugh defended his atypical commitment to excellence and specialization. The great German universities, he explained in 1864 to the state legislature, base their superiority not upon facilities[21]

> ... but it consists in the large number of their professors and the profoundness which necessarily results from this large number and from their unceasing devotion to the subjects they teach, so that the student lives, moves, and breathes in an intellectual atmosphere. . . Our Industrial Colleges must be experimental institutions, because they are devoted to subjects which need much more investigation before they can be taught with entire satisfaction...

There was no possible conflict, these would-be reformers argued, between the needs of scientists and those of the American economy. Fundamental scientific progress – and thus, all assumed, economic growth as well – would not come about through the popularizing of science to a virtuous yeomanry, but through cultivating "a few students to a high standard." "What a good influence," Pugh congratulated himself,[22]

> the European system has in giving us a contempt for that superficial smattering of everything without even an *idea* of what thoroughness in anything is which is too characteristic of our American system of education and our American notions of what education should be.

Mid-nineteenth-century American scientists had often to be entrepreneur and publicist as well as investigator; Johnson and Pugh were clearly well suited to this promotional role. They accepted as necessary and inevitable the long hours spent in cultivating men of influence, in speaking at fairs and farmers' clubs, in writing popular articles for farm weeklies. The humble arts of the lobbyist had been harnessed and tamed by the transcending logic of piety and patriotism.

Despite these vigorous efforts, however, it was not until the 1870's that American experiment stations actually came into being. And even then, as we will emphasize, the founding generation's particular ability to compromise in the service of absolute commitment laid the groundwork for an endemic ambiguity in the history of this institution – an ambiguity characteristic of a good many other relationships between the American scientific community and the society which has supported it. Yet it would, I think, be a mistake to emphasize alone the implicitly negative aspects of the mutually ingenuous quid pro quo which underlay this ambiguity. Though the structure of political alliance and ideological assumption forged by the generation of the 1850's became increasingly habitual and confining, it was perhaps a necessity in an open society seeking to maximize economic growth yet generally tolerant of science only in the form of rhetoric – or when it promised tangible returns.

It was a grimly inhospitable reality which confronted German-trained chemists in mid-nineteenth-century America as they disembarked after sailing homeward. A few returned to an expected business career. Here laboratory skills and the motivation implied by a willingness to study in Europe made success plausible; J. F. Magee, for example, founded a firm specializing in photographic chemicals; R. H. Lamborn became a prosperous mining engineer and entrepreneur; both had been good friends of Pugh and Johnson in Germany.[23] But for those who sought academic careers, prospects were still bleak indeed. Pugh, as we have noted, was made president of an embryonic and physically isolated agricultural college, by European standards a secondary school. J. P. Kimball, another of the Göttingen American colony, could at first only find work as an assistant in the geological survey, then a brief position with an abortive agricultural college in New York State, an experiment destroyed by the Civil War and – as Kimball put it – "apathy and neglect." Even men who were to make eminent careers in later decades found readjustment to American soil painful indeed. George C. Caldwell, later a professor at Cornell, spent almost a decade in grimly depressing teaching positions before arriving at Cornell's comparative luxury. Caldwell's diary records a typical day during this trying period:[24]

> My own work presses me hard . . . . I am up at six, work over notes of lectures till 7:30 – then breakfast . . . Then up to the college and get experiments ready for classes. Lecture at 10, recitation . . . at eleven. From about 11:45 to 12:45 I rest and behold my wife and enjoy her blessed company and my dinner. From 1 to 3 is the distracting Doctor here and Doctor there of my sixteen or eighteen laboratory students. . . . By the time 4 o'clock comes I am pretty well used up and ready for recreation, but must find my recreation in continuation of Laboratory work on my own account.
>
> I must be doing something, even though it be but little to save my reputation or myself from being forgotten by the circle of my scientific friends. . . . The evening is my only time to study – but with boys to keep in their rooms in study hours, I sometimes don't get much time to study.

The laboratory bench still seemed a dubious place for gentlemen. Charles F. Chandler, another Göttingen friend of Pugh and Johnson, and later professor at Columbia University, was forced in 1862 to correct "a great misapprehension on the part of some persons in regard to what is taught in the laboratory.

They have an idea that the laboratory students spend their time in mechanical operations, in acquiring a knowledge of the technical part of various branches of industry which are pursued by the lower classes of society."[25] Johnson, the real father of America's first agricultural experiment stations if any one individual can be presumed to deserve that title, received an assistantship at Yale's Sheffield Scientific School soon after returning to the United States. And though this was probably the most desirable academic position received by any of the Göttingen American colony, Johnson too had to spend countless hours in writing for popular audiences, in lecturing, in serving more than metaphorically as missionary of agricultural chemistry to the "spiritually dead" among Connecticut farmers.

Obviously, advocates of scientific research in agriculture had to deal with the assumptions – and power – of a laity at once skeptical and credulous. Though scornful of "mere theory," interested laymen still entertained a number of ingenuously optimistic scientific hopes. Most important was an uncritically positive attitude toward chemistry and its potential efficacy in solving economic problems. In regard to agriculture specifically, the popular impact of Liebig's work had made the rationalization of farming through chemistry an enormously and insidiously popular hope. (A student of Pugh's wrote, for example, from his father's farm that: "The barbarians expect me to raise corn without a cob I should think by the way some of them talk.") "Every farm should be considered a chemical laboratory," a representative popularizer explained, "and every farmer a practical chemist and philosopher: farming would then be honorable and lucrative." Were rhetoric alone a useful index to social priorities, the chemist would have been a favored citizen indeed.[26]

Unfortunately, the ordinarily vague expectations of laymen sometimes assumed embarrasingly concrete forms. Perhaps the most awkward of the popular assumptions which faced young agricultural chemists in the 1850's and 1860's was the illusion that simple testing procedures could ensure soil fertility; once missing constituents were identified in the test tube, they need only apply the prescribed fertilizer – and a marginal farm would become a source of profit. Worse yet, a number of chemical entrepreneurs – our European-trained chemists referred to them as quacks – deliberately ministered to such hopes by "promising to satisfy that vulgar notion."[27] Another pervasive misapprehension – even among those "intelligent farmers" most willing to support science – was that an experimental farm should show a profit. "Would it not be profitable," one supporter wistfully asked Pugh, "would the students and faculty commit wilful injury?"[28] Laymen in general demanded immediate and tangible rewards in return for their willingness to support science. In the words of another supporter of agricultural science, "the peculiar genius of our people must have something *practical,* something from which *dollars and cents* may be realized." Naturally enough, American scientist-entrepreneurs quickly learned to cast their appeals for support in the form of an enticing quid pro quo. An entomological contemporary of Pugh and Johnson, for example, was soon convinced that farmers would only support entomology if assured that it would increase their profits. "Much as I despise this sordid test of the utility of a science," he concluded, "I am forced to own its necessity when the public is to be enlisted."[29]

Yet men such as Pugh and Johnson could not turn their backs on fields ripe for harvesting. For both practical and ideological reasons, they persisted in their attempts to wring support from American society. "No worthy enterprise," Pugh wrote encouragingly to Johnson,[30]

> . . . can be accomplished without effort. If effort at first appear unavailing – if continued labor fail to produce the desired effect – if those for whom we labor close their eyes and stop their ears, and open their mouths to let *quacks* and *knaves* feed them still let not despair raise its scowling curtains before us.

But such missionary work was slow indeed, and the gathering of souls a difficult and discouraging task. Even after twenty years of devoted "political education," most enlightened and sympathetic laymen still failed to accept or comprehend the Göttingen world-view.[31] But the help of such influential laymen could still be solicited upon the convenient basis of mutual misunderstanding.

It was not until 1875 that Johnson's policies brought results. Under the immediate leadership of his politically acute student, W. O. Atwater, and influential farm publisher Orange Judd, the state of Connecticut established an agricultural experiment station. This was the formal initiation in the United States of an institution which has – as much as any other discrete factor – been responsible for the remarkable growth of productivity in twentieth-century American agriculture. In the dozen years after 1875, a number of states followed Connecticut's example and in 1887 the national government provided $15,000 a year for the support of an agricultural experiment station in every state – thus establishing the first significant instance of Federal support for scientific research and development in states and universities.[32] In mid-twentieth century the importance of such precedents can hardly be overestimated.

By the 1800's, conditions for research-oriented American chemists had begun only marginally to improve. The fledgling experiment stations promised relatively desirable positions. Yet the compromises demanded by an applied science context were making such positions less and less attractive to the best-trained among a new scientific generation. A measure of improvement in other areas – symbolized by the opening of the Johns Hopkins University and other pioneer graduate programs as well as expanding possibilities in industrial chemistry – paralleled the growth of an increasingly indigenous scientific esprit, one mirroring and utilizing the absolute self-justifications of the German professoriate in a fashion quite different from that of the young men of Pugh and Johnson's generation. In 1887, for example, the year that the Hatch Act establishing a national system of experiment stations was passed, the graduate-oriented Johns Hopkins University had already been in existence a dozen years and it was only natural for a Hopkins student to observe that[33]

> As far as I can see the chances for advanced research work in this country is very poor – the country is not old enough – such things will come only after the present race of money muckers has been turned to some agricultural use, I fear. Nothing *pays* here unless advertised and acceptable to the public and nothing is thought sane unless it is expected to pay.

This kind of alienated posturing would have been impossible for men like Pugh and Johnson – though they might well have agreed with the substantive burden of this criticism. Their peculiar commitment to their countrymen, to the necessity of improvement through pious activism, would have made such a position distasteful indeed.[34]

It is probably fair to concede that most experiment stations never achieved that successful synthesis of the pure and applied which had been so central to their founder's vision. The irony was complete. By 1900, the experiment stations still provided desirable and in some ways practically advantageous positions for young organic chemists. Yet the better-trained among these had become increasingly unhappy with the casual standards for original investigation maintained by many of the experiment station administrators and the unceasing pressure of these same administrators and an aggressive farm constituency for immediate results; farm leaders had already been courted assiduously for two generations with euphoric visions of prosperity to follow hard upon increased research appropriations. To this newer generation of scientists, Samuel W. Johnson's well-worn policies of farm-oriented research were narrow and constricting. He had become part of an old guard, an old guard whose peculiar contribution of energy, of idealism, and of advanced scientific training had long been forgotten.

The dilemma was probably insoluble. Working in an experiment station context before World War I, the scientist had either to adjust to a lack of autonomy, to shaping research hours in response to the demands of a client-constituency – or leave. "When I got through in Germany," one chemist trained in Germany in the 1870's put it in 1906, "I was fur die Wissenschaft, but since I have been connected with the expt. Sta. my thought has unquestionably [been] influenced by being constantly in close touch with practical men who make [their] livelihood fr. cultivating the soil. The question with them always is *how* can I do this or that to grow better crops for less money."[35] Experiment station research had to be shaped in response to the equally categorical, yet only partially consistent demands of the scientific disciplines on the one hand, and on the other hand, those of an imperious lay constituency. This implied sharing (if not surrender) of institutional autonomy bequeathed an ambiguous heritage to even the best agricultural research laboratories as in the twentieth century they sought to become contexts for high-level research in the biological sciences.

But such difficulties were in some ways implicit in the attitudes and contextual realities of the founding generation of the 1850's. "We must," one of these German-trained scientists wrote in 1852, "go out into the unscientific portions of our country and there raise up science, by so doing, we shall render a much more acceptable service to science than if we were propounders of some abstruse theories or the discoverers of new elements, planets or comets."[36] These men could not overcome the influence of this moral gradient; their European training served only to add immediacy and personal legitimacy (in the form of their recently acquired specific knowledge). To this small group of scientists and scientist-entrepreneurs, the discipline-oriented and self-conscious elitism of some of their contemporaries and many of their successors would have seemed egocentric indeed.

And in a sense they were correct. To accept the ultimate worth of applied research in mid-nineteenth-century America was to necessarily accept the constraints of working with and through an economically oriented client-constituency. And in accepting these terms such gifted institution-builders as Pugh and Johnson played an important role in the economic growth of American agriculture (and thus indirectly in that of the state universities and "basic" sciences as well). For it is hard to imagine how the historical circumstances which gave birth to the American experiment station movement would have allowed any alternative method of subsidizing agricultural research. Nor would these resources have been diverted to other means of underwriting pure or applied research. Neither individual units of production – the farmers – nor agriculture-related business would have considered such allocations of funds in the late nineteenth century.

The agricultural colleges and experiment stations have played an important role in the twentieth-century growth of American agricultural productivity – with its economic, social, and demographic consequences. Equally important, many of the precedents for government-designed and supported research – and the ramifications of this dependence for the scientific community – were forged most prominently in the agricultural colleges and experiment stations. In this particular sequence of historical development, the ideas and assumptions of the far-seeing and energetic founders of the 1850's played an important role, helping transmit ideas and institutional forms from the European metropolis to a similar yet distinct and physically distant culture. Their willingness to commit their energies to applied science provided an important element in ensuring an ultimately symmetrical development in America of the sciences, of technology, and of technologically oriented economy. Their energy and commitment were, as we have seen, as peculiar a product of this society as their assumptions about the nature of society and the scientist's social role – so meaningful to them yet so wistfully distant today.

Let me conclude with several more general observations in the form of a defense of some assumptions implicit in the organization of this paper. It might be objected that only a handful of Americans embraced a scientific vocation in mid-nineteenth century and that – as our own discussion has made clear – if some values pervasive in the culture were congenial to such a vocational choice, others were just as clearly antithetical. True enough. But it must be understood that we are dealing with a universe of individual personalities and coexisting if not always consistent cultural values. As they reached adulthood, Americans with individual personality needs chose among those careers made available to them by their culture – and in this choice found hopefully embodied a configuration of values appropriate to their emotional needs. In terms of our case study, I refer of course to those implications of anti-materialism, selflessness, service, and a potentially benevolent activism promised by the research scientist's role. Of course the scientific calling was a highly atypical one in mid-nineteenth-century America; indeed it seems fairly obvious that those very elements which made it atypical – even scorned by some – served only to enhance its attractiveness for those idealistic young men whose careers we have tried to explain.

I should like to clarify another and related problem. In implying that individuals chose the life of science as a form of adjustment, I do not mean to imply that all who made this decision were necessarily similar in personality. A commitment to the scientific vocation might be equally functional to the personality stabilization of individuals with quite different needs. Let me be a bit more specific. Evan Pugh seems – if I may be excused such instant psychobiography – on the basis of personal documents to have been extremely achievement-oriented, dominant in personal relations, as physically vigorous as he was ambitious; yet many of his culture's normal paths to achievement – politics and the law, for example – seemed to him tainted by materialism and sordid compromise. The role of scientific entrepreneur, on the other hand, promised accomplishment and, as we have argued, a spiritually irreproachable strategy for ego aggrandizement. Science was, moreover, an interest traditionally sanctioned by the Pennsylvania Quaker community in which Pugh had grown to maturity. The youthful Samuel W. Johnson seems, on the other hand, to have been equally ambitious, yet quiet and introspective. He too had been raised in an area in which enthusiastic religion had co-existed peacefully with a surprisingly strong interest in natural philosophy.[37] In both cases a commitment to science provided an appropriate means for attaining status security and individual achievement – as well as the expression of dominance and authority. What I would suggest, then, is that the scientist's life was equally functional in the adjustment of these young men to maturity – not that the function was precisely the same. Many other mid-nineteenth-century American scientists seem to have had other needs, ones for which the other-worldly and self-consciously elitist aspects of the scientific career seemed particularly congenial; to such investigators a devotion to applied science and its inevitable institutional compromises would have been intolerable, a denial of the emotional distance with which their vocation helped them to structure their relationship to society generally.[38]

FOOTNOTES

1 In this brief compass, I will avoid the structural problem of tracing the means through which such a small group of well-motivated and goal-directed men are able to effect a particular desired change in the larger society. The materials in this paper are drawn from a larger study of the experiment stations now in progress and in which such issues will be considered.

2 Fortunately, the personal papers of a good many of these agricultural chemists have, in varying degrees of completeness, been preserved, thus allowing greater insight into individual motivation than is normally the good fortune of historians of science. Most extensive are the William Brewer Papers at the Yale University Manuscript Division, the S. W. Johnson Papers in the Biochemistry Department of the Connecticut Agricultural Experiment Station, New Haven, and the Evan Pugh Papers in the Penn State Collection, The Pennsylvania State University. Also important are the John P. Norton Papers and George Brush Papers at Yale, the Charles C. Chandler Papers in Special Collections, Columbia University, and the George C. Caldwell Papers in Cornell University's Collection of Archives and Regional History. Extensive extracts from the letters and

journals of James F. Magee, another member of the Göttingen American colony, have been published: George Magee, ed., *An American Student Abroad* (Philadelphia: Magee Press, 1932). Selected extracts from the Pugh and Johnson Papers have also been reprinted: Elizabeth A. Osborne, *From the Letter-Files of S. W. Johnson* (New Haven: Yale University Press, 1913), and C. A. Browne, "European Laboratory Experiences of an Early American Agricultural Chemist- Dr. Evan Pugh (1828-1864)", *J. Chem. Educ.,* 7 (1930), 499-517.

3 Most had pretensions to middle-class status even if not possessors of great wealth. A relatively large middle class – as minimally defined by freedom from immediate dependence upon day wages – meant a comparatively large pool of *potential* scientists, even if only a very small absolute number found anything congenial in so atypical a career. There is no full-length biography of Evan Pugh, but see: Jacqueline Bloom, "Evan Pugh" (unpublished Master's thesis; Pennsylvania State University, 1960).

4 Benjamin Silliman, Jr. to W. H. Brewer, January 31, 1837, Brewer Papers; O. Wolcott Gibbs to George Gibbs, July 26, 1846, Gibbs Family Papers, State Historical Society of Wisconsin.

5 See Louis I. Kuslan, "The Founding of the Yale School of Applied Chemistry", J. *Hist. Med.,* 24 (1969), 430-451. For details of the laboratory's operations one may consult the letter-press copy book kept to record its affairs and now deposited in the Manuscript Division, Yale University Library. The financial circumstances of the laboratory are described by J. P. Norton in a revealing letter to Joseph Henry, December 5, 1848, Norton Papers.

6 The comparison between the degree of social acceptance granted students in Europe and America is taken from a revealing letter from C. M. Wetherill to J. F. Frazer, June 10, 1850, Frazer Papers, American Philosophical Society. George Brush to W. H. Brewer, January 20, 1853, and William Craw to Brewer, December 19, 1852, Brewer Papers.

7 S. W. Johnson to Dear Sister, December 30, [1850], Johnson Papers, and S. W. Johnson to Dear Father, Mother, Brothers and Sisters, December 1, 1850, Johnson Papers. These letters were written during a period of spiritual crisis and are thus more overtly pietistic than SWJ's usual and later prose. J. C. Baston to SWJ, [1855], cited in Osborne, *Letter-Files,* p. 94.

8 Pugh kept a detailed journal during portions of his German and English student years. There is a typed transcript as well as the originals in the Pugh Papers. Many entries reflect his moral stance, cf. that for September 9, 1855, which explains his distaste for all "alkaloids" – including equally tea, coffee, and opium – because all were unnatural. For a parallel study of the pietistic origins of contemporary American public health reform, see: Charles and Carroll S. Rosenberg, "Pietism and the Origins of the American Public Health Movement;. . .", *J. Hist. Med.,* 23 (1968), 16-35.

9 J. P. Norton, Diary, Entry for September 13, 1846, and August 3, 1851, Norton Papers.

10 Edward Hitchcock, "Private Notes", December, 1842, Hitchcock Family Papers, Amherst College Library. The introspective Hitchcock had also to confess that "distinction among scientific men has been with me a most powerful motive of action." "Private Notes", December, 1843. I should like to thank Stanley M. Guralnick for providing me with these and other references to the Hitchcock Papers. The emotional concordance between the life of science and that of the spirit found no awkward hindrances in the formal intellectual sphere. As is well known, a romantic theology with its traditional dependence upon arguments from design had become an unquestioned

commonplace among American Christians of all denominations in this period.

11 Entry for November 21, 1853; Theodotus Burwell to W. H. Brewer, April 21, 1852, Brewer Papers; S. W. Johnson to [A. A. Johnson], undated fragment, [1849-50], Johnson Papers.

12 J. C. Arthur to E. F. Smith, May 13, 187[8], Erwin Frink Smith Papers, American Philosophical Society.

13 *Address to the Cumberland County Agricultural Society at Their Fall Meeting, October, 1860* (Carlisle, Pa.: The Society, 1860), p. 5.

14 A. A. Johnson to S. W. Johnson, December 10, 1858, cited in Osborne, *Letter-Files,* pp. 130-131.

15 W. O. Atwater to Carroll Wright, October 16, 1893, Reel 6, W.O. Atwater Papers, E. F. Smith Library, University of Pennsylvania; Thomas F. Hunt, quoted in San Francisco *Call,* November 21, 1912. "In our new conception of a successful life," Hunt elaborated, "we do not have prosperity without morality, but we do have prosperity because of morality."

16 Compare the following comments by Pugh comparing American and German farmers. "But there is another consolation for the American scientific agriculturist – viz. we can safely say, that if our farmers should ever happen to be as ignorant as the German farmers we know they are not so *stupid* or so bigoted in following the old way." Pugh to Johnson, September 30, 1855. After forty years of work, however, S. W. Johnson had other thoughts. "An immense advantage enjoyed by the German investigator," he confessed in 1896, "consists in the fact that he has a clientele of landed proprietors who have themselves passed the strenuous discipline of the gymnasium and the university..." "President's Address', *Proc.,* Assoc. of American Agricultural Colleges and Experiment Stations, 1895-6, p. 45.

17 James F. Magee to Dear Brother and Sister, [1855], *An American Student Abroad,* p. 62; H. A. Rowland to J. C. Maxwell, March, 1876, as cited in: Samuel Rezneck, "An American Physicist in Europe: Henry A. Rowland, 1875-1876", *Am. J. Physics,* 30 (1962), 885; Charles W. Dabney, Jr. to My dear Pa, October 6, 1879, C. W. Dabney Papers, Southern Historical Collection, University of North Carolina.

18 Pugh to SWJ, [1857], Pugh Papers; SWJ to A. A. Johnson, July 3, 1854, cited in Osborne, *Letter-Files,* p. 62.

19 Pugh to SWJ, October 31, 1854, November 14, 1855. Cf. also Pugh to SWJ, March 15, 1855, in which Pugh discusses the possibilities of bringing a farm weekly "directly under our control." The symbiotic political alliance between agricultural scientists and administrators and "intelligent farmers" and farm spokesmen – that is, the more prosperous and better educated in the agricultural community – despite its undoubted efficacy in stimulating agricultural productivity soon became a characteristic – and in the twentieth century increasingly dysfunctional – aspect of the experiment station heritage. The success of the stations in increasing productivity meant – if I may be excused the crudity – among other things that on the whole the rich got richer and the poor, poorer. For those with education and the values conducive to innovation in addition to capital sufficient to allow a margin for innovation benefitted most prominently from technological advances in agriculture.

20 Pugh to SWJ, March 13, 1861, Pugh Papers. For the reference to the Federal government, see Pugh to SWJ, April 19, 1862. As early as 1860, Pugh had suggested the possibility of arranging a congressional appropriation to support experimental work, Pugh to SWJ, August 16, 1860.

21 The mainspring analogy occurs in a long and important letter from Pugh to SWJ, December 1, 1854, Pugh Papers; Pugh, *Statement made by Dr. E. Pugh . . . of the Agricultural College in reference to the Proposition to Deprive the College of its Endowment* (Philadelphia: William S. Young, printer, 1864), p. 5.
22 Pugh to SWJ, November 18, 1861.
23 Biographical material on Magee is to be found in Magee, *An American Student Abroad.* Lamborn left a large bequest and an extensive manuscript collection to the Philadelphia Academy of Natural Sciences. Unfortunately there is nothing in this collection which sheds light on his German years.
24 Kimball to Pugh, January 19, October 15, 1861, Pugh Papers; G. C. Caldwell, Diary, Entry for March 22, 1865, Caldwell Papers, Cornell University Collection of Archives and Regional History.
25 Chandler to Bishop Horatio Potter, January 25, 1862, Chandler Papers, Special Collections, Columbia University Library.
26 Samuel Halliday to Pugh, May 20, 1862, Pugh Papers; M. M. Rodgers, *Scientific Agriculture* (2nd ed. rev.; Rochester, N.Y.: Erastus Darrow, 1850), p. 17.
27 Pugh to SWJ, January 6, [1860], Pugh Papers. Cf. J. P. Norton to Solon Robinson, January 5, 1852, Letterpress book, Outgoing, Norton Papers. A fertilizer manufacturer, indeed, complained to Pugh that farmers sometimes assumed that a "guarantee" meant the guarantee of a crop. B. M. Rhodes to Pugh, April 11, 1862.
28 F. W. Cook to Pugh, August 14, 1861, Pugh Papers. Up through the First World War, experiment station administrators had to face the criticism of laymen who continued to assume that "an experimental farm" should be a source of profit.
29 G. Evans to J. P. Norton, March 19, 1849, Norton Papers; F. B. Hough to S. Haldeman, February 21, 1851, Samuel Haldeman Papers, Academy of Natural Sciences of Philadelphia.
30 Pugh to SWJ, March [16], 1855, Pugh Papers.
31 In 1875, for example, Orange Judd, the principal lobbyist in forcing the passage of Connecticut's pioneer experiment station, was a prosperous farm newspaper publisher and intimate friend of W. O. Atwater, a European-trained and research-oriented chemist. Yet Judd felt that the immediate and most important goal of the stations was to stop "manure swindling" – that is, the sale of misrepresented chemical fertilizers. "We must," he explained to Atwater in discussing plans for the station, "have something going on that will furnish *frequent* items weekly perhaps through the press." Judd to Atwater, July 6, July 23, 1875, Reel 1, Atwater Papers.
32 The best general history of agricultural research in the United States is still: A. C. True, *A History of Agricultural Experimentation and Research in the United States 1607-1925* (U.S.D.A., Misc. Pub. No. 251; Washington, D.C.: 1937). More recent and useful, though concerned largely with the history of formal policy discussion, is: H. C. Knoblauch, E. M. Law and W. P. Meyer, *State Agricultural Experiment Stations. A History of Research Policy and Procedure* (U.S.D.A., Misc. Pub. No. 904; Washington, D.C.: 1962). For a summary discussion of the experiment station scientist's role, see: Charles E. Rosenberg, "Science, Technology, and Economic Growth: The Case of the Agricultural Experiment Station Scientist, 1875-1914", *Agricultural History, 45* (1971), 1-20.
33 E. A. Andrews to J. J. Abel, June 6, 1887, John J. Abel Papers, Welch Medical Library, The Johns Hopkins University.
34 Even in mid-nineteenth century, of course, some American scientists with different

backgrounds and assumptions had tried to maintain a distance from the immediate demands of any particular client constituency. For example, two Harvard scientists – W. G. Farlow, a mycologist, and F. H. Storer, and agricultural chemist – though both active in training experiment station scientists always maintained substantial reservations in regard to the possibilities of conducting ultimately meaningful research in so pragmatically oriented a context. As early as 1880 Storer had warned S. W. Johnson – a close friend since their European student days – that "for successful research something more than good intentions is necessary; viz. technical training, modesty of thought, and an open mind." Farlow was even more openly scornful of the mycological pretensions of station and state mycologists. An Iowa farm boy commented after observing Farlow's reactions to a visit from an experiment station botanist that: "They do like to kick things & people here and set on them." Storer to SWJ, April 13, 1880, Johnson Papers; F. C. Stewart to L. H. Pammel, October 7, 1894, L. H. Pammel Papers, Iowa State University; Farlow to W. Trelease, January 5, 1885, Trelease Papers, Cornell University Collection of Regional History and University Archives.

35 J. B. Lindsey to E. W. Allen, September 9, 1906, Massachusetts File, Records of the Office of Experiment Station, Record Group 164, National Archives, Washington, D.C.

36 George Brush to W. H. Brewer, November 28, 1852, Brewer Papers.

37 Much of the Western New York State region in which Johnson grew up was known in prebellum America as "the burned-over district," because of the frequency and intensity of the revivals which swept through these counties. For an excellent study of this area, see Whitney Cross, *The Burned-Over District. The Social and Intellectual History of Enthusiastic Religion in Western New York, 1800-1850* (Ithaca, N.Y.: Cornell University Press, 1950).

38 To adequately document this point would demand another paper of the same length. For some examples of conflict between these aspects of the scientist's career, see the remarks concerning W. G. Farlow and F. H. Storer in n. 34 above.

The Ayrton Incident: A commentary on the Relations of Science and Government in England, 1870-73

He mocks the naturalist's pretensions vain,
Smiles down on Science with sublime disdain,
And frowns contempt on those who rather scan
The laws of Nature than the laws of man.

Portraits in Rhyme, *The Conservative,*
November 23, 1872.

THE AYRTON INCIDENT: A COMMENTARY ON THE RELATIONS OF SCIENCE AND GOVERNMENT IN ENGLAND, 1870-1873*

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INTRODUCTION

By the autumn of 1873 Gladstone's first Government was gravely troubled. Racked by dissention within the party, caught in the cross-fire between temperance and the brewing interest, the Government had scarcely time to fulfill its campaign promises of 1868. Gladstone resigned, and would have gone immediately into Opposition had not Disraeli declined to form a government. Disillusioned, and battered, the Liberal Government struggled on, to what seemed like a merciful defeat in the General Election of 1874.

Gladstone's defeat had many explanations. But among the great catalogue of Liberal miseries, the struggle between science and government, represented by the "troubles at Kew" remains today among the most illuminating. At the time, the "Ayrton incident", as it came to be known, was overshadowed by more popular issues, but it nonetheless made a distinctive impression upon the future relations of scientists and politicians. An analysis of this incident, which filled the literary weeklies in the summer of 1872, casts an important light upon the exercise of radical administrative reform, the power of the "scientific lobby", Gladstone's views on science, and upon public attitudes towards the existence of a scientific "Establishment" above the reach of parliamentary control.

I. THE LIBERALS AND SCIENCE

The first three years of Gladstone's first administration witnessed events of unprecedented importance to British science. Between the fall of Disraeli's Government in December, 1868, and the end of 1871, the "scientific movement", led by the British Association, the Royal Society of

Arts, and the new journal, *Nature*[1], had begun to draw public attention to the importance of securing more State provision for technical instruction, scientific education and fundamental research. From these beginnings grew the important campaigns for technical education and the "endowment of research" which were to play such important roles in the last quarter of the century. The Liberals had not neglected the politics of science. Whether for purely political motives, or for reasons of administrative reform which weighed so heavily with Gladstone's Chancellor, the Liberals had shown themselves careful of its growing importance.

Following the Norwich meeting in September, 1868, the British Association had set up a Committee–including Norman Lockyer, John Tyndall, T.H. Huxley and Edward Frankland–to consider the state of British physical science. From their deliberations grew a deputation to the Lord President of the Privy Council, which led in turn to a promise by the government to review the entire institutional position of British science. In May 1870, H.A. Bruce, Home Secretary, launched a Royal Commission on Scientific Instruction and the Advancement of Science, more familiarly known as the "Devonshire Commission". It sat for five years and published eight impressive reports.[2]

In 1870, the House of Commons sanctioned £6000 for clearing a site for a new Natural History Museum at South Kensington.[3] In September 1870, the tragic loss of H.M.S. *Captain,* a revolutionary new vessel with revolving turrets and a low side freeboard, pointed to the absence of experimental techniques and scientific staff in the Admiralty yards.[4] Science was also making inroads into university life. Between 1868 and 1872, the Clarendon Laboratory was built at Oxford; between 1871-1872, a physics laboratory was opened under Balfour Stewart at Owen's College, Manchester; in 1873 the Cavendish Laboratory was opened in Cambridge.

Meanwhile the demands of scientists on the State had begun to reach substantial proportions. In 1868 and again in 1869-70, the Admiralty placed first *HMS Lightning* and then *HMS Porcupine* at the disposal of deep sea investigations. Though these were not charged to the Civil List they led to the larger proposal of the *Challenger* expedition. In 1871, when the Liberals were obliged to accept responsibility for supporting the *Challenger* at the rate of at least £3,000 p.a.,[5] they were also obliged to recognize the larger principle implicit in the national support of science. As the *Spectator* pointedly reminded the Government, the support of science was a natural extension of the State's acknowledged duty to print money, provide standards of weights and measures, build lighthouses, conduct surveys and guard public records and monuments.[6] In 1869 and again in 1871, the Government received requests for £19,000 from the Royal Society and the Royal Astronomical Society for the support of Transit of Venus expeditions.[7] Beginning in 1870, the Government also supported a succession of solar eclipse expeditions in which J.N. Lockyer was to play an important part.[8] Within the civil sphere alone, the State was also helping to house the Kew Observatory, and was maintaining scientific establishments under the Government Chemist, the Royal Military Academy, the Home Office, the Board of Trade, the Board of Agriculture and the Local Government Board.

These events Gladstone viewed with some misgiving. Though highly respectful of individual scientists, Gladstone had little sense of what science, or scientific method, represented.[9] As a member of the famous Metaphysical Society, created in 1869 by James Knowles, to "unite all shades of religious opinion against materialism" he had come into close contact with Huxley, Tyndall, Lubbock, and the "critical, rationalistic and scientific spirit."[10] To him, science had early brought a theological challenge, which he sought time and again to resolve.[11] But in the public sphere, it also brought an administrative and financial challenge, with implications he could not foresee. Accordingly, the safest path seemed to follow the traditional route of Free Trade. As Gladstone announced to the Institution of Civil Engineers annual dinner in 1872:

> A fair field and no favour is the maxim of British administration. A field so fair, so extensive and so promising that all industry may find its place, and such an absence of favour that one as well as another may hope for success. If, under these conditions, the State does nothing for science, it cannot be helped, nor need it be lamented.[12]

But this policy was already flying in the face of economic, industrial and even political realities. The importance of science as a national asset was increasingly seen in the light of growing foreign competition,[13] particularly in the light of the sharp lessons of the Franco-Prussian war. As Lockyer reminded the readers of *Nature,*

> The Prussians, whatever their other qualities, are emphatically a scientific people, and to that predominating characteristic first and foremost, are their recent military triumphs due.[14]

Moreover, science was increasingly seen as the province of a specialist class, with a particular identity and sense of independence. As B.H. Becker wrote in reviewing the scientific circles of London:

> Specialty is indeed one of the salient features of the nineteenth century. Special

> knowledge and special power are everywhere in demand. In law, in the arts, and more especially in science, the specialist is looked upon with a peculiarly favourable eye. Mankind gratefully acknowledges its obligations to the man who devotes his life to the attainment of perfect knowledge within a certain narrow sphere.[15]

The specialist demanded special treatment. As George Gore told the Social Science Association at Plymouth in the autumn of 1872,

> The practice in this country of placing gentlemen comparatively ignorant of science to legislate for science, and to direct scientific men in government offices and public schools, is one of the most effectual that could be devised for retarding the progress of the nation in all those departments which depend upon science. If scientific men are to be legislated for and directed, justice requires that it should be by men possessed of scientific knowledge.[16]

The theme of Gore's address would be respoken a hundred times again, but in the autumn of 1872, its sentiments had a certain piquancy.

II. KEW AND THE OFFICE OF WORKS

The early months of Gladstone's first ministry were not comfortable for the advocates of science and art. Even among scientists with liberal politics the advent of Gladstonian administration dedicated to "retrenchment and reform" brought an air of severe austerity to the fairly easy atmosphere of cooperation that had existed in the 20 years of strong Cabinet government and weak party discipline. The dark angel of retrenchment appeared no more visibly than when it took human form in the person of Acton Smee Ayrton, M.P. for Tower Hamlets.[17] Following an early career in Bombay as an attorney, Ayrton returned to England in 1850 with a moderate fortune and a profound sense of public duty. Although widely regarded as a clever adventurer with no political skill,[18] he first stood as a Radical candidate for Tower Hamlets in 1852 and in 1857 was elected over a liberal candidate with a majority of 1100. With Sir Joshua Walmsley he conducted the Representative Reform Association, one of whose more pleasant projects was to publish a list of members of Parliament who defaulted in attendance. The public soon found that everything he touched turned to news. His habit of speaking "on every subject that came before Parliament", and his unhappy tendency of criticising his own front bench won him distinction as "the greatest bore in the the House" *The Times* periodically published articles[20] entitled "Mr. Ayrton and his Constituents", which distilled the substance of "noisy radicalism", strong opposition to military and naval expenditure and flogging, and protests against such "centralising measures" as Savings Banks, public health and burials legislation. In the early 1860's, he further disgraced himself and brought upon himself the wrath of John Bright, by referring slightingly to the Queen.

Ayrton's radical zeal as a catalyst in Opposition won him a wide reputation. In 1866, he took up cudgels against monopolistic privileges enjoyed by the City of London, and had himself placed on successive Select Committees investigating gasworks, taxation, education and poor law administration in the metropolis. Whatever appeared to be "excessive" expenditure or privileged status won his scorn–in creating public lighting provision, he caustically remarked, the City had for the first time passed "from gastronomy to gas".[21] As a defendant of individual freedom and the working man, he won the reluctant admiration of the House. The Contagious Diseases Bill he denounced as a "Bill for keeping public women at the public expense for the gratification of our soldiers and sailors;" compulsory vaccination he denounced for not taking "sufficient care to guard the public from the dangers likely to arise" from medical techniques. The Charity Commission he dismissed as a body "instituted for the purpose of ministering the follies of people who in former times left money for charity..."; on Government pensioners he observed, "Some gentleman who retired from the civil service in consequence of ill health enjoyed remarkable longevity."[22] The same year, he served as chairman of two committees on the Municipal Corporations Bill, and also became, through his own experience, an informed opposition spokesman on revenue and law in India.[23]

By the late 1860s, Ayrton's radicalism had won a certain notoriety that was useful to the Liberals. At times when his enthusiasm exceeded his discretion, as once on the Royal Parks Estimates,[24] Gladstone supported him, and scored successive political hits for the Opposition. During 1867 and 1868, Ayrton particularly distinguished himself in onslaughts against provision for the new Natural History Museum, for building the New Law Courts and for redecorating Burlington House. This was the age of massive building, which transformed Whitehall and much of central "official" London with huge, stately classical and Gothic construction. Ayrton was at pains to criticise every step towards what seemed architectural extravagance, and public waste.

While Ayrton developed his pugnacious skills at public criticism, he also moved closer to the Liberal Front Bench. In 1867, he stood in Gladstone's place at a Crystal Palace Reform fete,[25] and fought by Gladstone's side as the Reform Bill moved through Parliament. It was not surprising that, when Gladstone came to power in 1868, Ayrton was appointed a junior minister, as parliamentary secretary to the Treasury. Unhappily Ayrton soon fell out with the brilliant Chancellor of the Exchequer, Robert Lowe.[26] In particular, Lowe found himself having to restrain Ayrton's special enthusiasm for quashing expensive government artistic and architectural projects. Thus, Alfred Stevens, the rather slow and fastidious sculptor of Wellington's memorial in St. Paul's, was chronically harassed by Ayrton until rescued by Lowe.[27] In 1869, after only 10 months at the Treasury, Ayrton was quietly transferred to the Office of Works.

The post of First Commissioner of Works was descended from the high Crown office of the Surveyor General of Works, begun under James I in 1615 and occupied at various times by Inigo Jones, Sir Christopher Wren, Sir William Chambers and Robert Adam. For centuries, the office held responsibility for deciding upon plans for public buildings, usually without close scrutiny from Treasury clerks. However, Parliamentary steps were taken towards reform and control between 1814-1832, and in 1851 a Board of Commissioners of Woods, Forests, Land Revenues, Works and Buildings was created. This body was divided into a new Office of Woods and Forests and an Office of Works, both held under increasingly searching ministerial and Treasury control. The title of Surveyor General disappeared and the First Commissioner appeared in its place.[28]

From its earliest days the Office of Works had witnessed a close association between political expediency and artistic merits. In the change of Government of 1868, Lord John Manners was replaced by Sir Henry Layard, a famous archeologist and antiquary, who was described by the *Saturday Review*, with some historical hyperbole, as the first "expert" to hold the post.[29] In office only a year, Layard had fostered an easy and sympathetic relationship between the Liberal Government, the Board, its architectural advisers, and the public. But in so doing he soon fell afoul of Ayrton's wrath.

The circumstances were almost trivial. In February 1869, for example, Layard annoyed Ayrton by asking the Society of Antiquaries to prepare a list of historical tombs and monuments worthy of Government custody or protection; on the basis of this report a "Sepulchral Monuments Committee" was appointed. But real opposition from Ayrton arose in June 1869, when, with the support of Lowe and Trevelyan, Layard proposed that the new Law Courts should be built on the Embankment, rather than on Carey Street where the view was less pleasant, approaches more expensive, and traffic more dense. After intense opposition, Layard lost his case. The last straw broke in late 1869, when Ayrton criticised Layard for allowing Barry, the architect, to use Italian marble in the mosaics he had planned for the Houses of Parliament. After another bitter conflict, Layard's position became impossible. At Lowe's suggestion, Layard became H.M. Ambassador to Madrid and Ayrton was transferred to the Office of Works in his place. Layard bitterly grieved that his adversary was so quickly appointed his successor.[30] But his move at least silenced Ayrton's constant queries. *Punch* rejoiced at Ayrton's apparent promotion, but effective demotion:

> From the Works I see a blessing if Layard is set free,
> It will make a road for shunting Ayrton from the Treasurie.[31]

Ayrton found the Office of Works a difficult assignment. Indeed, his replies in the Commons, now phrased in the defensive, were remarkably timid. Yet, Ayrton held true to his principles of public accountability. He lost no time in informing his constituents that he held in contempt the "whole race of architects, sculptors and gardeners." Disavowing Layard's promises, rejecting his architectural plans and refusing to consider a Bill for ancient monuments, he put paid to progress in the field of preservation.[32] Soon it was common knowledge that "Every architect, artist and man of science who has had business to transact with this important department has his own grievance to complain of, his own story to tell of the insults he has received from "Ayrton the Arrogant."[33] His zeal for retrenchment cost him support from the very first. Artists and architects, particularly those concerned with public works, found his economies incorrigible. The *Illustrated London News* as early as November, 1869 decided that Ayrton would never "be anything except a check upon anybody who wants to make London handsomer", and demanded action:

> It is not the business of the Chief Commissioner of Works to do nothing but repress expenditure. On the contrary, it is his business to encourage and direct improvements and adornments. To hear some people talk one would think that the greatest and richest nation in the world had been obliged to put its affairs into the hands of accountants and, until a dividend could be declared, hoped to propitiate the world by severe retrenchment and strict economy.[34]

The plea fell on rocky ground. Gladstone's ministry had committed itself to economic reform, and Ayrton was merely doing the job that he had been appointed to do. His undoing was to come, however, not at the hands of the architects, but in the gardens of Kew.

Ayrton's post as First Commissioner of Works gave him jurisdiction over the Royal Gardens at Kew, and so over the botanical empire of Joseph Hooker, F.R.S. For thirty years the gardens and botanical collections of Kew had enjoyed a worldwide reputation. With specimens gathered for commercial analysis or classification from all corners of the world, it had created a superb reference library for botanical research. In 1861, it had helped introduce cinchona (quinine) from South America to India and Ceylon. In the 1870's, Kew was instrumental in transferring rubber seeds from South America to the plantations of the East Indies. Through its work with foreign and colonial ministries on the selection of officers, and through its advice to the Admiralty, the India Office and the War Office on the control of useful plants, Kew acquired vast imperial importance.

The work of building Kew, however, had been chiefly a family enterprise. Fifteen acres of gardens and a fine herbarium, formerly belonging to George III and united with the gardens of Richmond Palace, were transferred by the Crown to the nation in 1837,and in 1841, William (later Sir William) Hooker was made director. A few years later, the pleasure grounds belonging to the King of Hanover were added to form a National Arboretum.[35] In 1851, by an Act of Parliament which separated the Office of Works from the Office of Woods and Forests (14 and 15 Vict., c.42), Kew was transferred to the Office of Works. In 1855, Joseph Hooker was appointed assistant to his father, then aged 70, at a salary of £ 400. In 1858, he was given a house and £ 500, and in 1865, on the death of his father, he succeeded to the Directorship with a salary of £ 800 and an assistant of his own. Although Hooker was nominally a Civil Servant, and the gardens property of the State, Kew was still a part of the Hooker family history. To impugn the Hookers motives would have been normally unthinkable.

It was not, however, unthinkable to Ayrton. Ayrton's staunch radicalism bore little sympathy for Hooker's imperial connections and exotic tastes. Indeed. Kew stood like a semi-autonomous satrapy within his first ministerial domain, and loomed a silent challenge to his authority. Besides, the maintenance of a huge park which, despite the protestations of scientists, seemed chiefly recreational, made a mockery of the Government's policy of retrenchment. As his behaviour in Parliament had repeatedly demonstrated, Ayrton regarded "all outlay, however necessary, as extravagant and all extravagance however unwilling, as corrupt."[36] Kew was not expensive; it represented only £ 12,000 p.a. of the annual Works Vote of £ 1,200,000, or only 1%. But symbolically, Kew seemed an expensive garden of "jobbery", of little use to the common man. Like military expenditure or the City of London, it was a large target, and seemed easy to hit. In his attacks, Ayrton had the early blessing of Gladstone, chiefly on grounds of finance, but also on grounds of scientific scepticism. Gladstone had early won the mistrust of many men of science particularly those who, like Tyndall and Huxley, were solid advocates of Darwinism. Only towards Richard Owen, the comparative

To qualify, first lose all self-respect—
Be vain, a bully, braggart, and a dunce!

JACK IN OFFICE.

anatomist who had suffered bitterly at Huxley's hands at the famous Oxford meeting of the British Association in 1860, did Gladstone's friendship tend. It was easy for a *Nature* leader in 1872 to criticize Gladstone's leadership as one of "Mental Darkness in High Places".[37]

Against this background, between November 1869 and September 1872, unfolded an agonizing story of political maneuvers, misunderstanding, and protest which resounded through the national press and the community of Victorian science.

III. THE TROUBLES AT KEW

Shortly after his arrival at the Office of Works, Ayrton tactlessly sent Dr. Hooker a reprimand, for which Ayrton later conceded his apologies, on the basis of a cursory and inaccurate reading of Kew's accounts. This was bad enough, but Ayrton did not stay his sword. Soon he interfered, for example, with the appointment of a clerk at Kew by supporting a man who had technically won the post by open competition in the general civil service examinations, but who was unqualified to do the specialised work required. In March 1869, Ayrton refused to support Hooker's application for expenses to visit St. Petersburg on botanical work and he ended having to pay his own way.[38] By December 1870, the situation had further deteriorated. Going over the head of his nominal vassal, Ayrton called on Robert Smith, the Curator of Kew and Hooker's subordinate. Ayrton offered Smith a better job as Surveyor of Parks, and an assignment in Hyde Park which would take him away from Kew for months. The Curator was asked not to reveal this to Hooker, but Hooker discovered the truth, and protested strongly. To make matters worse, Ayrton, without asking Hooker's advice, then submitted a memorandum to the Treasury, outlining estimates for a new staircase at the Kew museum, despite the fact that Hooker had already set a plan before the Office which would do a better job at half the cost.[39] Although furious at the insult, Hooker contented himself with preparing pamphlets on the botanical parks and gardens of England, France and America for his chief's guidance, to enable him to "judge more generously of the requirements and duties of some of the offices of the department he controls."[40]

The next critical confrontation came with the construction of new plant houses – ostensibly a simple administrative matter, but one at the heart of Hooker's botanical research. In early 1870, Ayrton gained Government approval for administrative rearrangements in the Office of Works.[41] Having regard to the growing expense of new public buildings, and "especially to the need of an administrative officer to superintend the subordinate officers in the Department engaged upon Works and Buildings,"[42] the Treasury agreed to appoint one officer (but not an architect) for the job. A new post of Director of Public Works was created, and charged with preparing all estimates and supervising all new construction carried out under the auspices of the Government. The appointment of Douglas Galton, an experienced engineer and inspector,[43] was intended to end the amateur regime of architectural consultants, under which the Barrys, among others, had flourished.

Galton was not an enemy of science; on the contrary, he was an FRS (1859), and was soon to be elected one of the joint General Secretaries of the British Association. But Galton's appointment did give him rights of approval over all works at Kew. Under Lord Derby's government, Hooker had enjoyed full authority in the scientific affairs of Kew, and had been responsible directly to Lord John Manners. Accordingly, when Hooker wanted new building done, he ordered it done, following his normal practice, through his customary contractors, Messrs. Ormson of London. But Galton disagreed. To avoid any imputation of favouritism or wastefulness, Galton wished the contract to be advertised publicly and awarded competitively. Hooker learned of Galton's policy accidently, through his curator, and when he queried its accuracy, was respectfully advised to raise the question, if he so wished, with the First Commissioner.[44]

What thus appeared to be a relatively minor routine misunderstanding quickly grew out of hand. On the one hand, Hooker quite reasonably believed that at Kew "as in all other Horticultural Establishments...all responsibilities connected with the Heating Apparatus should rest with those who grow the plants heated thereby."[45] Second, and more significant, Hooker's relationship with Kew had always been more than merely official. Kew was both the legacy of his father, and the trust of the nation. Hooker's heritage had meant independence from administrative control. His attitude towards the Gardens was much more personal than any administrator could easily fathom. Official correspondence obscured the fact that the Commissioner's reformist intentions were construed as a threat to the status of the Hooker family, and to the intellectual aristocracy of English science.

This apparently simple grievance, with such complex undertones, Ayrton speedily mishandled. In reply to Hooker's demand to know why the Government had changed the policy of its Conservative predecessors, Ayrton said only that on "grounds of efficiency and economy", all future Works would be requisitioned through the Office, and sanctioned only after examination by the Director of Public Works.[46]

Hooker regarded these arbitrary instructions as no explanation at all. On 19 August 1871, he reminded the Board that this was not the first time he had been slighted, and his authority questioned. "I cannot avoid recognising", he wrote, "a further indication of that disregard of the Director's office or want of confidence in myself of which I have had such conspicuous proof since – and only since – the accession of the present First Commissioner."[47] To Lord John Russell, "founder" of Kew and the man most responsible for appointing the Hookers, both father and son, to the Directorship, he complained that Ayrton had tried to intrigue with his subordinates, to supersede him in important decisions and to embarrass him by making contrary proposals to the Treasury.[48] Without further delay, Hooker claimed the privilege of taking the matter to the Prime Minister.[49]

> After upwards of 32 years spent in the public service, at home and abroad [Hooker wrote], without a suspicion of mistrust...I have had, since Mr. Ayrton's accession, to submit to various arbitrary measures which, though compromising my position and authority, have been concealed from myself and become known to my subordinates, through whom alone I have been made cognisant of them.[50]

Hooker and Gladstone had rarely met, but there were mutual feelings of respect between them. Gladstone, when Chancellor under Palmerston in 1865, had recognised Hooker's scientific reputation, and acknowledged his contributions to English botany. While in no sense well disposed towards new biological discovery himself, Gladstone nonetheless knew the force of scientific ideas on the public temperament. Hooker's letter, lodging an official appeal from a natural scientist against an official superior, was unprecedented, and for that reason had to be carefully weighed. Gladstone's secretary assured Hooker that the matter would be studied, but Hooker's rage could not be easily cooled.

Finding no satisfaction at the Board of Works, Hooker on August 31 wrote directly to Ayrton and listed his grievances, dating from the infamous incident with his Curator the year before. Surely he, as Director, should have authority to determine a scientific question, especially when there were divergent views about the relative merits of different schemes. The same day, Hooker wrote again to Gladstone, enclosing a copy of his letter to Ayrton, and asserting his right to independence. To thus make a scientist subject to the opinions of laymen on apparatus was "as obviously wrong in principle, as to refuse a surgeon his choice of Instruments and Hospital Appliances."[51]

During the next months Gladstone appears to have discussed the matter with Ayrton, and on the basis of Ayrton's explanation, decided to support his minister. Gladstone replied to Hooker, advising him that it was an internal departmental matter.[52] Hooker, inflamed at the thought of misrepresentation and compromise, went directly to Algernon West, Gladstone's secretary and Hooker's personal friend. This letter of 30 October put the matter squarely:

> I am at a loss what to say as to my future position under a Minister whom I accuse of evasion, misrepresentation and misstatements in his communications to the First Minister of the Crown, whose conduct to myself I regard as ungracious and offensive, and whose acts I consider to be injurious to the public service, and tending to the subversion of discipline.[53]

Hooker next began to take counsel among his friends in Whitehall, including the Chancellor (Robert Lowe), Lord Cardwell, H.A. Bruce (later Lord Aberdare), then Home Secretary, and the Duke of Argyll, then at the India Office. From the outset, Hooker had the tacit support of the Chancellor. Indeed, although Lowe knew little science, he was likely to defend its advocates. "It seemed to him to have the promise of the future. It was the only knowledge in the world which was both certain and also progressive. Of Charles Darwin he spoke in a strain of respect which he would not have employed towards any living person."[54] But Cardwell, Bruce and Argyll offered Hooker little consolation. Hooker next tried the scientific "establishment". If Kew was in danger of bureaucratic suppression, perhaps Kew could be made independent, subject only to a Board of Visitors, like the Royal Observatory at Greenwich. Sir George Airy, Astronomer Royal, was asked for advice, but his reply was discouraging:

> A Board of Visitors is very desirable, first when the subjects of an institution are so technical that probably neither the Government nor the mass of people understands them; second, when the nature of the apparatus, etc. prohibits the admission of numerous visitors. Both these reasons apply to an Astronomical Observatory: you will judge whether they apply to Kew Gardens.[55]

Airy added that, in any case, his Board of Visitors often took decisions without consulting him. Hooker quickly found this formula quite uncongenial.[56] In the meantime, Hooker appealed to his scientific friends. George Bentham, F.R.S., one of the country's leading botanists, believed that Hooker's control must be maintained, and Sir Henry Holland, Physician to the Queen, agreed. Both, however, opposed the idea of a deputation to the Prime Minister, which might only reduce the question to a personal quarrel. There was growing parliamentary and private evidence that Gladstone was having difficulties with Ayrton in other quarters, and it was felt that Hooker's case would be more convincing if he appeared as "one of the celestial souls that dwell aloft in a serener sphere and merely look down in lofty scorn on the tribe of Ayrtons...". Instead of a deputation, R.S. Ball suggested a strong letter to the Prime Minister stressing Kew's uniqueness– "the only scientific institution in which England confessedly stands unequalled"–and the virtue of preserving a fine family and public tradition. It was not a case of asking Government to do more for science, but merely to "protect from danger and injury a treasure which has been acquired for the benefit and honour of the country." Because England's "public officers are not well organised for dealing with scientific questions and especially with scientific institutions", they would require scientific advice, authoritative and independent:

> Special knowledge is not possessed by those who have to decide the questions that arise. They must either be guided by the scientific head of the given institution, or, if unwilling to place full confidence in him...run the risk of doing irreparable mischief by overruling his suggestions and setting aside his authority without knowing the practical consequences of their own discussions.[58]

In the meantime, Sir Henry Holland learned privately that the Prime Minister wished to defer the question of Hooker's status to a time when the transfer of the British Museum (Natural History) to South Kensington was completed. He would, however, be willing to receive a deputation from Hooker.[59] A month later, Gladstone, preoccupied by other matters, continued to procrastinate. Lowe and his permanent secretary, Ralph Lingen, were willing to have Kew made responsible directly to the Treasury, but, as Sir John Lubbock observed, it was difficult for Gladstone to interfere arbitrarily with his ministers' domains.

Throughout the winter, Ayrton's manner had begun to isolate him from his colleagues and his constituents. In early 1872, Reginald Palgrave reassured Hooker, "crises may come and Ayrton may go."[60] But the suspense went on undiminished. During February 1872, Hooker began to explore new possibilities, and drew up plans for a sweeping reorganisation of Government scientific interests. A "federation" of scientific bodies, all on equal footing, and directly answerable to a Minister, was the solution,

> So long as each such body retained its autonomy, and was governed by a Director individually responsible to a minister, or a department of Government, such a federation would be a very effective one, and able to give valuable aid on all scientific matters to the Government at no cost to it.[61]

The Government declined to consider Hooker's plan, but Lowe offered to take it before Lord Granville, Lord President of the Council, but Granville's preoccupation with foreign affairs ruled out all hope of action. In the meantime, Lingen and Ball advised Hooker to let things rest. In a private letter couched in friendly terms, Lingen admitted he was "disposed (which is easy in other people's affairs) to counsel patience, or at least delay."[62] Reginald Welby, Lingen's young principal, made the same point more formally: "I am afraid...your remonstrations must be made to some higher power."[63]

What "higher power" could be called upon when even the Prime Minister declined to act, was not wholly clear. Short of prayer, the only answer seemed Parliament. By now, the issue was acquiring the force of a *cause célèbre*. Charles Lyell urged Hooker not to let the matter drop. If Ayrton won, it would be "one of the most serious blows aimed at Science in my time."[64] But Hooker did not let it drop. On the contrary, in March 1872, Hooker presented the Cabinet with an eight-point manifesto, demanding that his powers be restored and that some equitable division of responsibilities be made between the Office of Works and the Directorship of Kew.[65] Hooker was becoming desperate. Gladstone seemed bound by ministerial responsibility, and could not act; Lowe was too busy with the Spring Budget, and the Treasury refused to interfere, even on grounds of financial accountability.

On March 15, Gladstone appeared to relent, and through Lord Ripon, officially instructed Arthur Helps, Clerk of the Privy Council, to give Hooker a "final" (but unofficial) answer. This "final answer" read:

> Mr. Ayrton has been informed that Dr. Hooker should, in all respects, be treated as head of the local establishment at Kew–of course, in subordination to the Chief Commissioner of Works.[66]

"...A rupture with Mr. Ayrton is imminent", Hooker wrote Ripon, "his official conduct being altogether intolerable and his instructions of a nature which it is impossible for me to obey."[67]

With official channels obstructed, other avenues opened. Soon were heard the voices of collective scientific dissent. At the end of March John Tyndall, when dining with Lord Derby at the "Club", declaimed loudly against Ayrton to all in earshot, and scarcely a day later Derby offered Hooker his services. Although knowing nothing in detail about the case, Derby ventured the view that "Ayrton's habit of harassing and ill-using his subordinates is well known", and promised to see that "an eminent scientific man, whose position makes it difficult for him to defend himself, should not suffer injustice."[68] On April 2, Tyndall protested anxiously to Derby:

> The real character of Kew is only too likely to escape the superficial observer. The place has been made so beautiful and so attractive to the public that its immense

scientific importance is likely to be overlooked. That it is of the very highest importance to botanical science and to the application of that science in India and the Colonies, might be readily demonstrated.[69]

As to Hooker's qualifications—"a man of exalted natural endowments... cultivated in the very highest degree"—there could be no question. As to the merits of his case, it was clearly a civil duty to "prevent this inversion of equity, this placing of knowledge under the control of ignorance, this subjection of the high and refined nature to the tyranny of an Ayrton."[70]

The effect was electric. "...How delighted I am with Tyndall's letter", Sir Arthur Helps, Clerk to the Privy Council, wrote Hooker: "...It must do good. That fellow is decidely worth having as a friend."[71] Helps was himself interested in science, and "had a great reverence for scientific men".[72] Hooker enthusiastically told Tyndall of his larger plan for science—a scheme for bringing all scientific directors under the Lords of the Committee of Council. "As it is", he argued, "the Lords have no scientific council that has the confidence of the public; and science is jumbled up with Art at South Kensington, with the Parks under the Office of Works, with Literature at the British Museum and so forth". The opportunity should not be missed. "If the troubles at Kew were to eventuate in a better order of things I should never regret my share of them."[73] In return, Tyndall reassured his friend, "Keep your heart up dear fellow. The nation would bear your ordeal if they only knew your case."[74]

IV. THE SCIENTISTS RALLY (April–June 1872)

This, of course, helped not at all. In mid-April 1872, Hooker reluctantly accepted the advice of Huxley, Lubbock and Bentley, and put his case in the care of Parliament. "I must confess that

I feel a little sore at my own defeat as a scientific man on a scientific proposition and greatly regret having to hand my weapon to others; but I *have* fought for the position my father made and left me to defend and shall always be to the fore.[75]

Above all, Hooker, recalling his many happy hours at Hawarden, regretted causing pain to Gladstone, especially as the Prime Minister was in a difficult position. Yet, after six months, he felt he had no choice:

My present attitude is forced upon me; and I should be unfaithful to higher interests than those of personal friendship, were I to shrink from the consequences of that attitude, whatever they may be.[76]

To Tyndall, Hooker was rather more direct:

> Our guns are in position and we must now fight them. But Gladstone will be furious![77]

Lord Derby weighed their plan of attack. It would not do, he felt, to call for papers, which would make it politically impossible for both Hooker and Ayrton to remain in office; if one were forced to leave, the rules of the political fraternity would guard Ayrton. The best plan, Derby concluded, would be another informal approach through Arthur Helps. As Derby feared, however, Ripon and the Cabinet had already closed ranks. Ripon refused to let his discussions, including a meeting held at his own house, bind him to any position. Even Helps temporised. Hooker was left to pace his terrace. As he wrote to Tyndall,

> An apology from Ayrton is an impossibility. It was talked of, or rather hinted at 8 months ago, but I would not listen to it. Quarter-deck apologies are blunders of the gravest description. The superior cannot apologise to the subordinate without bitter hate, nor the subordinate receive the apology but with exceeding scorn. I have no personal feeling towards Ayrton, I have no wish to see him *humiliated* even if that were possible. I want to be placed in future in a better position in regard to the officials who surround me. I want to have the position of Director of Kew recognised as one of authority, trust and responsibility: given a status, in short, that cannot be attacked by an Ayrton. The position of the Director of Kew is no better now than it was when my father took it as a garden of nine acres without Collections, Museums, Herbarium, Library, publications, and an immense correspondence. If he has raised it to a first-class establishment, it is time that the official responsibility of the Director were recognised accordingly.[78]

On the 25 April, the Treasury sent Hooker an official confirmation of Helps's "explanation", which repeated merely that he was recognised as the head of Kew in subordination to the First Commissioner.[79] The letter, signed by a mere clerk, Hooker thought supremely insolent, and was shown round the Athenaeum and the Royal Society. All talks of peace were ended; "there never was such a chance as this for bringing the position of Science under Government into prominence." Derby and Russell took charge of collecting all correspondence for presentation to Parliament, in hopes of forcing Gladstone to refer the matter to the Devonshire Commission for a special Report. "My position is now in the hands of my friends", Hooker wrote Argyll,[80] and the scientific community, led by three fellow members of the famous X-Club, Lubbock, Huxley, and Tyndall, formed Hooker's *corps d'élite.* In sending Hooker's correspondence to Derby, Tyndall observed "The scientific men of England will be unanimous in their support of Hooker; and I think Mr. Gladstone will find them able and determined to state their case in language somewhat plainer than his reply."[81] All the informal devices of the X-Club were employed. William Spottiswoode, mathematical member of the "X" and also the chief printer for the House of Lords, was consulted at every stage. As Huxley told Lowe, Hooker's reputation was such "as to make a wrong done to him an injury and affront to all the men of science in the country, whether in the public service or out of it." Forebodingly, he added:

> They are persons who have means of making themselves heard and it is quite certain that they will spare no pains to get justice done to their friend and colleague.[82]

In an aside to Hooker, Huxley whispered "...I think that our power of making ourselves unpleasant is fully up to the level of my talk and that is something the ministerial mind can appreciate."[83]

Now that battle lines were drawn, Algernon West sent Hooker his regrets, and Lingen told his Treasury staff to stand their ground: '"...the motion must take its course and we must endeavour in the department to do the best we can with the questions forced upon us by Dr. Hooker." Behind the scenes, however, the Treasury and Ayrton were fighting over the terms of the Treasury letter. "Ayrton throws the blame on me", Hooker wrote, and "the Treasury on Ayrton". With the help of Lord Russell and the cooperation of Lingen, the facts of the case and the constitutional position of the Director were formally set out in legal language. Inquiries were made about parliamentary time and the views of the Opposition.

Under Tyndall's hand a memorial was prepared: "They shall know" he vowed, "that men of science can use a sledge hammer."[84] Tyndall used guest dinners to remonstrate against the Government. "I blew off some steam *à propos* of Ayrton (without naming him)", he wrote in early June,

> at the Chemical [Society] dinner on Friday, Lowe was there and when I spoke of the crass ignorance of men in authority (always excepting Lowe) and their doing things before High Heaven that would make the angels weep, Lowe said, "That's Ayrton–I quite agree...." [85]

In May, soon after Hooker gave evidence to the Royal Commission on Scientific Instruction, Ayrton asked Professor Richard Owen, head of the zoological collections at the British Museum, for a critique of Kew. Owen, no friend of Hooker and Huxley since the "Oxford meeting", was in the midst of a twenty year battle to create a British Musuem of Natural History at

South Kensington, to bring both fossil and living collections under a single roof. Anxious about his own zoological empire, Owen was highly defensive about his position at Bloomsbury. His reply to Ayrton's letter was therefore not calculated to produce an objective view of scientific needs. In fact, it went to some lengths to accuse Hooker of using his official post to advance his own reputation; of competing with the botanical section of the British Museum; of overpaying staff; of neglecting the great acacia trees in Kew Gardens; of failing to have rock works and garden sculptures in the grounds; and of keeping an expensive herbarium for "attaching barbarous binomials to dried foreign weeds."[86]

V. KEW IN THE PUBLIC EYE (June-July, 1872)

By early June, the memorial, toned down by Lubbock and Huxley and vetted by Huxley's administrative "chief", Col. (later Sir John) Donnelly of the Science and Art Department, was ready for the press. Proudly sending it to Derby, Tyndall claimed, "If we cared we could get the Science of England [to sign] it."[87] Eleven signatures appeared, including four members of the X-Club.[88] One significant hand–Sir George Airy–was noticeably absent. Airy, alone among scientists approached, refused to sign. Airy had recently accepted a K.C.B.–an honour which Hooker had dismissed as "A Gladstonian sop to science"[89]–whilst Hooker had declined the same honour, and there was a certain estrangement between the two men. But Airy felt there were also important differences in principle. In controversies of this sort he felt there was no place for "outsiders":

> ... It happened to me once to think myself aggrieved by my immediate superiors (the Admiralty) and I did thereon appeal to the Treasury. But it never occurred to me that under any circumstances whatever could any outsiders put in a word between us. I did not even ask my own Board of Visitors to interfere.[90]

If Airy represented the "orthodox view" of the "Civil Scientist", Hooker was fighting for the recognition of fundamental research as a national resource. As *Nature* thundered against Airy, in a different context:

> "it is certainly a little disheartening to find a great leader in science insisting so much on direct utilitarianism as the sole basis of national science, and withholding his testimony to the enormous moral and intellectual benefits of philosophical research ..."[91]

As Lyell ominously warned Tyndall, the memorial "may well help to turn out a Ministry."[92]

Delane of *The Times,* the editor of the *Daily News,* and the *Pall Mall Gazette* were all well known to Tyndall, and agreed to give the memorial publicity. By Monday, July 8, the memorial had been "leaked" to the daily press, and on Thursday (July 11), it was printed, for the record, in eleven columns of *Nature.* For over a month, between July 8 and August 10, the press reaction was enormous.[93] When The *Daily News* reviewed the memorial, it rebuked the electors of Tower Hamlets for having chosen a member who "had turned that admirable national establishment at Kew from a botanic into a bear garden." The Conservative *Standard* took political advantage from the Prime Minister's folly, while the *Morning Post* insisted that Gladstone should waste no time at all in restoring Hooker's status, lest his services be lost to the nation. The *Pall Mall Gazette* gave the scientists its full support and demanded a full parliamentary investigation. *The Economist* pleaded with the Prime Minister to teach his ministers the importance of delegating duties to experts.[94] The "established" *Civil Service Gazette* (July 13) surmised "It might be taken for granted that, even if chapter and verse had not been given, that the eminent men who signed the protest which has now been made public would not have adopted that extraordinary and unusual course had not the circumstances imperatively demanded it." Even *The Times* (July 8, 1872) stood back in wonder–

> We can only ask, as this Government has given us repeated occasion to ask, what is the use or need of provoking all this animosity? Even if a little more money is spent at Kew than a rigid economist would justify, the nation gets full value for the expenditure....It is not, after all, the money bestowed on such establishments...which burdens the country and perplexes the Chancellor of the Exchequer."

In the meantime, certain conflicts of loyalty were appearing. Lubbock, mindful of his place in the party,[95] privately sent an advance copy of the memorial to Gladstone, and carefully reworded the motion before Parliament from "Correspondence with the Board of Works" to "Correspondence regarding changes to be made in Kew Gardens" to avoid additional embarrassment to the Government. But his caution was perhaps unnecessary. The front bench was speedily coming to Hooker's side. As H.A. Bruce remarked on seeing the memorial:

> I don't think that he [Gladstone] could have known–I am sure I did not–how much the country owes to you and your father.[96]

At the Treasury, Lingen made a last minute attempt to avert parliamentary debate by a conciliatory letter to the Board of Works,[97] answering four letters of Hooker, but the situation was beyond recovery. On June 24, Lubbock moved in the Commons for the papers to be printed. The Government tried again to delay proceedings, but all to no avail. "Huxley is perfectly disgusted with Lowe", Hooker told Tyndall, "and altogether Science under Government is in a pretty mess."[98] Gladstone, bound by collective ministerial responsibility, tried to keep the matter out of question time and even visited Hooker at Kew,[99] in hopes of reaching some amicable settlement.

Norman Lockyer reported to Huxley that Gladstone had garbled the account of Ayrton's explanations. Hooker was dumb founded. Moreover, through the pages of *Nature* came an attack on Hooker by Huxley's old enemy, Sir Richard Owen, who persuaded Ayrton that Hooker was trying to have the British Museum botanical collections moved to Kew. It now appeared that Ayrton, acting on Owen's advice, was trying to disestablish Kew by "worrying" Hooker out in order to transfer his collections to Owen's empire.

By mid-July, annoyed at the Government's delay in producing the correspondence, Derby decided to move for a discussion in the Lords. Huxley, dismayed by Lubbock's indecision, was also anxious to get on. "We have everyone with us", he declared,[100] and surely it seemed so. James Bateman, F.R.S., conveyed to Hooker the support of the country's leading botanists and agriculturists.[101] Resolutions of support also came from the Zoological Society and the Royal Horticultural Society, led by Hooker's son-in-law (and successor) William Thiselton-Dyer. The press alerted Scottish scientists, who shared Sir Robert Christison's sorrow at this "dash of insanity"–

> the folly, rashness and ingratitude of the man, who would dare to assault at its very heart an establishment, which, almost alone in all England receives universal applause in all quarters....[102]

The Royal Society of Edinburgh spontaneously sent Gladstone a resolution signed by Christison (as President); P.G. Tait, the physicist; Wyville Thomson, the oceanographer; A.C. Crum-Brown, the chemist; Archibald Geikie geologist; William Turner, the anatomist, and George Buchanan, the meteorologist. George Chalmers, of the West India Committee, wrote Gladstone that Kew was so important to his planters and merchants that Hooker's resignation would be a "national misfortune".[103] Letters to the press also came from abroad. A former Harvard professor wrote the *Standard* that "Mr. Ayrton would not be missed outside his own party. Dr. Hooker belongs not only to England but to the whole civilized globe."[104] Letters of sympathy came even from Edward Barry of the National Gallery, who had also suffered at Ayrton's hands.[105]

There were few attempts to counter this growing wave of sympathy. As Hooker said, the papers continued "to make a prodigious clatter."[106] The *Telegraph* made some kind remarks about Ayrton; a clerk at the Stationery Office tried to explain that the confusion over the publication of the *Flora of Tropical Africa* was not Ayrton's fault, and that "even the Devil should have his due."[107] But the only serious support of Ayrton came from men whom Hooker had sacked, or who had grudges against him.[108] The lesson was clearly before the press: "A prudent official", wrote the *St. James's Magazine* (July 19, 1872), "would almost as soon put his finger into a hornet's nest as treat a scientific man with contumely". Scientists were "not a patient generation, they are conscious of enjoying high general estimation, they know their own importance and they certainly have the merit of standing by one another."

When Lubbock moved for a Commons debate in the week of July 21, Ayrton dug in his heels, assured everyone that he would not go without a fight and prepared a memorandum for the Treasury criticizing Hooker for injudicious management, and excessive expenditure. Ayrton reinforced his anti-scientific stand in July, when he objected to a set of Sir Frederick Abel's guncotton experiments in Downing Street which unexpectedly smashed neighbouring windows. "There was only one person rejoiced and that was the triumphant Ayrton."[109]

VI. KEW MOVES TO PARLIAMENT (July-August, 1872)

The Prime Minister, for practical reasons, was unwilling to let the matter slide into a debate. It was rumoured in *Nature*[110] that if the debate were to occur, "the Government, if they had ventured to support Mr. Ayrton, would have been beaten." On July 24, the Treasury addressed a Minute to Ayrton which tried to oil the differences between the First Commissioner and Kew.[111] This Minute, soon reprinted in *Nature,* defined Hooker's position as head of the botanical division, asserted that "no alterations...in the scientific branch of the department should be made without the Director's concurrence", and agreed that "in various cases Dr. Hooker should have thought that he had just cause of complaint..."[112] The Treasury note was, in effect, a rebuke to Ayrton. Five days later, Lubbock asked Hooker to be guided by its attempt at an *amende honorable*. Gladstone assured Lubbock that Ayrton meant no offence, and had expressed the hope that Hooker could be persuaded to withdraw his charges.[113]

On July 26, the correspondence was finally presented in print to

Parliament as a Blue Book of 77 expensive folio pages.[114] It was a medley of letters, at once over-abundant and incomplete. Much trivia was included, but all correspondence with Gladstone himself was absent. The Blue Book was reviewed at length in *The Times,* the *Daily News,* and the *Daily Telegraph.* The scientists were pleased at their ability to raise a storm. "How well the *Times* has behaved", Tyndall wrote from Switzerland, "That last article was capital and I have thanked the editor for it."[115] The *Telegraph* (July 29) hope that a little *gentillesse* would end the dispute and good manners prevail.

Lord Derby congratulated Tyndall on Hooker's success. The published correspondence and the Treasury Minute, though written "in the guarded language familiar to official men",[116] were to be taken as signs of victory. On Monday, July 29, Derby spoke on the subject in the House of Lords. Before a crowded gallery, Derby reviewed the case again, this time in the hearing of Ayrton himself, who stood at the bar of the House.[117] In the event, Lubbock, quietly faithful to Gladstone, asked to withdraw his motion. A settlement seemed near.

Unfortunately, the Government was not free of troubles. Hooker was displeased by the Treasury Minute because it suggested that *botanical* responsibilities could be separated from *horticultural* ones, and that the latter could be placed under Ayrton. Moreover, the printed correspondence included a memorandum by Richard Owen, written at Ayrton's request, which claimed that Hooker had recently given evidence to the Devonshire Commission, suggesting that he might usurp the biological work of the British Museum, which Owen was then trying to move to new quarters at South Kensington. Because Owen's memorandum appeared before Hooker had seen it or had time to reply to its charges, Hooker again angrily protested, and again hopes of a settlement faded. Hooker told Lubbock that he could not withdraw his charges unless Parliament were told that Owen's statements were false, and unless Gladstone sanctioned a new role for Kew's director. Under the circumstances, Lubbock again tried to mollify Hooker's anger, and assured Gladstone that Hooker would withdraw his imputations if Ayrton would only accept the Treasury Minute.[118]

In Hooker's view, there had been but three possible solutions—Ayrton's resignation, his own, or the separation of Kew from the Board of Works. Paradoxically none of these happened. Instead, on Thursday,August 8, only two days before the end of the session, the House of Commons endured what Reginald Palgrave, then Speaker, called "the pain of a horrid discussion".[119] All attempts to stop a debate in the Commons were ended by Henry Fawcett (M.P. for Brighton), who independently brought the subject up as a Parliamentary Question. Ayrton had heard the case against him in the Lords,

A PARTING WORD.

Dr. Bull:—"YOU'RE MORE TROUBLE THAN ALL THE REST OF THE BOYS PUT TOGETHER—WITH YOUR BULLYING AND STUPIDITY;—I'VE A GREAT MIND TO EXPEL YOU! MIND YOU BEHAVE BETTER AFTER THE HOLIDAYS, OR I WILL!"

and caught the scientists' lobby unprepared. For them, the ensuing debate which filled 48 columns in Hansard, was a fiasco. Compelled by Fawcett's Question, Lubbock reluctantly recounted the case to the notice of the House. His twenty-minute speech was seized upon by the Liberals, led by R.B. Osborne (Waterford City), McLaren, and Gladstone himself. Ayrton assumed the role of the injured party,[120] and accused Lubbock of attacking him "without proper notice". Claiming that Hooker had misrepresented his actions, Ayrton admitted he took Owen's advice, as a "disinterested opinion". More important, Ayrton accused the scientists of working behind his back, of agitating without consultation for a new department, and of placing themselves above the law. With sweeping rhetoric, which won much sympathy in the House, Ayrton concluded that:

> "It would be a principle fatal to the administration of the public service if you were to allow it to be proclaimed that there is any one person who occupies such a position that he is entitled to dictate to his official superior, who is invested with the discharge of public duties, or to the Government, the course which they are to pursue." [121]

Gladstone rose when Ayrton finished, and assured the Commons that Ayrton had been innocent of evasion, and guiltless of any intention to give Hooker offence. He took quick political capital from observing that none of Hooker's advocates (save Lubbock) were present, but closed with a plea to "bury in forgetfulness the recollection of these differences." Hooker's distress, Gladstone concluded, had been caused by his own extreme sensitivity: ... "scientific men, as they are called by the exclusive appropriation of a title which I must protest against, have a great susceptibility. It is natural that it should be so. But independent of that, those who are not accustomed to enter into our sturdy conflicts take reproof in a much more serious manner than we who are hardened by long use are accustomed to do."[122]

Ayrton had presented himself, in the words of the *Saturday Review* (August 10), as the "helpless victim of a scientific tyrant." He may have displayed what Huxley called a "characteristic mixture of shiftiness and brutality"[123], but Ayrton had vindicated his role as a minister. Moreover, he had once again the support of Gladstone, who supported Ayrton, it seemed, with more energy than seemed necessary. The tables were turned: Hooker was asked to withdraw his statement or resign.

Derby immediately regretted that the scientists had not been content with the half-apology from the Treasury. *The Times* (August 9) criticised the Government for not allowing sufficient time for an adequate debate after such a long delay, but it also urged that Hooker apologise for his indiscretion and Ayrton repent of his vehemence.[124] The *Spectator* of August 10 felt that Hooker was left to bear unjustly the whole weight of the crowd's displeasure. Two days later, Hooker wrote to Ayrton volunteering to withdraw any imputations of a personal character, but this Ayrton now felt was not enough. The authority of the First Commissioner was still at stake.[125] Ayrton, glorying in his vindication, demanded that Hooker retract not only all personal remarks but also all official criticisms he had made of the First Commissioner.

Hooker's friends advised strongly against a categorical retraction which would take the whole issue back to square one. When Ayrton repeated his demands, on August 20, and 21, the Prime Minister wrote personally to Hooker, for the first time in the controversy, and agreed to talk with Ayrton. Derby wondered why Gladstone had left the whole issue so long. "Hasty and violent he may be, but love of justice is his strong point."[126] But Gladstone hung back, and refused to commit himself to altering Hooker's post or status. When Hooker again drew up a list of grievances and offered to submit it to arbitration, Gladstone declined. Ayrton crowed with triumph. "Your several letters", he wrote Hooker on September 4,

> have relieved me of the necessity of taking any further notice of your letter of 30 October last to Mr. West which would appear to have resulted from your failing to appreciate the facts and to write about them in appropriate language.
>
> Had you been guided by the better judgment of Mr. Gladstone, and of that to whom he referred you on the subject...you would have been saved from writing the letter to question which is now shown to have been groundless.
>
> It is desirable that in future all communications respecting Kew Gardens should be carried on between the Board of Works in which the powers and duties of administering them are vested by law and yourself as its subordinate officer in the manner which has been explained to you by the Secretary of the Board.[127]

The scientists' arrows failed against the shield of ministerial responsibility.

By mid-September, *Nature* expressed the hope that it had seen the last of the Ayrton-Hooker dispute. Indeed, the political dust settled quickly into quiet hostility. The settlement, in fact, was no more than an armistice. No apology was ever made by Ayrton, and no retraction by Hooker. The hostile camps merely moved apart, and viewed each other with cool contempt.

The debate had of course, gone on too long. The contestants wearily dispersed. Hooker retired from the field in nervous exhaustion. On September 28, Tyndall left for his lecture tour in the United States, and Huxley turned back to his research. But Ayrton's victory had a Pyrrhic outcome, not least because the scientists' memory was likely to be much longer than the Government's political lifetime. In November, 1873, just eleven months after the debate reached its denouement, Hooker was elected to the Presidency of the Royal Society, and his views assumed an importance no government could ignore. In the meantime, Gladstone took steps to ensure that the "Ayrton incident" would not reoccur. In August 1873, using a Cabinet reshuffle to propitiate public opinion outraged by the great Post Office scandal,[128] Gladstone "promoted" Ayrton to the office of Judge Advocate-General, where his personality could do less damage to the Government's reputation.[129] At the same time, Gladstone appointed Lyon Playfair, the leading parliamentary "statesman of science", to the office of Postmaster General for what became the closing six months of his administration.[130]

Gladstone transferred Ayrton without regret, but with a strict sense of absolution: he later wrote that if he had had "the same task to encounter in the case of a few other members of the Cabinet, his office would become intolerable":

> But before a public servant of this class can properly be dismissed, there must be not only a sufficient case against him, but a case of which the sufficiency can be made intelligible and palpable to the world. Some of his faults are very serious, yet he is as towards the nation an upright, assiduous and able functionary.[131]

At the cost of much public sympathy, Gladstone had done his duty by his Minister.

CONCLUSION

On the face of it, no one had won a clear victory. Hooker's job was spared, and Ayrton had miraculously survived. The bouleversement of August had come as a complete surprise, and "a well-planned campaign had turned out a fiasco".[132] Appearing wise after the event, Huxley agreed that "So much depends in this matter, not only upon what is done, but how it is done, that unless you can see an idea carried out it is dangerous to suggest it."[133] From the Ayrton incident, there were valuable lessons to be drawn. In the later judgment of his contemporaries, Hooker should have remained quiet, rather than moving eccentrically upon a doubtful interpretation of Gladstone's ambiguous statements, and letting Tyndall's tempestuous style govern his actions. Sir Algernon West recalled, with pardonable hindsight, that "Ayrton had an evil tongue, but I confess that I thought him the more reasonable man of the two."[134] Allowing that moderation is rare among missionaries, the scientists' outrage nevertheless seemed naive to experienced parliamentarians.

Within the scientific circles of London, the debate continued to glow long after August. In January 1873, Huxley reported that "the tail of the Ayrton-Hooker storm is drifting over the scientific sky in the shape of fresh attacks by Owen .... Hooker answered the last angelically and I hope they are at an end."[135] But the attacks were not over. Hooker remained Director of Kew until 1888, but relations with Owen were strained until the natural history collections of the British Museum were safely ensconced in South Kensington in 1881, and until Owen retired in 1883.[136]

In an administrative sense the incident was important for three reasons. First, it marked one stage in the transformation of the amateur natural scientist into the professional "scientific civil servant", subject as any other civil servant to the rules of central departmental authority. From 1870 onwards, this process began to raise fundamental questions about the accountability of scientists, their role in Government departments, and their wish for independent access to Parliament—a process which ultimately led after 1918 to the "Haldane principle," and to scientific Research Councils, answerable to Parliament through the privileged route of the Lord President of the Privy Council. On Douglas Galton's retirement from the office of Director of Public Works and Buildings in August 1873, the post of Director was abolished, and the Office of Works was reorganized to permit closer contact between administrators and professional men.[137] But the question of departmental control versus scientific autonomy, already gaining wide recognition, would reappear in different guise time and again throughout the next hundred years, reaching its most recent climax in the public debate surrounding the Rothschild Report.[138]

Politically, the Ayrton incident soured relations between men of science and Gladstone, and contributed to the growing sense of disillusionment with Liberalism which was noticeable among many leading men of science during the last quarter of the century. This was in effect one of many occasions when opinion would be divided by Gladstone's policies; the debacle of Gordon at Khartoun in 1885 and the "iniquitous" Home Rule Bills of the 1890's[139] would be others. Huxley and Tyndall eventually rejected Gladstone as an intellectual opportunist, and compared him to "one of those spotted dogs who runs on in front, but is always turning round to see whether the carriage is coming."[140]

In a direct political sense, the incident served to drive a further nail into the coffin of Gladstone's embattled and divided Government. Nonconformist opposition to Forster's Education Act, widespread disenchantment with Liberal foreign policy, and a public weary of "dull administrative reform" and interminable attempts at "Irish experimental legislation", all reacted together in the political alembic, driving the Liberals towards defeat. Ayrton merely added a catalyst, hastening the Government's fall on the Irish Universities Bill in early 1873. Gladstone's "range of exhausted volcanoes", as Disraeli had described them, struggled in office until January 1874, when the General Election returned Disraeli and the Conservatives with a solid majority. In January 1875, Gladstone resigned from the leadership of the Liberal Party, and remained in the background until the Midlothian campaign of 1880. Ayrton was soundly defeated by a Conservative and two other Liberals at Tower Hamlets, and polled less than one-third of the votes he had won in 1870.[141] He retired from politics, and died in 1886.

Acton Ayrton had laboured under many disadvantages of personality and background which were not in themselves representative of any political party or its policies. Moreover, his perilous candour and undoubted concern for public accountability suggested thoughtless virtue rather than calculated aggression or deliberate intrigue. Nevertheless, his lack of grace and his apparent disregard for the traditional freedoms enjoyed by scientific men revealed an indifference which many were willing to identify with Gladstonian policy towards science. In the last analysis, both Hooker and Ayrton, heirs to rival traditions of the "gentleman amateur" and the "iconoclastic Radical", were representative of life-styles that were gradually passing from English public affairs. In the late 1870s, as men of science were rather better treated by Disraeli and even better by Salisbury, the issue of "Mental Darkness in High Places" was momentarily defused. But the taste of political combat was not soon forgotten, either by Hooker, by Huxley and Spottiswoode (who succeeded him as Presidents of the Royal Society)[142], by Tyndall or by the learned community at large. As the *Art Journal* declaimed on July 27, 1872, "Science has spoken as she never spoke before". In the stalemate of 1872, the scientists perhaps lost a political skirmish. But through it they asserted their right to be heard.

FOOTNOTES

*Manuscript materials concerning the Ayrton incident are chiefly to be found in the Hooker collection at Kew (abbreviated in text as "Kew Papers"), and in the Tyndall collection at the Royal Institution. I am indebted to the Librarian of the Royal Botanic Gardens, Kew and to the Ministry of Agriculture, Fisheries and Food for permission to use and cite the Kew Papers, and to the Librarian of the Royal Institution for permission to cite letters from the Tyndall papers. No private papers of Ayrton could be traced. A brief discussion of the incident is given in L. Huxley, *Life and Letters of Sir J.D. Hooker,* (London: John Murray, 1918), Vol. II, ch. XXXV.

1 Created by Norman (later Sir Norman) Lockyer and Alexander Macmillan in

November, 1869. Cf. R.M. MacLeod, "The Genesis of *Nature*", *Nature, 224* (1969), 423-440.

2 Cf. R.M. MacLeod, "Resources of Science in Victorian England: The Endowment of Science Movement, 1868-1900", in Peter Mathias (ed), *Science and Society, 1600-1900,* (Cambridge: 1972), pp. 111-166.

3 The long sequence of events leading to the Natural History Museum are recalled in Professor Richard Owen, *On the Origin and History of the British Museum (NH)* (MSS collection, Owen Library, BM (NH), 1879.

4 CF. E.G. Fishbourne "On the Causes of the Insufficient Stability of HM's late turret ship, *Captain", J. United Services Institution,* XV (1870), 1-57: A. Hawkey, *HMS Captain* (London: 1963).

5 After two years of negotiation, *HMS Challenger* started from Portsmouth in December 1872 and returned to Sheerness after circumnavigating the globe in May 1876. The *Challenger's* voyage resulted in tremendous additions to oceanographic, meterorological, magnetic, geological and zoological knowledge of the Atlantic, Pacific and Polar seas. Its report published in 50 volumes between 1880-1885, cost H.M. Treasury nearly £30,000.

6 "Government and Scientific Investigation", *Spectator, 44* (July 22, 1871), 882.

7 By 1877, government expenditure on transit work reached £39,766, or over £20,000 more than the government estimated. This by no means unique example of a cost "over-run" in Admiralty (and scientific) expeditions was explained as having been due to the fact that more persons were sent out to observe, and for a longer time, than was originally intended. The Treasury also admitted that "the original estimate was necessarily crude." Treasury letter L18,697/76, in T.165/6, "Blue Notes", *Transit of Venus,* 1882, p. 3. For commentary on the expedition, see *Nature, I* (March 24, 1870), 527; (December 18, 1873), and *Nature, 11,* (December 17, 1874), 121-123.

8 *Vide* T. Mary Lockyer and Winifred L. Lockyer (with the assistance of Prof. H. Dingle) *Life and Work of Sir Norman Lockyer* (Macmillan, 1928).

9 When Gladstone visited Darwin at Downe in 1877 he expressed himself in terms of deep respect and offered Darwin a place among the Trustees of the British Museum in 1881. (J. Morley, *The Life of W.E. Gladstone,* (London: 1906) vol. II, p. 145). But Gladstone's suspicion of science was well known. He believed that "scientific men talk a great deal too confidently about many points..." (Quoted in L. Tollemach, *Talks with Mr. Gladstone,* (London: 1898), p. 152.) W.E.H. Lecky admitted there "were wide tracts of knowledge with which he [Gladstone] had no sympathy. The whole great field of modern scientific discovery was out of his range." See Lecky, *Democracy and Liberty,* vol. I, (London: Longmans, 1896), p. xxxi. Lord Morley recalled that Gladstone watched science "vaguely and with misgiving....from any full or serious examination of the...scientific movement he stood aside, safe and steadfast within the citadel of Tradition", (Morley, *op. cit.,* vol. 1, p. 209).

10 Alan Brown, *The Metaphysical Society: Victorian Minds in Crisis, 1869-1880* (New York: 1947), Preface. p. 21.

11 Gladstone's well-meant but ill-informed attempts to reconcile scripture with science led to his classic duel with Huxley in the *Nineteenth Century* in 1885.

12 Quoted in *The Times* (April 26, 1872), and in "Mental Darkness in High Places" *Nature, 6* (May 9, 1872), 21.

13 *Vide* George Haines, *Essays on German Influence upon English Education and Science, 1850-1919* (Connecticut College Monograph No. 9), esp. ch. III.

14 "Scientific Administration", *Nature, 2* (October 6, 1870), 449.

15 B.H. Becker, *Scientific London* (London: H.S. King & Co., 1874), p. 136.
16 George Gore, FRS, "On the Present Position of Science in Relation to the British Government", *Transactions of the Social Science Association* (Plymouth: 1872), p. 284.
17 Ayrton (1816-1886), M.P., 1857-1874. Ayrton's nephew was W.E. Ayrton, the electrical engineer; two of his great-grandnephews (now living) include Professor Oliver Zangwell, the psychologist, and Michael Ayrton, the painter.
18 The *Birmingham Daily Post* (August 12, 1872).
19 *Weekly Despatch* (July 14, 1872); *North British Daily Mail* (July 15, 1872).
20 Cf. *The Times* (November 8, 1857; August 25, 1858; October 1, 1867).
21 Hansard, *Parliamentary Debates*, Third Series, 181, c.1613.
22 Hansard, 182 c. 815, 1111; 183 c.201; 198 c.963.
23 Hansard, 187 c.362; 885; 189 c.1341-1343.
24 Hansard, 188 c. 1682.
25 *The Times* (October 1, 1867).
26 Cf. *Kew Papers* (afterwards *KP*), vol. II, f.480, Earl Derby to John Tyndall, August 22, 1872.
27 A. Patchett Martin, *Life and Letters of the Rt. Hon. Robert Lowe, Viscount Sherbrooke* (London: 1893), vol. II, p. 379.
28 Works 22/30 PRO. "A Historical Survey of the Ministry of Works", (ca. 1945). In 1942 the Ministry of Works and Buildings absorbed the Ministry of Health powers under the Town and Country Planning Acts to become the Ministry of Works and Planning. In 1943, with the creation of a new Ministry of Town and Country Planning, these functions were separated, and a new Ministry of Works emerged. This Ministry was absorbed by the Department of the Environment created in 1970.
29 W.N. Bruce *Sir Henry Layard, GCB, DCL. Autobiographical Letters from his childhood until his Appointment as H.M. Ambassador at Madrid* (London: 1903), vol. 2, p. 257.
30 *Ibid*, p. 262, Cf. Hansard, 199 c. 168.
31 Quoted in Bruce, *op. cit.* p. 264.
32 *The Globe* (July 13, 1872).
33 *Weekly Despatch* (July 14, 1872).
34 *Illustrated London News* (November 13, 1869), p. 498.
35 Cf. Sir Francis L.C. Floud, *The Ministry of Agriculture and Fisheries* (London: 1927), ch. 18. In 1903 the gardens were placed under the Ministry of Agriculture.
36 *Daily Telegraph* (July 16, 1872).
37 *Nature, 6* (May 9, 1872), 21.
38 *Tyndall Papers*, VIII, f. 2600. Hooker to Tyndall, March 24, 1869.
39 *K P.*, vol. I, f. 26, letter to First Commissioner, August 31, 1871.
40 *K P.*, I, f. 38, Hooker to First Lord, Treasury, August 31, 1871.
41 Under previous arrangements Consulting Architects on the Board's staff were paid commissions of 5% on all new buildings designed by them, together with a regular salary for supervising existing buildings. This meant that in 1869, for example, over £100,000 of public money was being paid to consultants (including the Barrys, Waterhouse and Street), who themselves determined the size and cost of public works. Ayrton wished to abolish the commission system and appoint an architect on the basis of an ordinary civil servant. The Treasury agreed to do the first but not the second. Works 22/2-20 PRO. Creation of the Director of Works, 1870.

42 Works 22/20.P.R.O.George Hamilton, Treasury to Ayrton, January 17, 1870.
43 Douglas Galton (1822-1899); educated Rugby and Royal Military Academy, Woolwich; Royal Engineers, 1847, Secretary of Railway Commission, 1854, Secretary, Railway Department, Board of Trade; 1857, Government Referee for Main Drainage of Land; 1858-1861, member of Royal Commission on Sanitary Condition of Barracks and Hospitals; 1859-1861, Chairman of committee to investigate submarine telegraphy; 1866, member of Royal Commission on Railways (with Seventh Duke of Devonshire); 1862-1869, Assistant Permanent Under Secretary of State for War; 1869-1876, Director of Public Works and Buildings, Office of Works; Member of British Association from 1860; General Secretary of British Association, 1871-1875; President of the British Association, 1895; CB 1865, KCB, 1887, DCL (Oxon), 1875.
44 *KP.*, I, f.9, Galton to Hooker, July 3, 1871 "private".
45 *KP.*, I, f.10, Hooker to Galton, July 4, 1871.
46 *KP.*, I, f.13, Board of Works to Hooker, August 17, 1871.
47 *KP.*, I, f.14, Hooker to Board of Works, August 19, 1871.
48 *KP.*, I, f.24, Hooker to Lady Russell for Lord John Russell, August 26, 1871.
49 *KP.*, I, f.18, Hooker to Gladstone, August 19, 1871, enclosing letter of same date to Ayrton.
50 *Ibid.*
51 *KP*, I, f.38, Hooker to Gladstone, August 31, 1871.
52 *KP*, I, f.45, Gladstone to Hooker, October 4, 1871.
53 *KP*, I, f.70, Hooker to West, October 30, 1871.
54 Benjamin Jowett's Personal Memoir, in A.P. Martin, *op.cit.*, vol. II, p. 497.
55 *KP*, I, f.57, Airy to Hooker, October 18, 1871.
56 *KP*, I, f.64, Airy to Hooker, October 25, 1871.
57 *KP*, I, f.83, Ball to Hooker, November 1, 1871.
58 *KP*, I, 84, Ball to Hooker, November 3, 1871.
59 *KP*, I, ff.88, 89, 90, Holland to Hooker, November 3 and 9, enclosing letter from Gladstone to Holland, 8 November 1871.
60 *KP*, I, f.103, R. Palgrave to Hooker, February 7, 1872.
61 *KP*, I, f.105, Hooker to Lingen, February 19, 1872.
62 *KP*, I, f.113, Lingen to Hooker, February 26, 1872.
63 *KP*, I, f.119, Welby to Hooker, March 9, 1872.
64 *KP*, I, f.109, Lyell to Hooker, February 23, 1872.
65 *KP*, I, f.126, Memorandum laid before Cabinet Committee, consisting of Lords Ripon, Halifax, and Cardwell, March 13, 1872.
66 *KP*, I, f.129, Helps to Hooker, March 15, 1872.
67 *KP*, I, f.134, Hooker to Ripon, March 19, 1872.
68 *Tyndall Papers*, VIII, f. 2608, Derby to Tyndall, March 27, 1872. For evidence of Derby's scientific interests, see Sir T.H. Sanderson and E.S. Roscoe, *Speeches and Addresses of Edward Henry, XVth Earl of Derby, K.G.*, 2 vols. (London: 1894).
69 *KP*, f.140, Tyndall to Derby, April 2. A copy exists in *Tyndall Papers*, VIII, f. 2610.
70 *Ibid.*
71 *KP*, I, f. 146, Helps to Hooker, April 2, 1872.
72 E.A. Helps, *Correspondence of Sir Arthur Helps, KCB, DCL* (London: 1917), p. 15. Helps (1829-1875) was also well regarded among scientists for his consistent support of Dr. John Simon at the Medical Department of the Privy Council.

73 *KP,* I, f.147, Hooker to Tyndall, April 8, 1872.
74 *KP,* I, f.149, Tyndall to Hooker, April 18, 1872.
75 *Tyndall Papers,* VIII, f. 2620, Hooker to Tyndall, April 17, 1872.
76 *KP,* I, f. 157, Hooker to West, April 24, 1872.
77 *Tyndall Papers,* VIII, f. 2626, Hooker to Tyndall, (n.d.)
78 *Tyndall Papers,* VIII, f. 2623. Hooker to Tyndall, April 23, 1872.
79 *KP.,* I, f.164 (T 7113/1872). Charles Strange, Treasury to Hooker, April 25, 1872.
80 *KP.,* I, f. 189, Hooker to Argyll, May 9, 1872.
81 *Tyndall Papers,* VIII, f.2629, Tyndall to Derby, May 3, 1872.
82 *KP.,* I, f.183, Huxley to Lowe, May 4, 1872, marked "private". The political maneuvers of this "scientists' caucus" are described in R.M. MacLeod, "The X-Club: A Scientific Network in Late Victorian England", *Notes and Records of the Royal Society, 24,* No. 2 (1970), 305-322.
83 *KP.,* I, f. 197, Huxley to Hooker, May 11, 1872.
84 *KP.,* I, f. 210, Tyndall to Hooker, May 18, 1872.
85 *KP.,* I, f. 222, Tyndall to Hooker, June 3, 1872.
86 B.M. Add. Mss. 39, 954, f. 530. Ayrton to Owen, May 8, 1872. Cf. "Dr. Hooker's Reply to Professor Owen", *Nature, 6* (October 24, 1872), 516-517.
87 *Tyndall Papers,* VIII, f.2648, Tyndall to Hooker, June 8, 1972, marked "private".
88 The signatures were Charles Lyell, Charles Darwin, George Bentham (President of the Linneaen Society), Henry Holland, George Burrows, (President, Royal College of Physicians), George Busk (President, Royal College of Surgeons), H.C. Rawlinson, (President, Royal Geographic Society), James Paget, William Spottiswoode, T.H. Huxley and John Tyndall.
89 *Tyndall Papers,* VIII, f.2653, Hooker to Tyndall, June 11, 1872.
90 *Tyndall Papers,* VIII, f.2664, Airy to Tyndall, June 20, 1872.
91 "A Physical Observatory", *Nature,*vol. 5, (April 25, 1872), 497.
92 *Tyndall Papers,* VIII, f. 2649, Lyell to Tyndall, June 9, 1872.
93 *The Times* on Monday, 8 July, carried an approving Leader visibly based on Tyndall's letter to Derby in April, and concluded that the incident marked a fresh example of "Mr. Ayrton's unfortunate tendency to carry out what he thinks is right in as unpleasant a manner as possible" (July 8, 1872). Other attacks were more personal and vicious. *The Morning Advertiser* sneered at the "gentle and courteous" Mr. Ayrton, who, not content with his exploits on the Thames Embankment, Epping Forest and Victoria Park, had sighed for new worlds to conquer. (July 9). The *Standard* declared that, if either Dr. Hooker or Mr. Ayrton had to resign, "the former would be a great loss" but "the room of the latter is far preferable to his company." (July 9). The Tory *Globe* dismissed Ayrton as a "beggar on horseback", a "suburban demagogue beloved for the ill-breeding which simulates independence, and the manners of a rough with the top dressing of a "swell", (July 9). "It is not very easy", concluded the *Guardian* "to comprehend Mr. Ayrton's object in offending against the ordinary rules of taste and manners whenever he gets an opportunity". "There might be a question", said the *Guardian,* "as to the tower of the Law Courts or as to the lighting of Westminster Clock, or as to the preservation of public monuments, but there is none as to Dr. Hooker and Kew". (July 10).
By Saturday, July 13, the week of scandal had reached a climax. The weekly *Gardeners Chronicle* published a Punch-like parody of the episode and collected new reports from

all the daily papers. Even the *Civil Service Gazette* indicted the Prime Minister for defending Ayrton and questioned the fate of a ministry which defended him. The *Saturday Review* thrashed Ayrton for his policies generally. (July 13, 1872).
The following week saw more press interest, different papers rivalling each other with stories of "Mr. Ayrton Again", or the "Ayrton Scandal" and "the Suppression of Works by the Commissioner of Works". [The *Greenock Telegraph and Clyde Shipping Gazette* (July 16), The *Edinburgh Courant* (July 17), and the *Glasgow Herald* (July 18).] *Punch* (July 20) mocked Ayrton in a poem entitled "Noble Savage", while the *Sporting Times* said that to call Ayrton a noble savage was to "insult Nature" (July 20). The *Cambridge Chronicle* saw an ominous parallel between Ayrton's folly and the attitude that had recently sent HMS *Captain* to founder at sea for want of accepting scientific advice. Such slanders were "far too expensive for the nation when they sacrifice the best talent of the country to gratify a false and costly economy" (July 20).
94 *Daily News,* July 10; *Standard,* July 11; *Morning Post,* July 11; *Pall Mall Gazette,* July 11; *Economist,* July 13.
95 There is, for example, no mention of the Ayrton incident in Horace G. Hutchinson, *Life of Sir John Lubbock, Lord Avebury* (London: 1914), 2 vols. which, on the contrary, stresses Lubbock's strong friendship with Gladstone and other Liberal leaders.
96 *KP.,* I, f. 241, H.A. Bruce to Hooker, June 24, 1872, marked "private".
97 *KP.,* I, f. 244, Lingen to Hooker, June 26, 1872.
98 *Tyndall Papers,* VIII, f. 2673, Hooker to Tyndall, July 2, 1872.
99 *KP*, I, f.275, J. Tollemache to Hooker, July 12, 1872.
100 *KP,* I, f. 294, Huxley to Hooker, 15 July 1872.
101 *KP*, I, f. 296. James Bateman to Hooker, July 16, 1872.
102 *KP*, I, f.305, Sir Robert Christison, Edinburgh, to Hooker, July 18, 1872; f.306, Christison to Hooker, July 18, 1872.
103 *KP* , I, f. 313, George H. Chalmers to Gladstone, July 20, 1872.
104 J.S. Lombard, M.D. to *The Standard,* July 17, 1872.
105 *KP*, II f.402, Edward M. Barry to Hooker, August 1, 1872. Barry's brother was the architect of the Victorian extensions to Burlington House.
106 *Tyndall Papers,* VIII, f. 2685, Hooker to Tyndall, July 19, 1872.
107 *KP*, I, f. 309, W.D. Greg, HMSO, to Hooker, July 19, 1872.
108 *KP*, I, 315, Hooker to Greg, July 20, 1872.
109 Sir Algernon West, *Recollections, 1832-1886* (London: 1866), vol. II, p. 58
110 *Nature 6* (August 8, 1872), 280.
111 *KP.,* II, f.355, George Russell, Board of Works, forwarding Treasury Minute, July 24, to Hooker, July 25.
112 T1/7231A/12009. Lingen, Minute, July 24, 1872; *Nature 6* (August 8, 1872), 280-2.
113 *KP*, II, f.391, Lubbock to Hooker, July 30, 1872.
114 Papers relating to changes introduced into the Administration of the office of Works affecting the Direction and Management of the Gardens at Kew. 1872 (335). xlvii. 527.
115 *KP*, II, f.415, Tyndall to Hooker, August 5, 1872.
116 *KP*, II, f.400, Derby to Tyndall, July 31. Also in *Tyndall Papers,* VIII, f. 2698.
117 *The Scotsman* (July 30, 1872).
118, *KP*, II, f.411, Hooker to Lubbock, August 7, 1872; f. 427, Lubbock to Hooker, August 7, 1872.

119 *KP*, f. 435, Palgrave to Hooker, August 9, 1872.
120 *Tyndall Papers*, VIII, f.2702, Derby to Tyndall, August 22, 1872.
121 Hansard, 213. c. 749.
122 Hansard, 213. c. 756.
123 *KP*, II, f.436, Fawcett to Hooker, August 9; *KP.*, II, f.447, Huxley to Hooker, August 11, 1872.
124 *The Times*, August 9, 1872.
125 *KP*, II, f. 450, Ayrton to Hooker, August 13, 1872.
126 *KP*, II, f. 480, Derby to Tyndall, August 22, 1872.
127 *KP*, II, f. 565, Ayrton to Hooker, September 4; also in *Tyndall Papers*, VIII, f. 2710.
128 The Chancellor (Lowe), the Postmaster General (Monsell) and Ayrton were implicated in a complicated affair in which L800,000 had been detained on its way to the Exchequer and supplied to the telegraph services. Cf. Morley, *op. cit.*, vol. II, p. 68-69.
129 Ayrton was replaced by W.P. Adam, the Scottish Chief Whip; Bruce was given a peerage (Lord Aberdare); Lowe went to the Home Office, and Gladstone himself assumed the job of Chancellor.
130 In the meantime, Gladstone, recalling "with dissatisfaction that a name so distinguished in the history of research remains without a note of honour from the State", propitiated Richard Owen with the offer of a C.B. in May 1871. Rev. Richard Owen, *The Life of Richard Owen* (London: 1894), vol. II, p. 273.
131 Morley, *op. cit.*, vol. II, p. 72.
132 *Tyndall Papers*, VIII, f. 2712, Tyndall to Huxley, September 5, 1872.
133 *Tyndall Papers*, VIII, f. 2713, Huxley to Tyndall, September 9, 1872.
134 West, *op. cit.*, p. 14.
135 Owen's actions may have been inspired by his wishes to break up the botanical establishment at Kew and transfer it to his supervision at the British Museum. Owen's performance in the Ayrton incident completed his estrangement from the scientific community. As Tyndall wrote Huxley:

> I never broke with that man as you know. I hoped against hope that matters might in the end be healed up. But this last trick makes me feel that those who broke with him knew him better than I did. It will greatly augment his isolation.

Huxley's recollections were even more bitter than Tyndall's:

> ...Even my artistic powers could not put in the shadows more strongly than he has himself in that beautiful autotype memorandum furnished to Ayrton. I don't know whether the folly or the baseness of it shocks me more. But he is not worth good ink and paper.

Tyndall Papers, VIII, f. 2412, Tyndall to Huxley, September 5; f. 2713, Huxley to Tyndall, September 9.
136 Hooker accepted a Knighthood (KCSI) in 1877 before Owen was made KCB in 1884, but only with much reluctance, and from Disraeli's hands.
137 Cf. Works 22/8/12 (P.R.O.) Treasury Minute concerning Sir Douglas Galton, August 12, 1875.
138 Cf. Lord Rothschild, "The Organisation and Management of Government R & D", in *A Framework for Government Research and Development* (Cmnd. 4814), HMSO, 1970; and *Framework for Government Research and Development* (Cmnd. 5046), HMSO, 1972.

139 T.A. Hirst once described Tyndall as "afflicted with Gladstonophobia". In 1890 Tyndall characterised Gladstone as "the wickedest man of our day and generation". A.S. Eve and Creasey, *Life and Work of John Tyndall* (London: 1945), p. 242. Tyndall's role in the Ayrton incident is described in Eve and Creasey, pp. 163-166.
140 Cf. M.E. Grant Duff, *Notes from a Diary, 1892-1895* (London: 1904), vol. II, p. 112.
141 *The Times*, February 7, 1874.
142 Hooker was PRS until 1878, Spottiswoode from 1878-1883, and Huxley from 1883-1885.

Social Marginality and Political Legitimacy in 19th-Century Madagascar

SOCIAL MARGINALITY AND POLITICAL LEGITIMACY IN NINETEENTH-CENTURY MADAGASCAR*

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I have chosen to discuss the perhaps not very well-known case of westernization of Madagascar in the early part of the nineteenth century for two main reasons:

The first is that I feel a strong personal sympathy with that small group of European diplomats and what we would today call technical advisers, who decided to turn 19th century Madagascar into the "Little Europe" of the Indian Ocean.[1] Without the help of any Ministry for International Cooperation, or United Nations Technical Assistance Programs, or the International Bank for Reconstruction and Development, or UNESCO, they undertook, by themselves, over 150 years ago, many of the present-day activities of these sophisticated modern international bodies. They failed in the attempt: to a certain extent, I am afraid, like many of their better-known successors, who have the advantage of operating in a world which at least knows the use of remedies against malaria and yellow fever. But the lessons which can be derived from this failure are quite relevant to the contemporary historian of science and to students of modernization problems.

The second reason for my choosing Madagascar as the topic of this paper is my belief that in the field of transculturation, or social change and development, one of the great obstacles to the understanding of the reasons why some societies "succeed in succeeding", and others do not, lies in the success itself. The final results of successful transculturation (as in the case of Japan and Israel) seldom allow a clear post-facto vision of the complex process which has brought them about. I remember the remark made by an African student to an Israeli who was showing him a six-month-old motorcar assembly plant in Nazareth, and telling him that unlike in Detroit, where nobody could show how a factory originally started, here he could see the very inception of a car assembly plant. The African replied: "Can you also please show me where these machines conceal the 4,000 years of Jewish history?"

*I am indebted to Dr. Yehuda Elkana and Mrs. Lydia Aran for their comments and critical reading of this paper.

Madagascar is quite a different case: every piece of the machinery created in the attempt to westernize and industrialize that country some 150 years ago is scattered about and available for study. Like the pieces of a toy, broken before it was completed, the works of the unsuccessful, pre-colonial modernization attempt lie before the eyes of the historian and the social scientist.

The purpose of this paper is to show how some, at least of these relics, if properly interpreted, can offer some interesting insights into the mechanism of the modernization process in a non-European society.

I shall attempt to do this *firstly* by presenting a very concise description of the Malagasy situation between 1820 and 1856; *secondly,* by suggesting an interpretation of the motivations of the main actors involved in the modernization process in Madagascar (namely the indigenous aristocracy, the Protestant missionaries, the British military and political advisors, and the European adventurers who replaced them); and *thirdly*, by submitting the claim that it is possible to reach a better understanding of the reasons for the failure of Malagasy modernization–and through this, of some of the problems facing developing countries of our time–by looking into the relation between the carriers of modernization and the authority for which, or under which they operated.

I. THE HISTORICAL BACKGROUND

It is generally agreed that the year 1810 represents a turning-point in Malagasy history. This was the year in which radical changes took place in and around Madagascar.

In 1810, Ile de France–or Mauritius, as the island had previously been named by the Dutch in honor of Maurice of Nassau–was lost by France to the British in the wake of the Napoleonic Wars. Mauritius was the "pearl" of French possession in the Western Indian Ocean, the "entrepot" for the now substantially reduced colonial empire which the pre-revolutionary Paris government had tried to create on the west coast of India. However, the other French island in the area, Bourbon–better known today as La Reunion–smaller but at that time more populous than Mauritius, was returned to France following the Treaty of Paris of 30th May, 1814, probably because the British found it difficult to replace a Bourbon on the throne of France and at the same time deprive him of the island which bore his family name.

The treaty, however, made no mention of Madagascar, the huge island-continent where the French had for a long time been trying unsuccessfully to enlarge their scattered coastal bases. This great island was soon to become an object of contention between the governments of Paris and London, both acting under the pressure of the European settlers of the two, tiny, and now politically separated islands of Mauritius and La Reunion, who regarded Madagascar as their national *lebensraum.*

French-British rivalry over Madagascar began in 1816, when the French governor of La Reunion attempted to revive the presence and influence of France on the southern and eastern coasts of Madagascar.

He was stopped by the British Governor of Mauritius, Sir Robert Farquhar, who claimed that in the archives and the official French publications of the Island, the French establishments in Madagascar were described as dependencies of Mauritius. The French appealed to London where, in deference to Louis XVIII, they were informed by Lord Bathurst that Madagascar was not to be considered a dependency of Mauritius.

Sir Robert Farquhar, who did not share his government's views and was aware of the political turmoil inside Madagascar, decided to support the native party most likely to oppose the French. These were the Merinas[2], and it was with their leader, Radama, that the British Governor of Mauritius established diplomatic contact. As long as Radama lived, the British influence remained supreme and unchallenged in the highlands of Madagascar, while the Merinas, with direct British help, extended their political control from the center of the island to its coasts, expelling the French from their maritime "*comptoirs*", and bringing under one central political rule most of the tribes of Madagascar.

After Radama's death in 1826, his successor, Queen Ranavalona I, embarked on a new policy of total political isolation: in 1829 she fought the French on the eastern coast. Not receiving much concrete help from the British in this venture, nor being able to distinguish very clearly between France, Britain or any other white man's country beyond the seas, she refused, in 1830, to renew the treaty with Britain, and later expelled all the British missionaries who had established themselves in the country. Foreign influence, and in particular Christian influence, was considered by the new xenophobic government of the queen as a threat to the security of the country. In 1845 the queen's army fought a joint British-French naval expedition dispatched to protect European traders who were trying to break the economic isolation imposed by the Queen, and whom the Malagasy wanted to keep under their own jurisdiction (making them liable to punishment by death or slavery). The British later had to pay a heavy fine to be allowed to trade again with Madagascar.

After the queen's death in 1861, her son, Radama II, deeply influenced by Christian teachings and by a French adventurer Jean Laborde,[3] reversed her policy and threw the country open the Europeans, offering the French a virtual economic monopoly over the country.[4] In 1863 he fell victim to the reaction of the more conservative elements in the country.

Thereafter the Malagasy struggled to maintain an uneasy balance between foreign influence and political independence under two more queens, Rasoherina (1863-68) and Ranavalona II (1868-83), who converted to Christianity. In spite of the notable efforts at modernization made by the government under the able Prime Minister, Rainilaiarivony (who ran the country from 1864 to 1895), the abolition of slavery and the adoption of an advanced code of law, the country could not oppose the spread of European influence.[5] In 1885 the French established a protectorate over the island with the consent of the British, and in 1896 the country was annexed to the French empire as a colony, the last queen of Madagascar, Ranavalona III, being exiled to Algiers.

Bearing in mind this general historical background, it is now possible to turn to the more specific story of the process of modernization which took place in Madagascar, and more particularly in the Merina highlands, between 1820 and 1860.

On the initiative of Sir Robert Farquhar, a British officer, Captain Le Sage, accompanied by 30 soldiers, was dispatched to Tananarive in 1817 to sign the first treaty of friendship and commerce between Britain and the Merinas kingdom. This treaty was followed by another one, negotiated by James Hastie, a Scottish sergeant from the Indian Army, who later became British Resident in Madagascar and the driving force behind the British policy of technical cooperation there. In this second treaty, King Radama undertook to put an end to all slave-trading with the outside world (and promised to punish with slavery anyone engaging in the trade from then on). In exchange he was recognized as King of Madagascar by the British, and given a yearly grant of Spanish dollars, guns, powder, military uniforms and horses.

The agreement was accompanied by a British promise to train young Malagasies in England and in Mauritius in useful trades. This agreement, signed between the London Missionary Society and the King's representatives in 1821, is probably the first treaty of technical cooperation ever made between an African state and a European country.[6] It contained, *inter alia,* what could be called a guarantee against "brain drain", the British Missionary Society being asked to return the young Malagasies home at the end of their studies. So started that British-Malagasy cooperation for the modernization of Madagascar which had a deep social impact on the tribal, underdeveloped and traditional Merina society.

The Merinas are a very curious people of mysterious origin. They come from Melanesia but how they crossed the Pacific and Indian oceans, when they crossed them, and how they penetrated into the heart of Madagascar, the highlands of Imerina (Imerina meaning "the place where the eye can look far away")–is still a much-debated subject.

They are one of the 21 "tribes" of African, Arab, Indian, and later on, of European origin, who form the present-day population of Madagascar. Together with the Islamized Sakalava, established on the north-western side of the island, they remained from the 16th century onward the most active political element in Madagascar.

From their powerful neighbors, the Sakalava, the Merina–or Hova–took many of their feudal political institutions.[7] When, earlier, they developed–apparently independently–the technique of growing rice in irrigated paddies instead of in the ashes of burnt-down forests (like their predecessors in the highlands, the Vazimba), an agricultural class of landed proprietors interested in peace and security began to grow up alongside the military, slave-trading aristocracy.[8]

Merina culture remained for a long time backward and unbalanced: it did not know the use of the wheel, but understood the basic principles of hydraulic engineering; it developed a common language throughout the island–but no writing. To provide some of the necessary means of government, the Hova kings imported "magicians" in the 16th century from the small, Islamized group of Antehdro in the south, who knew the secret of the Arabic alphabet, and guns from the Arab traders in the north.[9]

At about the time of the French Revolution, a great leader, Andrianpoinimerina, unified the highlands of Imerina, gave his people a new political and administrative organization, more progressive legislation, and for the first time envisaged the unification of the whole island (which is as large as France and the Benelux countries put together).[10] He did not, in fact, go beyond the limits of a great tribal ruler, though some historians regard him as a revolutionary innovator. At all events, it was left to his son, Radama I, to take the crucial step of admitting into his country the new "magicians" from beyond the seas–the European missionaries, advisors and soldiers.

The bargain which he struck was simple: he stopped selling slaves abroad, thus depriving his country and his aristocratic military class of their major exchange commodity. In exchange for joining the British in the great anti-Slavery campaign, he received weapons and military advisors, which made his army so powerful that he could conquer his neighbors. The political advantages for the king were evident, the economic ones for the Malagasy military elite much less. Although they were still free to enslave their captured enemies, the fact that they could use them only for work at home, and not sell them abroad, reduced their price and was more beneficial to the rice-growing "bourgeoisie" than to the military aristocracy, which was soon forced to make way for the growing trading-military plutocracy. This created strong tension in the society favoring an unstable political compromise between the military, the great land owners and the old aristocracy, which at the death of Radama brought to the throne Queen Ranavalona, one of his wifes, instead of his son and his pro-Christian, innovating brother. Ranavalona was chosen for her faithfulness to the native religion and customs, while all the pro-Christian, pro-British and modernizing members of the King's family, including her mother, were quickly disposed of by the new ruling clique. This was not enough to stop the process of westernisation since the King had imported the white "magicians" to teach new trades to the people and the secret of writing to his officials. Their work and their religious teachings were upsetting the traditional society. Furthermore, Radama I had established himself as the undisputed controller of the modernization process and a political arbiter of the conflicts and currents which this process unleashed. Since he had the authority to do this, but no one after him, the missionaries became a direct challenge to the authority of the new rulers.

We can find, therefore, in the Imerina highlands in 1820, many of the elements—albeit in embryo—of a contemporary modernization drama in an undeveloped country: imported development versus traditional backwardness; private initiative versus government-controlled modernization; mass modern education versus indigenous cultural tradition; technical assistance and westernization versus tribal anarchy and xenophobia; economic development versus political decay; new social and religious values versus old traditions; diplomatic intrigues versus dispassionate self-sacrifice; hopes for quick development versus frustration from slow advance.

Even a powerful traditional ruler like Radama I with a mind open to innovation but with a clear understanding of his limitations, had to fight against strong political and religious resistance groups which saw in innovation in general, and in innovation imported by foreigners in particular, a challenge to their status, authority and vested interests. When, in the great *Kabary* of 1820,[11] he sought approval for the introduction of foreign innovations and the banning of slave-exports (almost paraphrasing Bacon's dictum—Science is power), his aims were probably not understood by his people, but his actions were trusted as those of a legitimated leader. The king's weaker successors could not without risking internal revolt continue to promote innovations—which they needed mainly for military purposes. They turned for help and for legitimation of their collaboration with foreign 'experts' to the traditionalist elements. These latter were prepared to accept technical innovations such as guns, factories, new building techniques, soap and furniture production etc.–but not the economic, social and political changes which would have been indispensable, if the work of the Europeans established in Madagascar were to become part of a general process of modernization. More than anything else, they were afraid of the religious ideas which the British missionaries insisted on importing into the country together with education, trade and technical skills. Ramada I had been able to strike a balance between imports and the spread of foreign goods and foreign ideas. His successor could not, since by the time of Radama's death foreign goods and ideas had snowballed into a foreign "progressive" party opposed to the indigenous traditionalist one. They therefore resolved that the country should retreat into a state of complete isolation, within which they would separate foreign goods which needed to be imported, from foreign ideas which had to be suppressed. There followed many years of great investments in military industries accompanied by ruthless persecution of native Christians; of fabulous social and economic opportunities offered to a handful of European adventurers combined with strict control over their political behavior and religious teaching. The later rulers thus choked off all possibility of any self-sustained development.

But the fact that both during the time of the British missionaries (up to 1835) and the time of the European adventurers who followed them (up to 1857) there were never more than a dozen foreigners at any one time involved, raises an obvious question: was the Malagasy experiment in modernization an experiment carried out, so to speak, in a laboratory test-tube, so unique that it cannot serve as a basis for any larger conclusions? Or can it support some general ideas about the interaction between traditional societies and imported modernization? I would say that the Malagasy case was more of a "spasm" or spurt of development than a process of development. But just as a neurosis can give insight into the human character, so can this "test-tube" experiment in pre-colonial modernization, this "spasm" of development, I believe, help the student of social sciences to gain some insight into the process of transculturation.

II. THE "CARRIERS" OF MODERNIZATION IN PRE-COLONIAL MADAGASCAR

One can distinguish four main groups involved in the process of transculturation in pre-colonial Madagascar: firstly the missionaries, secondly the Malagasy elite, thirdly the official political and military advisors, and fourthly the expatriate adventurers.

The missionaries were far and away the most active and interesting group. They were all British Protestants, convinced anti-Catholics, which, at that time and in that place, meant being anti-French as well.[12] They operated as an organized religious group from 1818 to 1835, and two of them continued afterwards, from 1836 to 1841, in a private capacity, divested of all religious status.[13] All of them had left the Merina kingdom by 1842, and they did not return there in force until 1861, though the Foreign Secretary of the London Missionary Society, E.W. Ellis, paid a visit to Madagascar as early as 1857.[14]

These missionaries were a very small group, almost never more than half a dozen men at any one time (say twenty souls, including their wives and children), yet they achieved extraordinary results.[15] They transcribed the Malagasy language into Latin characters, organized a vast native educational system, over which they presided, and which including about 10,000 pupils over a period of about 10 years; they introduced the idea of Christianity more widely and more deeply then the actual number of conversions might imply; they brought with them "mechanics" (the artisans who introduced many basic trades into the said island–printing, shoemaking, tanning, weaving, ironmongery, carpentry, building, toolmaking, and so on). Some of these "mechanics" showed extraordinary energy and genius, such as James Cameron,[16] who built the first palace for the Queen, created a number of workshops, trained hundreds of native artisans, taught chemistry and mathematics at school, and acted as a kind of Robinson Crusoe in an area thickly inhabited by industrious Man Fridays.

What type of men were the missionaries? On the whole, a small but significant sample of an identifiable social group in the British society of the time. Socially they belonged to that lower middle class from England, Wales and Scotland who, in the wake of the religious revival of 18th century Britain, regarded their religious activities among the pagans as a kind of promotion into the gentleman class.[17] To be a church man was already to be "somebody": to be a missionary was to add the actual practice of a kind of upper class way of life.

The spreading of religion overseas, like the spreading of political influence through colonization, is intrinsically an aristocratic activity: it implies many values and attitudes proper to the nobleman as conceived by western chivalry–courage, a sense of superiority mixed with personal humility, a taste for sport and adventure, a readiness for sacrifice and generosity, a need to serve and to command at the same time.

The missionaries who went to Madagascar–and elsewhere–were all deeply convinced of the superiority of their Christian faith; they were ready to die for it, and often did so. However, their faith also had a political tinge, not only because it aimed at keeping the French Catholics out of Madagascar, but because it was part and parcel of a British way of life which they regarded as being culturally and socially higher than other cultures and societies, just as Protestantism, to them, seemed higher than other religions.

Financially speaking all these missionaries had no means of their own and had to make a living. They were always pestering the "Directors" of the Missionary Society in London for more money, and were, on the other hand, encouraged by London headquarters to try and become materially self-sufficient, either by reducing their expenses or by promoting remunerative trades. This was especially true for the mission "mechanics", who were entitled to draw less money on their London accounts than the clergymen.

Once abroad, the British missionaries in Madagascar and elsewhere[18] formed a small group of people who made a point of living according to what they thought were the dignified standards of life of the British gentry: no easy resolution in a far-away country, submitted to foreign rule, to strong economic and power temptations, to social pressures and petty community tensions.[19] The missionaries' diaries and correspondence (which can be counter-checked against documents from the Malagasy archives) show us how their work met with unexpected obstacles and how their actions were misunderstood by the natives, not because they pursued any given policy but just because their conduct was guided by values, habits and social codes pertaining to a different society and culture.[20] Tied up in these multiple objective and subjective strait-jackets (like so many of our contemporary technical advisers), the missionaries had to struggle with problems of status, faith, money, and their political and social relations with the native authorities—four problems which I would like to discuss in some detail.

The status problems of the missionaries were in most cases caused by their constant feeling of living at the periphery of their own and of the native society. They stood somewhat outside the British society in which they lived at home and into which many hoped to re-integrate (at a higher social level) upon their return, thanks to their missionary achievements overseas. Furthermore, when they did not die of fever or by the sword, missionaries tended to achieve abroad a position of authority and wealth which was very different from the poverty and social inferiority they usually knew in England. British diplomatic reports from Madagascar tell us, in a very critical way, of their horses and houses, their wives' dresses and their slaves.[21] In Tananarive, the capital of the Hova kingdom, the missionaries resented this kind of social control by the official political representative and fought bitterly against the British Agent in order to maintain direct, and possibly more influential channels of communication with the native government.[22] Their pressure, *in loco* and at home, on the British authorities, to promote, protect and extend their religious activities in the country was at the same time persistent.[23] If one replaces the word "Agency" with the word "Embassy", and "Mission" with "technical aid mission", one has in the Tananarive of the 1820's many of the institutional personal and bureaucratic problems and intrigues usually to be found today, in newly independent countries' capitals, where the representatives of the metropolitan Foreign Ministries jockey for influence and position with the representatives of the Ministries for Cooperation, those of the Defence Ministries with those of the Economic Ministries, and so on.

All this is neither new nor surprising: what is interesting is that the Malagasy have left us written evidence of how they regarded these foreigners' squabbles–how they played the (aid) mission against the political mission, how rapidly they discovered the true social status of each and every missionary, how they used their psychological complexes, and their feelings of social marginality or their economic needs as political cards to be played for the benefit of Malagasy national–and more often–internal political interests.[24]

One could wish we had similar official records of the attitudes of contemporary political leaders in the underdeveloped countries to our foreign aid policies. All we have to rely on instead are the speculations of the experts on the donor side, who–like the missionaries of old–tend to divide the actions of those involved in aid-to-development into the schematized performance of "good" and "bad" Americans, Russians, French, British or Israelis, etc.

The second important point with the missionaries was faith. Of the honesty and strength of their personal faith there is little doubt. With one possible exception, the missionary group which worked for over ten years in Madagascar gave outstanding proof of its high religious qualities. But not all their religious activities, however, had either a religious aim or religious consequences.

I have already suggested how important it was for the British Protestant clergymen to keep out Catholic ministers with their French influence. Far more important was the political impact which Christian teaching had on the political life and structures of Malagasy society. By introducing printing into a country which had no written language and no transport system apart from human carriers, they introduced a new, powerful system of communication, which was so selective (since it could be used only by a small number of educated men) as to look like a "secret code", or at least like the basic condition for joining a powerful social and political "club".

The Christian message formed the main subject of what was printed, and taught, together with grammar, arithmetic and trades. Textbooks for reading and writing the Malagasy language were based on Biblical texts. Basic economic and social ideas, essential to modernization of a western type, such as trade, credit, free organization, democratic systems of decision by voting, etc., were constantly introduced through religious channels and with religious terminology. With these ideas came others: revolutionary social and political ideas such as the equality of man, the dignity of the individual, the rights of the poor, the morality of a society based on private property, monogamy, and so on. To accept these ideas and to elaborate on them meant undercutting some of the political promises on which the traditional, tribal and aristocratic Malagasy society was based. It was no coincidence that the decision to expel the missionaries was taken soon after a Malagasy "prophet" began to preach a new kind of native religion strongly tainted with Christian ideas. It was also not a coincidence that one of the first quarrels between King Radama I (in spite of his fondness for the missionaries' work) and the missionaries was over the organization and running of the *school society*, whose aim was to finance the development of education in Madagascar, and which was founded with extraordinary foresight in 1826.[25]

Turning to the second group, the Malagasy elite, we can easily see how much more impressed they were by the new political and social ideas brought in by the Europeans, than by their techniques.[26] Quite apart from the human, moral and social inspiration which the Malagasy–a deeply religious people–derived from Christianity, the new religion, which today would be called an "ideology", served as a common denominator for people from different levels of society. This denominator, by cutting across the traditional strata, mixed old-established loyalties, provided the framework for new cooperation or common resistance among people who felt they had common interests to promote or defend. However, what was lacking and what the missionaries were unable to create was a set of new institutions through which to express and coordinate these feelings. This was not due to their lack of authority (they organized a school system for thousands of pupils, and their mechanics were capable of building great workshops and even trade companies), but because they had no control over the sources of legitimation of the accepted authority. To be Christian or pro-Christian in Madagascar soon came to mean to belong to a party whose foundations were alien to the indigenous mind and society. Christianity became an idea to be fought against by the traditionalist elements in Madagascar, not for what it said about God, but for what it spurred men on to say about their leaders and their own rights, on the authority of a foreign god.

Of the thousands of Malagasies who fell victim to the anti-Christian persecutions which followed the expulsion of the missionaries, few were really Christians: they were upper-class people, army officers, merchants, officials, intellectuals, who had found in the new Christian faith and ideas a new and wider political framework for the expression of, and new justification for,their views and interests. They had thus become a danger to the traditional vested interests and elites in the country.[27]

If we substitute the words "capitalism" or "Marxism" for the word "Christianity", the old quarrel between the Malagasy government and the missionaries takes on a modern form. "You should give us the tools", said the Malagasy government to the Europeans, "and we will do the job". To which the retort of the European missionaries was: "The tools are part of western Christian civilization: without accepting its values, you cannot use its tools".

The Malagasy attempt at modernisation was beyond their ability. Valid for societies like Japan, India or China, which could oppose to the "metaphysics" of the West their own "metaphysics", and their own compact body of traditional learning to the foreign culture, it was totally unpractical for Madagascar. In fact, when the meeting of two cultures takes the form of a clash between a highly developed culture and a very weak one, then the cohesion of the group belonging to the more developed society and working with a sense of mission becomes all important: not only because of the combined energy it can develop, but for the choices it makes of subjects to be taught and those to be withheld.[28]

In the case of Madagascar, the choices made by the Protestant missionaries were largely independent of the wishes and needs of the recipient party, since they, the "giving side", were passionately attached to the vision they had of their own values, and of their own appreciation of what was good or bad for the recipient side.

As for the young Malagasy sent to England to study, they were immediately caught upon their return home in the dilemma of what they should teach. Quite apart from their resentment of being put under the supervision of foreigners (which sometimes created personal tension), they had to face the fact that they could not detach the tools they were using from the intellectual background which produced them. (Where were they to take the text of the lessons to translate from, if the Bible was the only literary material available to them?)

They were supposed to inherit the white man's knowledge and pass it on to their fellow natives. In fact, because of the white man's suspect political position in the Malagasy society, the only thing that the native trainees could do was to despise the values of a culture they could not master. Instead of being a source of even limited information, they become a source of faulty communication to their fellow countrymen. The foreign message they were supposed to pass on, even if correctly translated into the native language, became distorted and unintelligible for their pupils because the native teachers could not convey the values underlying the message. They used much of their newly acquired foreign knowledge to fight those people whose very presence represented a permanent witness to their lack of knowledge.[29]

The political and military advisers to the Malagasy were, on the whole, a third and far less complicated group. They had the advantage–unlike the missionaries–of belonging to institutions which possessed clear and defined aims, which exercised some sort of disciplinary control on them, if by no other means than through the regular payment of salaries. The interesting point about the British military advisers in Madagascar is that the most successful ones were those who came from low-ranking positions in the British military colonial establishment. In their dealings with the native aristocracy they found not only a professional challenge, the outcome of which could bring praise from their own British superiors, but a true feeling of social promotion.

Brady, who rose to be a marshal in the Malagasy army, was a mulatto drill sergeant from Jamaica (so low in the British military consideration that his full name is never mentioned). He found no difficulty in marrying into the Malagasy upper class.[30] James Hastie, who became the King's blood-brother, financial associate, and the great reformer of Malagasy society, was an Indian Army sergeant of Scottish extraction. He was undoubtedly a man of extraordinary moral and intellectual qualities. In Mauritius, where he had been stationed, he was not allowed to sit down while reporting to General Hall, the Acting Governor, on his mission to Tananarive. The colonial society of the time would not accept him as a "gentleman", but only as a "sergeant".[31]

To Hastie, the diplomatic mission to the King of Madagascar was not only a great intellectual challenge but also a great social opportunity. He married a local princess, became rich, and was so attached to Madagascar that he asked to be buried in the new home country. He was the perfect type of successful administrator and dispenser of technical aid: a man who knew how to adapt himself to the logic of the local situation. He was helped to do this by his realization that the native society could recognize his abilities better than his own, and was ready to repay such recognition with devotion and understanding.

Robin, a French sergeant, probably a deserter from Napoleon's army, who arrived in Madagascar in 1819, was certainly not controlled by any "institution", but starting from his N.C.O. rank, he became secretary to the King and his teacher of French and arithmetic. He played a large part in the organization of the Palace Officers' School and rose to the rank of Grand Marshal of the Palace in 1836.

Hastie's successor as British agent, on the other hand, was Dr. Robert Lyall, a gentleman, a scientist, and a man of established reputation in British society. When appointed to Madagascar, he tried to impress the King with his superior scientific knowledge–a "magic" knowledge he could not share with the Malagasy. He gave to the role of British Resident a new image: that of a powerful foreign magician who could no longer be considered "one of the family". It was a different approach which implied a different style. Lyall was unable to part, as Hastie did, with the outward symbols of his official position, for instance, from his heavy red and gold uniform. Even in the greatest heat he took much pleasure in wearing it.[32] He could not stand the sordid but humanly understandable business which some of the missionaries were doing in order to make a living.[33] He could not admit that the Queen's orders should be transmitted to him through the Senior Churchman, even if this missionary's role of temporary adviser to the Queen might have been of some use to the agency.[34] He stood on protocol–British protocol, naturally–on and off duty. Among other things he insisted on riding his horse every morning, as a gentleman should. Once he rode too near a Malagasy holy place, of the importance of which he was probably not aware. The Malagasy were offended and took their revenge by putting a basket of snakes into his house in order to drive him out of the city, and eventually out of his mind. Thus ended the British political presence and protectorate in Madagascar. Were the reasons political? Undoubtedly yes, but Hastie would have tried to overcome the Malagasies' suspicions and to neutralize the anti-European party by making the political and diplomatic image of Great Britain in Madagascar fit–through his behavior –the changing psychological and social conditions of the native society. This Dr. Lyall could not do: he was so imbued with British self-confidence, British prestige, and European science, that with the best of intentions he could not become an accepted member of the native community. He was, and remained to the end, a "foreigner", his behavior underlining, not minimizing the foreign power which stood behind him.

When he was expelled in 1830, his place remained unfilled. When later the missionaries followed (1835), their place was taken by a band of mainly French adventurers, who did not represent any foreign government but were a useful means of communication with the outer world. (The most famous of these adventurers was Jean Baptiste Laborde, after whom contemporary France has named a luxury passenger liner. Another was M. de Lestelle). They were asked, or rather, allowed, by the Malagasy to continue the process of modernization started by the British, but with the condition that they work in total isolation from the outside world, and in close association with the Malagasy.

Their services were required primarily to provide a modern infrastructure to support the military establishment. To this end Laborde created gun factories and steel mills, workshops for the repair of all kinds of weapons, and laboratories for the preparation of gunpowder and primers. However, in the process, he and his associates also created factories for the production of non-military goods, such as bricks, furniture, hats, chocolates, lightning rods and soap. They worked exclusively to satisfy the requirements of the Queen, her court and the plutocratic elite of the capital. They operated on lavish grants and concessions with unlimited forced labor provided by the government. Out of their activities came the first chemical products of Madagascar–for instance, sulfuric acid (obtained by the strange method of soaking animal bowels in buckets of urine, which the citizens of the capital had to provide and carry to the factory). Laborde managed to smelt iron and produce potash; he imported steam machinery from Europe and Merino sheep from Australia; he developed vineyards and zoological gardens, built aqueducts, dams and palaces, and organized night banquets with fireworks, in imitation of those of the French Kings in Versailles. He acted as a member of the Malagasy upper class. On him the Queen bestowed Malagasy honors. He wore native dress, married a Malagasy princess, and one of his sons, who went to study in Paris, later became for a while a Malagasy Foreign Minister.[35] With his help and that of other European adventurers, the Malagasy government came to control a small merchant fleet, operated spirits distilleries and sugar plantations, and shared the benefit of organized monopoly over all the export and import trade with the outside world.

All this was neither an indication of real economic development nor of social progress. It was a symbiosis between a small number (not more than a dozen) of European adventurers, and a tyrannical, isolated, xenophobic and traditionalist native government. But it was also a perfect example of what technical advisers could achieve in certain favorable conditions. The fact that the adventurers were "marginal men" who had, for a variety of reasons, run away from their own societies–French, Greeks, Americans; the fact that they were often quite uninhibited by their national and religious feelings (Laborde's guns repulsed a Franco-British landing on the shores of Madagascar in 1845, and the most terrible persecution of Christians took place while he was an influential adviser at the Malagasy court); that they acted on a strictly mercenary basis–all this did not alter the fact that as vehicles of change and innovation, and as promoters of modernization, they were superbly successful. In a country without a monetary system or a paid administration, they were able to mobilize large capital for investment. In a society without an educational system, they were able to create a framework of technical training which provided hundreds of indigenous skilled workers and craftsmen. In a kingdom which lived secluded from the rest of the world, which regarded religious changes and foreign influence as dangerous evils, they were able to persuade the governing class to spend millions abroad to purchase the tools which would create industrial and agricultural enterprises, some of which present-day Madagascar has not yet been able to achieve. The price, in Malagasy human labor and suffering, was enormous, but the results were spectacular. The answer to the question of how they did it, can thus be as relevant to modern planners of technical aid as it has been intriguing to students of Malagasy history.

The fact that these innovators were people who, by choice or of necessity, stood in a certain sense on the margin of their own society and of the society in which they decided to operate, is not in itself an explanation of their ability to act as good or bad vehicles of culture.

The concept of the marginal man as "an incidental product of a process of acculturation", as observed by Robert E. Park,[36] is much less a personality type than an expression of a process. The existence of a hybrid, whether social or cultural,[37] does not in itself help us to understand why the same marginal person who (as in Madagascar and probably elsewhere also) appears at one moment to be a resourceful vehicle of innovation, progress and transculturation, becomes on another occasion a vehicle of stagnation, decay and resistance to transculturation.[38]

There is little difficulty in accepting the thesis that the marginal man is "the key personality in the contacts of culture...the crucible of cultural fusion" and that the "practical efforts of the marginal person to solve his own problem lead him consciously or unconsciously to change the situation itself".[39] Men who are satisfied with themselves and deeply embedded in their own culture do not usually seek to move away, physically or spiritually, from the place in which they have made a nest for themselves. The history of transculturation is also the history of migration, peaceful or otherwise, of men and ideas. But this again does not yet help us to understand why some "marginal" individuals are better vehicles of culture than others.

The link between marginality and effectiveness in the process of transculturation is misleading because it overstresses the psychological and individual aspect of transculturation itself. This aspect is certainly important, and the story of Madagascar's early attempts at modernization are a case in point. But the role of the Europeans in that country in the 18th century could equally well be described by calling them "amphibious" persons, men who had learned to live equally well in two different worlds. Their psychological motivations were of importance, but what was essential was the ability to adapt themselves to different situations well enough and for long enough to be able to translate the ideas or aspirations of one world into the actions of the other, in a manner acceptable to both.

The ability of a carrier of innovation to act in a way acceptable to himself and to others, is very much the ability of a "man-in-between" to act as a true insider.

This is the role which Georg Simmel has attributed to the "stranger"[40] and which, by an extension of his reasoning, could be opposed by the role of the "foreigner".

The stranger, says Simmel, is the man who "comes today and stays tomorrow"; not the physical wanderer but the "potential" one: "although he has not moved on, he has not quite overcome the freedom of coming and going. He is fixed within a particular spatial group, or within a group whose boundaries are similar to spatial boundaries. But his position in this group is determined, essentially, by the fact that he has not belonged to it from the beginning, that he imports qualities into it, which do not and cannot stem from the group itself."

"The stranger", continues Simmel, "like the poor and like the sundry 'inner enemies', is an element of the group itself. His position as a full-fledged member involves both being outside it and confronting it... Insofar as members do not leave the circle in order to buy [outside products] –in which case *they* are the 'stranger' merchants in that outside territory–the trader *must* be a stranger, since nobody else has a chance to make a living".

Thus, the one prototype of the stranger for Simmel is the trader–trader in goods between closed economic societies, trader in ideas–an apt description for the European in precolonial Madagascar, which fits equally well the more contemporary "carriers of transculturation", whether they are traders in ideology, technical aid experts, expatriates in the underdeveloped world, or expatriate students and trainees from underdeveloped countries in more developed ones. Their motivation for filling the role of middleman, of "keeper of the gate",[41] may in many cases derive from their psychological marginalism, but this is a secondary aspect of the problem: the major problem faced by the man-in-the-middle, irrespective of whether he is "marginal" or not, is his relation to authority.

On this point, too, Simmel has some interesting, though not fully developed thoughts. He notes that the stranger is, "by nature, not an 'owner of the soil'; soil not only in the physical but also in the figurative sense of a life-substance which is fixed, if not in a point in space, at least in an ideal point of the social environment".

Every student of colonial history knows the weight of "ownership of the soil" in the relations between different societies, especially when one of them is stronger than the other. In Madagascar, one of the permanent sources of trouble between the missionaries and the government was the different conception held by both sides of real estate property, particularly when it was a question of a house which served as a church, or of native slaves serving foreign masters.[42]

But the problem of ownership interpreted "as a life-substance" has been overlooked, probably because of its more impalpable nature. The close similarities between the concept advanced by Simmel and the concept of legitimacy, has not often been stressed enough.

The "possession of title or status as a result of acquisition by means that are or are held to be according to law or custom"—to quote the definition in Webster's dictionary, is a firmer possession than that of the soil. In fact, the latter is largely based on the former.

However, there is a more subtle aspect of legitimacy, namely the one which—again to quote Webster's—indicates a "conformity to recognized principles". "Ownership" of rules and principles is less evident but still very relevant to the process of transculturation and to the "stranger" who is involved in this process. For Simmel—but also for the Queen of Madagascar—the "stranger" should not be an "owner of the soil" in the material and metaphysical sense attributed, in this context, to the word "soil". His domain is the domain of "mobility", since he is not restricted to any "soil". His power resides not in ownership but in his objectivity, which "does not simply involve passivity and detachment; it is a particular structure composed of distance and nearness, indifference and involvement... Objectivity is by no means non-participation (which is altogether outside both subjective and objective inter-action) but a positive and a specific kind of participation."

The ability of the stranger to adapt himself to the world surrounding him even if he does not belong to it, also derives from his objectivity. Objectivity, says Simmel, "may also be defined as freedom", namely the ability not to be hampered by prejudices, habits, precedents, as, in fact, an efficient technical adviser should not be.

A stranger is therefore a man who is in-and-out at the same time, whose real force is his double ability: to adapt himself to the mentality of the group within which he operates, and when necessary to persuade them that his acts and decisions are consonant with their accepted concepts of their own legitimacy—or "possession of the soil".

As against this "stranger" there is another "wanderer" whom Simmel does not take the trouble to define. He can be identified quite easily by putting him in apposition to the "stranger", namely, the "foreigner". The "foreigner" could be described as the man who "came yesterday and goes away tomorrow": a man for whom "possession of the soil" is of primary concern. Further, when he acts to achieve possession–in the wider sense described above–he does so in accordance with his own legitimacy. Force, not persuasion, partiality, not objectivity are his prerogatives. He is either in or out of society. The obvious prototype of the foreigner is the conqueror, but although force is an obvious ingredient in the foreigner's behavior, it need not necessarily be physical, military might.

In the extreme case of the slave, it is the lack of force which makes him a foreigner to the society in which he lives, in the same way as any "object", however dear, is foreign to the subject who possesses it, by virtue of its own nature.

Between these two extremes, the conqueror and the slave, there are many grades of alienation which fit the definition of "foreigner" better than "stranger".

The first British missionaries in Madagascar certainly acted as strangers: they introduced a lot of innovations, but kept their final aim–religious conversion–for themselves. The British military and political advisers also come into the "stranger" category: the drill they introduced into the army was British, but the disciplinary code remained Malagasy:[43] the battle plans were British, as were the organizational solutions to logistic problems, but the decisions remained Malagasy.[44]

As for the adventurers, up to the late 1850's they were prototypes of the stranger described by Simmel: in-and-out of the native society, fully accepting the legitimacy of its authority (though applying to its interpretation a logic different from that of the natives) and never attempting to impose their own moral principles together with their suggestions for innovation. When the two fitted together, so much the better: when they did not, as in the case of openly organizing their own religious life,[45] they found adequate – even if hypocritical – solutions.

These individuals,–missionaries, political and military advisers, adventurers,–during the latter part of their activity in Madagascar turned from strangers into foreigners. The British missionaries and the British Political Agent Dr. Lyall were kept in check by the Malagasy and then expelled without major difficulties for the Malagasy. The adventurers were not kept in check. They grew so strong and powerful that their expulsion in 1857 was soon followed by their return (in 1861, together with the missionaries), and in their wake came the political decay and finally the political conquest of Madagascar.[46]

In 1818, the second British Agent to Tananarive, Capt. Lassalle, and the first British missionary, did not consider even for one moment kneeling in front of King Radama I, as they would have knelt, if knighted, to their own, British sovereign. In 1830, wives of British missionaries were fighting among themselves to get an invitation to tea with the Queen and proudly displaying the little gifts received from her. In 1862, the missionaries' wives were causing the British Consul a lot of trouble by refusing to get out of their palanquins when crossing the path of the Queen who, because she was making a pilgrimage to a holy site outside the town, demanded that her subjects clear the streets for her passage.[47]

The borderline between the stranger and the foreigner thus appears to be far more a matter of the objective relationship between innovation and authority than of the subjective relation between the carriers and the recipients of innovation. No less important for a clear understanding of this borderline is the legitimacy of authority itself, in the particular field in which such authority expresses itself. Force can impose or reject innovation, but only legitimate authority can elicit imitation.

Clearly, such legitimate authority can exist in a given field of society and be lacking in another. In the Middle East, for instance, while foreign domination was equally repulsive to both Christian and Moslem Arabs, the educational and cultural authority of the West looked far more legitimate to the Christian Arabs (who, in most cases, led the political revolt against the West) than to the Moslems, who rejected foreign cultural domination as much as foreign political rule.

For the European and Malagasy innovator in 19th century Madagascar, the legitimacy of the authority under which they operated, its splintering and regrouping by and around the political rule, in terms of military, educational, religious and economic legitimacy, became the key to the innovators' almost cyclical successes and failures, quite independently of the personal, psychological tendencies of the innovators themselves.

The study of this brief precolonial adventure in innovation can thus still be of considerable relevance to the contemporary student of development—from both societies—as well as to the student of history.

FOOTNOTES

1 Sonia Howe, *L'Europe et Madagascar.* (Paris: 1936); G.S. Chapus, *80 ans d'influence européenne en Imerina.* (Tananarive: 1925); E. W. Ellis, *History of Madagascar,* 2 vols. (London: 1838). Much of the relevant material concerning the modernisation of Madagascar lies still unpublished, in the archives of the London Missionary Society,

London, and in the Royal Archives at Tananarive, writtten in Malagasy, French and English. The two major documents from Malagasy sources, dealing in detail with European activities in Madagascar in the early 19th century are: *Tantaran ny Andriana* (History of Kings), trans. by R.P. Callet, 4 vols. (Tananarive: 1953-58), and *The Raombana Ms.* (written in English) under publication by Prof. Simon Ayache, Tananarive University, mainly vol. II for the period 1810-1828. For a more general history see: R. Coupland, *East Africa and its Invaders.* (Oxford: 1938), pp. 438 ff., and H. Deschamps, *Histoire de Madagascar.*

2 G. Ferrand, "Les Voyages des Javanais a Madagascar." *Journal Asiatique, 910,* 281-330; and Alfred et Guillaume Grandidier,*Collection desouvrages anciens concernant Madagascar* (1903-1920), mainly vol. IX, Flacourt: Relation de la Grande Ile de Madagascar (1624-1660); G. Grandidier, *Histoire politique et coloniale* (de Madagascar), 3 vols. (Paris: 1958), vols. I and II, Histoire des Merinas; J. B. Razafintsalama, *La langue malgache et les origines malgaches,* 2 vols. (Tananarive; 1928-29).

3 No detailed studies have yet been made of this extraordinary personality. The best attempt is J. Chauvin, "Jean Laborde", *Memoires Acad. Malgache* (1939), which does not, however, attempt to analyze the social and political impact of Jean Laborde's work on Malagasy society. Many relevant remarks on this aspect can be found in P. Boiteau, *Madagascar, Contribution a l'Histoire de la Nation Malgache* (Paris: 1958), pp. 63-121, where despite the author's determined effort to apply Marxist interpretations to Malagasy events, the impact of European influence on Malagasy society is underlined from the sociological and economic, more than from the political viewpoints.

4 A detailed report on the early stages of the tug-of-war between French adventurers, British missionaries and Malagasy politicians for the economic control of Madagascar, including the secret correspondence between Radama II and the French, is contained in Baron P. de Richemond,*Documents sur la Compagnie de Madagascar – Precede d'une Notice Historique* (Paris: 1887). See also F. H. Bonnafoy de Premont, *Rapport à l'Empereur sur la Question Malgache et la Colonisation de Madagascar* (Paris, 1850). For the later part of the negotiations concerning the attribution of economic rights to the French and the British at the time of Radama II, much relevant material, still unpublished, is contained in the despatches of the first British Consul in Tananarive, J. B. Packenham, to the British Foreign Office, Public Records Office (P.R.O.), F.O. 49/9, 48/10, 1961, 1962, 1963.

5 G.S. Chapus et G. Mondain, *Rainilaiarivony, Un Homme d'Etat Malagache* (Paris: 1953).

6 An interesting insight into the result of the negotiations which took place between the Malagasy and the L.M.S. concerning the conditions under which the Malagasy students would stay in England, is found in the corrected draft of the memorandum prepared by the Mission for the British Government, in L.M.S. Archives, Madagascar, Box 1, Folder 4, document 70. Para 3 states:

> "The object [of training] is understood to be to give to the youth a plain English education including a strict attention to the inculcation to religious and moral principles and (when qualified to enter upon it) instruction on certain mechanical arts, and some branches of science, according to a minute to be handed to the Society by Mr. Harrison".

Article 6 envisages that

> "the expenditure [for the trainees] will be of an economical kind as it regards Board, Lodging and Clothing, it being understood that it is in habits of industry and

frugality, as Artificers, that they are to be brought up; but it will be necessary, considering their former stations and for the purposes of health, that this clothing and diet be somewhat liberal."

The contribution which the L.M.S. required from the British Government for the upkeep of each Malagasy trainee was £52-10/- per annum, which did not cover the cost of education, this being borne by the Society itself. Article 9 provided that additional payment was to be asked from the British Government for "initiation into the arts or trades to which they are destined", adding that "the rank which these young students held in their own country will make it proper that situations more select than those of common English apprentices should be found and that their clothing and maintenance should be superior in comfort than that usually enjoyed by such persons". Finally, Article 10 states that "when the youths are sent back to their own country after the end of their residence in England has been adequately effected, they shall be furnished, at the expense of the Government, with such tools and instruments and books as the Society shall judge necessary for the communication of the knowledge they have acquired to their countrymen".
The document is undated but was presumably written sometime in 1823.

7 R. P. Callet, *op. cit.* and P . Boiteau, *op. cit.*, pp. 45-61.

8 P. Boiteau, *op.cit.*, ch. 9; F. Standing, "The tribal differentiations of the Hova Malagasy", *Antananarivo Annual, 11* (1887).

9 H. Deschamps, *op. cit.*, pp. 51-53 and 92-93.

10 See R. P. Callet, *op. cit.*; G. Julien, *Institutions politiques et sociales de Madagascar,* 2 vols. (Paris; 1908), especially ch. IV, L'oeuvre politique et administrative de Andreananpuinamorina (1787-1810), pp. 172 ff.

11 J. Valette, "Etudes sur le regne de Radama I", *Bulletin de Madagascar, 187* (December 1961).

12 See Le Pere de la Vaissiere, *Histoire de Madagascar* etc., (Paris: 1883), vol. I, pp. 27 ff. Also G-S. Chapus and G. Mondain, *L'action Protestante a Madagascar* (Tananarive: 1938), p. 12.

13 One was the Rev. David Griffith, who dissolved his association with the Society in 1834, going back to England for a while and then returning to Madagascar for purposes of trade. The other one was the Rev. David Johns, who never interrupted his missionary activities and was, in fact, the only missionary allowed to return – as a visitor – in 1841, at the height of the anti-Christian persecution.

14 W. Ellis, *Three visits to Madagascar* (London: 1859).

15 For the full list of British missionaries and their families in Madagascar, from 1818 to 1836, see: *Madagascar and its Martyrs – A Book for the Young, by the Author of Missionary Stories etc.* (London: 1842), pp. 18-19, and *Register of L.M.S. Missionaries.*

16 Born in Perthshire, Scotland, in 1800, he sailed for Madagascar in 1826 as the official carpenter of the Mission. His activities extended well beyond his job: he set up the first cotton mill in Madagascar at Amparibe as well as a printing press. It was mainly due to the desire of the Hova Government to keep him at the capital that permission for the continuation of missionary activities was granted from 1829-35. Cameron taught the Malagasy how to make soap, a manufacture which so much impressed them that it was probably a decisive element in obtaining for him permission to stay in the country. After leaving Madagascar in 1835, he established himself at Capetown and returned to Madagascar as a delegate of the Chamber of Commerce of Mauritius, to negotiate with the Malagasy government for the renewal of trade with Europeans in 1853. After

successfully completing his mission, he went back to the Cape Colony and returned for the third time to Madagascar with Bishop Ellis in 1863, to supervise the erection of the Memorial Churches for the Martyrs of Madagascar. He died in Tananarive in 1875.
17 A description of the material, social and spiritual situation of the British missionaries in Polynesia, is made, with great richness of detail and insight, by W. N. Gunson, in: *Evangelical Missionaries in the South Seas, 1797-1860.* (Doctoral Thesis, The Research School of Pacific Studies, The Australian National University, 1959,) well applies also to the small Mission in Madagascar. As for the social origins of the missionaries and their "stated motivations" for embarking on "missionary life among the savages", much relevant material can be found for at least some of the missionaries who worked in Madagascar in: *L.M.S. Candidates' Papers.* These records are, unfortunately, not complete, due to the losses suffered by the Archives during the bombing of London.
18 See W. N. Gunson, *op. cit.*
19 The case of the transliteration of the Malagasy language is a famous and illuminating one. For several months the then Head of the Mission in Tananarive, the Rev. David Jones, a Welshman, fought a bitter fight against the Rev. John Jeffreys, an Englishman, over the correct transliteration of the sound "oo". Jones wanted it represented by a "w", while Jeffreys, a better educated man with a socially ambitious wife, insisted that the transliteration be made with a double "o". The fight, which was carried on with incredible fierceness, ended in a Court of Inquiry held by the British Agent, who had to investigate the reason for Rev. Jeffrey's request of an audience with the King to persuade him that Rev. Jones' suggestion, was based on the Rev. Jones' tribal attachment to a low caste British tribe who use "a mean language, which is Welsh". The King, who was quick to discern the personal and social animosity underlying the quarrel, finally and solomonically fixed the transcription of Malagasy language by decree, ordering that some of the vowels and diphthongs be transliterated according to French grammar and others according to English. L.M.S. Box 1, Folder 4, Jeffreys to Jones, 1st April, 1823, and Jones and Griffiths to? (illegible) 24 April, 1823.
20 The case of the Rev. W. Griffith is a typical one. He amassed a considerable fortune in Tananarive, becoming one of the main moneylenders in the capital, a situation which he did not want to abandon when recalled to England by the Directors of the L.M.S. His relations with other members of the Mission were so bad that he involuntarily became instrumental in supplying to the Malagasy many of the accusations which were later used to justify the expulsion of the missionaries from the country. To a considerable extent he was probably also instrumental in worsening the relations between the British Agent, Dr. Lyall, and the Malagasy authorities. L.M.S. Archives, Madagascar, Box 3, Folder 1; Minutes of the meeting of the Mission members, 2nd February, 1829; and L.M.S. Archives, Madagascar, Cameron to Arundel, Box 4, Folder 2, April 11th, 1832, Johns to Arundel, April 15th, 1832.
21 Lyall to Jones, L.M.S. Archives, Madagascar, Box 3, Folder 1, August 13th, 1828.
22 *Le Journal de Robert Lyall,* translated by G-S. Chapus and G. Mondain, Academie Malgache, V. (1954), Lyall's entry of February 2, 1829, p. 170.
23 The missionaries were naturally a very important source of political information for the British Government and in some cases the only available translators of official documents and conversations. Some of the reports they were asked to send back to the L.M.S. in London were, in fact, detailed answers to political questionnaires. See, for instance, the unsigned questionnaires on Malagasy politics and religion, probably dated end 1832, L.M.S. Archives, Madagascar, Box 4, Folder 1. In some instances the

missionaries far exceeded their religious role. This seems to have been the case with the Rev. Freeman, a leading member of the Tananarive Mission, who apparently had no small share in the responsibility for the failure of the Malagasy diplomatic mission sent by Queen Ranavalona to London in 1836-37 to try to re-establish relations with the British Government but without restoring missionary activities. See: Freeman to Palmerston, February 4, 1837, February 23, 1837, March 18, 1837, P.R.O., F.O., 48/1. In this instance it is interesting to note that although there are abundant proofs, in the Foreign Office correspondence of the time, of the political initiative taken by the Rev. Freeman in connection with the Malagasy missions, no trace of them or of this correspondence related to them can be found in the archives of the L.M.S. itself, the Directors of the Society being probably aware but unwilling to share any responsibility for the political initiatives of Rev. Freeman.

24 As I said above, it is not impossible that many of the accusations made by the Malagasy against the British Agent, Dr. Lyall were – at least in part – suggested to the Malagasy by the Rev. Griffith himself. See: David Jones to W. Orme, Foreign Secretary, L.M.S., from Port Lewis, Mauritius, 16th September, 1830, Box 3, Folder 4.

25 The missionaries' idea was to establish a Society for the advancement of education among the Malagasy which would draw its income directly from patrons in England and thus add to the income of the Mission, over and above what they were receiving from L.M.S. Headquarters in London and also – for a while – from the British Government. The King agreed in the first instance to be the President of the Society and also agreed that some of his generals should join it as well. He became very cool, however, when he learned that the Society's decisions would be taken by the Board on the basis of a majority vote. "For a proposition to be carried out by a majority in a Society in which he [the King] belonged would not do in this country, where the word of the King is law". Jones & Griffith to Burder, L.M.S. Archives, Madagascar, Box 2, Folder 2, 19th December, 1825.

26 This is somewhat different from what happened in other non-European countries, where Western techniques were far more appreciated than the metaphysical and cultural framework which produced them. This may be due to the deep religious character of the Malagasy society. A modern analysis of their religious attitudes which throws much interesting light on the reaction of the Malagasy to the European spiritual impact, has been written by a religious scholar in Tananarive, the Rev. Richard Andriamanjato, *Le Tsiny et le Tody* (Tananarive, 1957). The author, who is a leading progressive, Marxist political figure in Madagascar, is significant also a descendant of the first Christian aristocratic families of Madagascar, who were involved in the modernization of the country in the 19th century.

27 An idea of what the Malagasy thought of the missionaries as a political force can be obtained, *inter alia,* from the report sent by Johns to Arundel, L.M.S. Archives, Madagascar, Box 4, Folder 2, April 15, 1832, and D. Johns, J.J. Freeman, J. Canham to W. Ellis, May 28, 1833.

28 This is not the place to elaborate on this topic which will be discussed in detail in a forthcoming paper written in collaboration with Y. Elkana, the Hebrew University of Jerusalem.

29 Freeman to Hunkey, L.M.S. Archives, Madagasar, Box 4, Folder 2, June 10, 1832, about behavior of three of the Malagasy trainees who had returned from England. The behavior of the European-trained Malagasy intellectuals of those days seems to me to be

an early example of the behavior of many contemporary native intellectuals, who are supposed to assist the European advisers in their work of transculturation, but cannot overcome their hostility to them. The frustrated asisstant technical advisers, the over-confident, emotionally unsatisfied Peace Corpsmen, the reticent native officials parachuted into jobs for which they are not prepared and the responsibility for which they do not want to take – all seem to reflect the state of mind, the inhibitions, the self-destructive and pessimistic approach which appears in the "men-in-the-middle" of old Madagascar.

30 Until the British Agent was expelled, Brady, already a General in the Malagasy army, received his Sergeant's pay from the British Paymaster in Mauritius, despite the fact that he was no longer a serving soldier and had taken up Malagasy citizenship.

31 The complicated personal relations between Hastie and his superiors in Mauritius are well illustrated by the difference in the tone of the correspondence he conducted with the British Governor, Sir Robert Farquhar, and the Acting Governor, General Gage Hall. With the former, a civilian who had a keen appreciation of Hastie's qualities and did not hesitate to appoint him British Agent in Tananarive despite his Sergeant's rank, Hastie exchanged deferential but very cordial letters. See, e.g. Hastie to Farquhar, 3rd June, 1818, P.R.O., C.O. 167/51. On the other hand, the very fact that Hastie was a Sergeant was sufficient reason for General Hall, a rigid disciplinarian, to describe him as a second rate person and not to take his political advice into consideration. Hall to Bathurst, June 26, 1818, P.R.O., C.O. 163/39.

32 Raombana Ms., *op. cit.*, vol. II, p. 95.

33 The accusations and counter-accusations between the missionaries and the British Agent are contained, *inter alia,* in the reports of a Court of Inquiry set up to investigate the charges of Dr. Lyall against Rev. Griffith, giving a summary of the correspondence exchanged between the Agency and the Mission during the period February 31, 1831 and November 2, 1831. L.M.S. Archives, Madagascar, Box 4, Folder 3.

34 *Le Journal de Robert Lyall, op. cit.* Entry of February 2, 1829, p. 171.

35 Fontoynont et Nicol, *Memoires de l'Academie Malgache,* XXXIII (Tananarive, 1940).

36 Robert E. Park in the introduction to: Everett V. Stonequist, *The marginal man – a Study in Personal and Cultural Conflict*(New York: 1961), p. 18.

37 See Stonequist, *op. cit.,* chs. 2, 3 and 4, dealing with the racial and cultural hybrid, as well as Professor Joseph Ben-David on the role of the social hybrid in scientific discovery. *Minerva,* IV:1 (Autumn, 1965).

38 The preliminary results of a study carried out on the motivation of Israeli technical assistance operators overseas between the years 1958-1968 show that there is no apparent relationship between the feeling of social marginality of the operators themselves and their success or otherwise in their work as vehicles of transculturation. I am grateful for this information which I received from my student, Mrs. Naomi Chazan, who is working on problems of African modernization as a part of her doctoral thesis at the Hebrew University.

39 Robert E. Park, *ibid.*

40 *The Sociology of Georg Simmel,* ed. and trans. Kurt H. Wolff (New York, 1950), pp. 402-8: "The Stranger".

41 I am grateful to Professor Robert Merton for suggesting this description of the man who stands between two cultures, and also for the many helpful suggestions he

generously gave me for the correction of this part of my paper.

42 According to Malagasy ideas, all land and buildings belonging to the missionaries remained the inalienable possession of the King. Also their slaves, if freed by the missionaries, had to revert to the King.

43 Raombana Ms.,*op. cit.*, vol. II, pp. 107, 108 ff.

44 *Ibid.*

45 De la Vaissiere, *op. cit.*, vol. I, ch. 8.

46 It is interesting to note that one of the claims used by the French to justify the occupation of Madagascar was the dispute over the estate of Jean Laborde, the very man who, more than any other European, contributed to the modernization of Madagascar and who had himself become a Malagasy citizen of high standing. Deschamps, *op. cit.*, pp. 182, 186.

47 Pakenham to Earl Russell, 1st December, 1862, P.R.O., F.O., 48/9; Pakenham to Ellis, April 25, 1863, P.R.O., F.O. 48/10, and Pakenham to Earl Russell, June 13, 1863, P.R.O., F.O. 48/10; Pakenham to Earl Russell, March 28, 1865, P.R.O., F.O. 48/14, report No. 7 on the behavior of the missionaries.

Japanese Culture and the Problem of Modern Science

JAPANESE CULTURE AND THE PROBLEM OF MODERN SCIENCE

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I. SCIENCE, CULTURE AND SOCIAL STRUCTURE

Japanese Perspectives

On November 22, 1901 professors and students of the Tokyo Imperial University Medical School held exercises honoring Dr. Erwin Baelz on the twenty-fifth anniversary of his appointment to the faculty. Seemingly ignoring the purely ceremonial nature of the occasion, the German physician chose to unburden some of his accumulated frustrations in replying to the greetings of his Japanese colleagues and commented as follows on the state of science in Japan at that time:

> It seems to me that in Japan erroneous conceptions about the origin and nature of Western science are widely prevalent. It is regarded as a machine which can turn out so much work every year....which can without further ado be transported from the West to any other part of the world, there to continue its labors. This is a great mistake. The Western scientific world is not a piece of machinery, but an organism and like every other organism, if it is to thrive, it needs a particular climate, a particular atmosphere. [1]

Baelz especially criticized what he thought was a tendency of the Japanese to seek the "latest acquisitions of science, instead of studying the spirit which made the acquisitions possible."[2]

The retiring German professor was saying, in effect, that science in Japan despite thirty years of sustained growth had remained epiphenomenal and largely uncreative, continuing to rest lightly on the surface of a culture indifferent to its values and impervious to its influences. A historian taking note of these strictures several decades later might well be disposed to regard them as the angry words of a man displeased at being eased out of an agreeable position to make room for a Japanese. Indeed, they may have been just that. There is, however, one very important reason for treating the German scientist's remarks as something more than an expression of personal resentment. That is the remarkable extent to which succeeding generations of Japanese scientists have themselves bemoaned the supposed lack of creativity in prewar science and its "peculiar" relationship to society and culture.

Modern critics have leveled four principal accusations against prewar science, of which one involves the old issue of "copying." The exponents of this thesis, who stress the key role of the government in the promotion of science, have claimed that high-level government officials had little interest in scientific research and preferred to promote science by importing knowledge, techniques and skills from abroad. The government's awareness of science in the words of one scientist, was dominated by an "illusion of copying everything;"[3] and to that extent it failed to understand science correctly or support it adequately.[4]

Others assert that traditional Japanese ways of thinking were so irrational as seriously to inhibit the development of scientific thought. Dr. Nakamura Hajime, a leading exponent of this thesis, has said certain features of the Japanese language encouraged irrationality. He observes, for instance, that Japanese has no established method for creating abstract nouns, lacks the infinitive form of the verb and the relative pronoun "which," and tolerates frequent changes from one grammatical construction to another within the same sentence.[5] Others who emphasize the influence of "irrationalism" attribute its persistence to attitudes toward nature,[6] tradition-inclined ideological movements in the Meiji Period (1868-1912)[7] and an excessive concern with practical social and political needs by early Japanese scientists which obscured their awareness of the need for intellectual reforms.[8]

Yet a third group claims the "peculiar" nature of the institutionalization of Japanese science results from the historical fact of its having entered the country "separated from industry." One interpretation of this view, expressed by the Japanese Communist Party and Kobe University physics professor, Dr. Yuasa Mitsutomo, among others, states that science's lack of a well-developed, independent base in Japan's private industry made it excessively dependent on support by the government and thus vulnerable to various kinds of state "interference."[9] Yuasa suggests such interference was possible because Japanese scientists had no tradition of resisting authority as European scientists did. But other critics who stress the relationship of science to industry as an important factor in its later development say Japanese science in any event lacked a tradition of scientific thought sufficiently powerful to resist the government's "semi-feudal absolutism" successfully.[10] They attribute the weakness of scientific thought in Japan to the timing of the Meiji Restoration in world history on the assumption that science and technology are largely a function of economic development. In their view, consequently, because Japanese capitalism in 1868 was so backward compared to European capitalism, the state had to intervene on its behalf and in doing so necessarily caused technology and science to become dependent on itself as well. Such dependence is then said to have "distorted" their development.

Finally, it is held that either the formal organizational arrangements[11] within which science existed or the behavior of scientists themselves were responsible for certain alleged shortcomings. In particular senior scientists are charged with perpetuating a "feudalistic apprentice system" of recruitment and socialization in dealing with students and younger colleagues, exercising such control over them that free exchange of views within the research group and cooperation with members of different groups were obstructed or even precluded.[12] Whether these practices occurred with greater frequency than could have been the case in Western countries is not an issue for these critics as all assume that to be true. Disagreement focuses rather on the question of whether the motivations of prewar scientists were "improper" to begin with, or alternatively, whether these motivations became deviant because certain formal organizational arrangements channeled them in certain ways. Considering the large number who believe the university chair system (*koza seido*) had adverse effects on research, it appears that most favor the latter possibility.[13] Yet even among those attributing principal blame to organization, ambivalent tendencies toward the other point of view persist. This is seen, for instance, in the writings of Dr. Sakata Shoichi, a leading physicist at Nagoya University who has been among the most influential critics.

In 1947 Sakata published an essay called "Research and Organization" in which he criticized senior Japanese scientists for allegedly obstructing cooperation and criticism but claimed the chair system was ultimately responsible for these problems. However, he went on to make the interesting claim that a new form of organization introduced at Nagoya University had substantially reduced such practices in his department.[14] The innovation to which he referred was the laboratory council system proposed originally in 1939 by the British physicist, John Desmond Bernal.[15] At Sakata's instigation a laboratory council was established in the Nagoya University Physics Department and every researcher given an equal voice in it, diminished but slightly by a democratically elected chairman. The stated objective was to encourage a "democratic spirit" among the researchers and this had largely been achieved after a one-year trial, according to Sakata, as even the chairman's ideas had received their share of criticism from the younger physicists.[16]

Despite the optimism of these early claims it should be noted that later participant-observers took a much less sanguine view of the new reform than Sakata had done. In 1963 two of his colleagues, Dr. Otsuki Shoichiro and Dr. Nojima Tokukichi described the laboratory council system as follows:

> Whenever the [social and cultural] forces to support it were lacking, the new system even became a means for concealing the contradictions within the old system. One may question whether the [formal] signs of democratization actually brought into being conditions encouraging either greater freedom for the individual researcher to develop his abilities or more effective cooperation among researchers. The laboratory council system retains within itself the perpetual danger of degenerating into the chair system compared to which it represents only a change in form.[17]

Sakata himself had been aware of this possibility, however; for in the 1947 essay he wrote: "Democratization of research organizations will be extraordinarily difficult without democratization of the entire society." While not wishing to pursue this point just yet, it may be suggested that in this case at least, changes in organizational arrangements apparently did not have the impact on interpersonal relations which some sociological theory would presumably have predicted.

When each of the preceding arguments is carefully analyzed three basic conclusions emerge. Two are reasonably self-evident and are universally accepted among the Japanese critics mentioned here: that science in Japan was not very creative in the prewar period; and that science remained culturally epiphenomenal in the sense that certain social and cultural values associated with its presence elsewhere were not accepted in Japan within either the scientific community or the general society. The third conclusion, by contrast, is much less obvious and is not universally shared. It would hold that science, so far as it was epiphenomenal remained uncreative; and that to the extent it may have been uncreative, remained epiphenomenal. Such a conclusion necessarily follows from arguments advanced by the first and fourth groups of critics but is not implicit in those of the other two, though many of them do believe Japanese science was uncreative nevertheless. This is a point of some importance to which the discussion will return later on.

The question remains, however, whether any of these conclusions should be accepted. It cannot be conceded at the outset, for instance, that Japanese science was epiphenomenal in this sense. Such a proposition, if true, could only be established by certain comparisons between science in Japan and science elsewhere. Accordingly, the question of whether Japanese science in the prewar period was really epiphenomenal or not will be the first to be taken up in the following discussion. Later it will be suggested that science in prewar Japan was in fact epiphenomenal, a conclusion which leads to the second question: why? The third question, then, will focus briefly on creativity. No attempt will be made to evaluate prewar science either in general or in any of its specific manifestations. Rather, the question considered will be of the form, to the extent Japanese science was uncreative in some demonstrable way, could any of the arguments described earlier contribute substantially to an acceptable explanation?

These questions will be considered within the framework of a specific case study, the institutionalization and development of the biomedical sciences in Japan, especially bacteriology, during the latter part of the nineteenth century and the first two decades of the twentieth. In order to establish certain points in the argument specific comparisons will be made with the situation in Germany during the same period. But before proceeding directly to the case study it is necessary first to discuss in greater detail several aspects of the charges leveled against prewar science by its critics and to point up their relationship to a more comprehensive body of sociological theory.

The Functionalist Perspective

It is often argued, as indeed most of the Japanese critics cited here do, that scientific research requires the maintenance of certain *values*[18] in whose absence science cannot function. In accord with this view modern functionalist sociology has defined science as a social system integrated by these value orientations and even claimed their maintenance within the scientific community explains the persistence of science through time.[19] The best-known definition of these value-orientations is probably Professor Talcott Parsons' pattern-variable scheme in which science is said to require adherence to the values of universalism, achievement, functional specificity, affective neutrality and collectivity orientation.[20] Moreover, Parsons would say, the practice of science must by *definition* be characterized by adherence to these values at three levels of generality. In decreasing order these may be described as the level of philosophical or epistemological orientations, the level of institutional or organizational norms and, at the lowest level, personal value commitments.[21] Within this analytical framework, of course, the term "institutionalization" of science refers primarily to inculcation of the necessary values at all three levels.[22]

This approach to science has certain implications for the view functionalists adopt concerning the historical relationship of science to culture and social structure. If it is true, in other words, that science cannot flourish without certain values which, for the sake of this discussion are assumed to be at least similar to the above-mentioned pattern-variables, one can only conclude that in the culture or society "receiving" it, science in varying degrees will replace or destroy those values which conflict with its own. From a functionalist point of view science could be said to produce "standardized contexts of experience"[23] in every society or culture where it becomes established, a hypothesis which Kenneth Downey has termed the Destruction Theory of the institutionalization of science.[24]

Arguments advanced by two groups of Japanese critics seem to define science precisely as functional analysis has done and thus subscribe necessarily to the Destruction Theory of its relationship to culture and social structure. Their contention in essence amounts to saying that science "ought" to have destroyed certain aspects of the traditional culture and social structure but failed to do so because certain basic values associated with science in other countries were not accepted in Japan. Consequently, the process by which science became established (not "institutionalized") there was somehow improper, deviant or peculiar. The implication of the copying thesis is that this occurred because the government's policies precluded the necessary socialization in research. The argument about formal organization, on the other hand, suggests the chair system was to blame because it gave senior professors too much power and restricted horizontal movement between research groups; while the argument about behavior merely says the requisite values were not institutionalized without providing any explanation.

In response, this paper will endeavor to show that these arguments are incorrect, that the assumptions on which they are based are dubious and that they are incompatible with the empirical evidence.

Critics emphasizing the importance of irrationalism, language and historical factors, on the other hand, seem to accept quite a different set of theoretical assumptions while reaching similar conclusions about the epiphenomenality and uncreativity of prewar science. Of strategic importance for all of their claims is a definition of science as *ideas* rather than one based on *values.* Moreover, there is an implicit assumption that ideas under certain conditions may be regarded as independent variables in socio-historical analysis. Thus some areas of Japanese science might have been uncreative in their view because of linguistic inadequacies while science in general could have remained epiphenomenal due to certain historical factors or failures of *ideology*.

The position taken in regard to these arguments will be that they are essentially valid in so far as they lead to a more accurate empirical description of Japanese science in its relations to culture and social structure and help substantially to answer the three questions posed earlier. While not necessarily accepting the conclusions they reach about creativity, their basic assumptions seem potentially compatible with a different conclusion and at the same time capable of providing valuable clues to any potential answer to the third question.

It may be immediately objected, of course, that functionalism also affords *ideas* a high degree of autonomy or independent variability in the creation, though not the application, of other scientific ideas. Indeed, Parsons said precisely that in a 1938 essay.[25] In practice, however, functionalists have invariably proceeded from institutional structures to cognitive ones in their analysis of science, explaining the creation of scientific ideas as a function of certain value orientations and formal organizational arrangements, a procedure which seemingly denies the very autonomy with which scientific ideas are said to be endowed.[26]

Functionalism's explicit assumption that values can be regarded as non-problematical and given appears to be the principal source of this contradiction. In Professor Parson's words: "The whole nature of the theory of action in general is....such that precisely with respect to variability of structure, patterns of value orientation as the focus of institutionalization must play a crucial role."[27] Moreover, functional analysis displays notable inconsistency in its use of the value-orientation concept. In sharp contrast to the levels of philosophical or epistemological orientation and institutions where values or norms are treated as the independent variable, at the level of actual human behavior values become a product of interpersonal relations which, in turn, are partially a product of formal organizational arrangements – being thus regarded as a dependent variable. (Although the concept of functional equivalents resolves this contradiction in certain cases, it is not relevant to the particular issue this paper will raise – whether certain allegedly fundamental values are needed at all below the highest level of generality). Nor is such confusion merely the result of using one term where another should have been introduced; since the pattern-variable scheme by *definition* covers all three levels. This assumption, it is argued, leads to conclusions about the relationship of science to culture and social structure which are unsound in general. Moreover, in the Japanese case particularly it will be asserted that functionalist assumptions produce conclusions about the relationship of science to culture which are demonstrably inconsistent with the empirical evidence.

How can the validity of these arguments be demonstrated? Because of their basic assumption that values are given and non-problematical at all levels of generality, functionalists have typically stressed the role of formal organizational arrangements in either facilitating or inhibiting the ability of individuals to act in accord with the values of science. While the possibility that more than one behavior pattern can uphold the necessary values is not only admitted but even insisted upon, there are clearly limits to the amount of possible variation. Ultimate failure to uphold the values of science, of course, means by definition that creative science cannot be done.

Among the most provocative studies incorporating functionalist assumptions are those of Joseph Ben-David and Awraham Zloczower concerning the biomedical sciences in nineteenth-century Germany. They have argued in various papers[28] that the formal organization of German academic medicine exercised an inhibitory influence on the productivity of medical research there and did so by encouraging inappropriate patterns of behavior among scientists. It is interesting to note that the eminent Japanese bacteriologist, Kitasato Shibasaburo (1853-1931), trained in Germany at that time and well acquainted with conditions there, made precisely the same argument with reference to the Tokyo University Medical School (and the institutions modeled on it) in Japan, claiming that similar patterns of behavior among biomedical scientists in the two countries resulted from the same cause. The studies of Ben-David and Zloczower concerning Germany and Kitasato's arguments concerning Japan therefore permit a comparative analysis of formal organizations and of patterns of behavior among biomedical scientists in each of the two countries within the organizational context. Both because Kitasato's arguments focused on bacteriology and for reasons to be explained momentarily the analysis will be confined almost entirely to that discipline.

Based on this analysis two arguments will be advanced. First of all, formal organizational arrangements concerning science in Japan were, if anything, considerably more flexible than those in Germany and ought to have elicited very different patterns of behavior than those which actually existed if the basic assumptions of functional analysis were correct. Their failure to do so, of course, means that different value orientations existed in Japan than existed in Germany at both the institutional and personal levels. From a functionalist perspective this conclusion means that science in Japan was not only culturally epiphenomenal in the sense defined earlier but by definition was not science at all! However, and this will be the second argument, the research done by the Japanese scientists in question seems to have suffered not at all from the effects of whatever values the biomedical science community maintained. In fact, the claim will be made that the values existing in the Japanese case were at least as congenial to science as those which functional analysis has claimed are essential.

Procedurally, the following section will present the case for the "epiphenomenality" of prewar Japanese science, that is, that it emphasized certain strategic values markedly different from those of German science; while the reasons for this, together with the creativity problem mentioned earlier,will be discussed in the last section.

II. THE SOCIAL RELATIONS OF BACTERIOLOGY IN GERMANY AND JAPAN

Government and the Biomedical Sciences

Aside from Kitasato's arguments and the researches of Ben-David and Zloczower, bacteriology was made the focus of the case study for two other reasons. A principal aim of this paper is to demonstrate that creative scientific research can be done without adherence to certain values deemed indispensable by functional analysis at all but the highest level of generality. Thus, it was necessary to investigate an area of science in which important work was done by the Japanese; and bacteriology, together with the closely related field of pathology, meets this condition. Another reason has to do with the nature of bacteriology itself. Because it studies the relationship of specific microorganisms to specific diseases, this science has always required an intimate relation between the research and the clinical aspects of medicine. Robert Koch, who was largely responsible for its establishment as a full-fledged science, began his medical career not as a professor in a laboratory but as the District Physician at Wollstein in East Prussia; and Professor Ben-David rightly stresses the importance of the clinical experience for Koch's success as a bacteriologist.[29] Referring to this characteristic of bacteriology, Dr. Abraham Flexner once described it as a kind of "bridge" between the two branches of medical science which in nineteenth-century Germany were gradually driven further apart through the effects of specialization and professionalization.[30] One may reasonably assume, in consequence, that a science of this kind would be especially vulnerable to the effects of changing organizational arrangements on its overall effectiveness.

As bacteriology was the field in which Japanese scientists made the largest number of important contributions to knowledge during this early period it is not surprising to discover that it was among those most securely institutionalized[31] and among the first to become so. While other sciences had but a single institutional base in Japan before 1900, bacteriology had two. In Germany this field emerged as a distinct, theoretically-based discipline within medical science in the mid-1870's due to Koch's pioneering studies of anthrax and particularly his discovery of methods for attaining bacterial cultures in a pure form. Only six years later, in 1881, this new science was being taught regularly at the Tokyo University Medical School and by 1884 had been recognized there by appointment of a full-time lecturer.[32] Interestingly enough, this first faculty appointment was made in bacteriology at Tokyo University a full year before Koch himself became professor at Berlin.[33]

The pioneer bacteriologist in Japan, who held the first chair at Tokyo

University until his death in 1919, was Dr. Ogata Masanori, a graduate of the same institution who also studied four years in Germany, both at Max von Pettenkofer's Hygiene Institute in Munich and at the Imperial Health Office's Pathology Institute in Berlin. He returned to Japan in 1883 and began lecturing at Tokyo University while directing a small laboratory called the Hygiene Institute operated by the Japanese Home Ministry.[34] In 1908 a second chair was added at the Tokyo University Medical School to which a junior colleague of Ogata, Dr. Yokote Chiyanosuke, was named professor. By the time Ogata died, there were also three associate professors of hygiene and bacteriology at the Tokyo University Medical School.[35]

Much of the work in this field at the University was done in Ogata's Hygiene and Bacteriology Section but by no means all of it. The remarkable popularity of these studies in Japan is also attested to by contributions from professors in physiology, anatomy and even surgery.[36] More important were studies carried out in Dr. Aoyama Tanemichi's Internal Medicine Section and the Pathology Section of Dr. Miura Moriharu, Dr. Yamagiwa Katsusaburo and later Dr. Nagayo Mataro. As research in bacteriology was sustained only in these sections, together with the Ogata Section, our analysis of its social relations will be confined to these three groups.

There was also a second major center of bacteriological studies in Japan at this time, more important in some ways than the Tokyo University Medical School. This was the Institute of Infectious Diseases, established originally by Kitasato with private support in 1893. Two years later it became an official agency of the Home Ministry's Bureau of Public Health and in 1899 with Kitasato's approval passed wholly into the control of the Home Ministry. Though challenged on occasion by certain elements within the bureaucracy, he continued to exercise virtually complete authority over the Institute until October 1914 at which time there occurred certain changes in the relationship of the Institute to the bureaucracy which were not to his liking. He therefore resigned and established a private laboratory of his own to which the entire research staff of the Institute of Infectious Diseases emigrated with him.[37]

Bacteriology's remarkable development in Japan, which establishment of these two institutions symbolizes, was only possible for two principal reasons. One was that Japanese scholars had made strenuous efforts to acquire as much scientific and technical information from Europe as possible during the country's two and a half centuries of isolation. Annual trade missions from the Netherlands brought books and scientific instruments to Japan which by the end of the eighteenth century had permitted the stirrings of an incipient native research tradition in astronomy, medicine and occasionally in other fields as well.[38] Still, the importance of these developments during the seclusion period for the development of modern science after the Meiji Restoration should not be overestimated. Such knowledge as Japanese scholars acquired of scientific subjects was usually incomplete and often erroneous. And because so much of the empirical knowledge entering the country in these years was partially or even wholly detached from theory, most fields of science after the Restoration had to be created out of nothing.[39]

The biomedical sciences were a partial exception because the Japanese had taken particular interest in this area for practical reasons and before 1868 were far closer to the frontiers of knowledge here than in any other field of science.[40] Such progress in medical science provided a basis for rapid intellectual development later on but was also important in two other ways. For one, even the most rudimentary education in medicine served to introduce relatively large numbers of Japanese youth to science before the Restoration. While only a small fraction of those who graduated from schools where Western medicine was taught later pursued careers in science, one may assume the number of scientists produced during the period before 1900 would have been smaller yet had earlier attempts to acquire knowledge of Western medicine not been made. But the fact that early progress in medicine established that discipline as the "official image" of science in Japan whereas physics had enjoyed that distinction in Europe is perhaps even more important. This, it is suggested, may account in part for the failure of modern science to influence other spheres of Japanese culture more than it actually did.

Modern science's development as an organized social activity in Japan cannot be understood apart from the activities and policies of the Japanese government. During the first few years after the 1868 Restoration Japan's new leaders adopted a wide range of basic reforms aimed at making their country the equal of any in the West. Expansion of military power was the keystone of these reform efforts; but tied to it were policies calling for abolition of the traditional class system, industrialization, expansion of education and the promotion of science and technology. A dual strategy was adopted in the latter case which brought foreign teachers to Japan while Japanese youth were sent by the government to Europe and occasionally to the United States. Except in the very early years, scientific and technical subjects had no monopoly on those which the Japanese *ryugakusei* were sent to study as law and public administration were studied considerably more often than science or engineering. However, a consistently high percentage of the *ryugakusei* went abroad to study medicine, usually to Germany but sometimes to Britain, France or Austria. Indeed, of the total number sent abroad by the government between 1895 and 1912, some one-hundred and eight, or about seventeen percent, fell into this category.[41]

Establishing universities, special schools and technical institutes was still another way in which the government endeavored to foster scientific progress. Creation of Tokyo University in 1877 by merger of three existing schools was the most important single step of this kind. In 1886 a graduate school was added and in 1897 a second "imperial university" was established at Kyoto. By 1920 there were four imperial universities, each having schools of medicine, law, engineering, science and liberal arts. For all of these institutions Tokyo University served as an organizational model.

Kitasato's Criticism of the Tokyo University Medical School

Among the hundreds of young Japanese who benefitted from these efforts of the government to promote science was Kitasato Shibasaburo. Born in the Kumamoto region of Kyushu island, Kitasato first encountered Western medicine in 1871 at a school in Kumamoto which the ruling daimyo of the area had established within the grounds of his castle. He decided to study medicine in Tokyo after completing his course there and in 1874 entered an academy which later became part of Tokyo University. Hoping for a career in medical research he decided to enter the service of the Bureau of Public Health after graduating from the Medical School in 1884. Fortunately, employment there gave him the opportunity to assist Dr. Ogata Masanori in his experiments and thus introduced him formally to bacteriology. Within a year the government awarded Kitasato a stipend for advanced work in this field at Koch's laboratory in Berlin University.[42]

During his six years in Berlin Kitasato made two contributions to science of great importance. In 1888 he published a paper describing the procedures he had used to achieve a pure culture of the tetanus bacillus. As this was an effort in which several other prominent bacteriologists had failed, his success attracted considerable attention.[43] However, subsequent research which he did in some sense overshadowed it. Continuing his work on tetanus, Kitasato was able to demonstrate in 1890, together with Emil von Behring, working on diphtheria, that there exist certain substances in the blood serum of the body capable of neutralizing foreign materials. The epoch-making paper in which they reported these researches not only explained the basic processes of immunization but also laid the theoretical foundations of the new science of serology.[44]

These years were also important to Kitasato in another way and it is this aspect of his experience in Germany on which attention will be focused here. For Kitasato, while working with Koch, came to believe that bacteriology in Germany was in an "unfortunate position" generally and that the organization of the German medical schools, especially the one at Berlin, made an already undesirable situation worse. The experience in Germany convinced him also that these defects had been transmitted to Japan through its adoption of German academic medicine as an organizational model. In his estimation the worst feature of German medical science was the separation it made between clinical medicine and basic medicine. Using the University of Berlin Medical School as a frame of reference he argued that the formal organization of the Tokyo University Medical School was harmful to medical research because it differentiated sharply between clinical medicine and basic medicine just as the Berlin Medical School did.[45]

Although some of the force behind Kitasato's argument stems from the characteristics of bacteriology described earlier, a consideration which influenced his thinking as much or more was the fact that his teacher, Robert Koch, believed the Berlin Medical School's organization had made *his* research difficult. Two letters Kitasato wrote in 1892 describe an incident occurring in 1890 which affected Koch's interests so adversely as to prompt the German scientist's resignation from the faculty of the Berlin Medical School. The incident in question was the tuberculin controversy which resulted from a speech by Koch implying that a new substance he had discovered, called tuberculin, might help cure tuberculosis. When the new discovery failed to do so, Koch's reputation suffered a temporary disgrace which, as he saw it, might have been avoided had the Medical School not insisted on the usual division of labor between its basic and clinical sides. Because Koch held a professorship in hygiene and bacteriology on the basic side, he had to entrust the requisite clinical tests to two professors on the clinical side. Whether reasonably or not, he seems to have blamed the two colleagues for his own mistake when the tests which they made after his announcement showed tuberculin to be therapeutically inefficacious.[46] This experience of Koch seems to have been a very considerable influence in turn on Kitasato's thinking about the social relations of bacteriology in Japan.

In the writings and statements of Kitasato, then, are two interrelated criticisms of the social relations of bacteriological research at the Tokyo University Medical School based on the social relations of bacteriological research at the University of Berlin Medical School. The first was that the organization of the Medical School inhibited cooperation among each of the various sections relating to bacteriology. His second criticism was that the progress of that discipline was also impaired by the fact that the Medical

School had established a sharp division of labor between its basic and clinical sides. Both of these criticisms were directed at the formal organizational arrangements characteristic of the biomedical sciences in the two countries. For analytical purposes it is useful to view the first as essentially a critique of the so-called chair system and the latter as referring to certain influences of professionalization in medicine on bacteriology.

Formal Organization in Two Medical Schools: Berlin and Tokyo

What was the chair system and how did it come into being? Essentially, the term referred to the structural division of the spectrum of knowledge into a series of discrete units with a full professor presiding over each. To that extent it was an inheritance from medieval times in Germany. The principal concern of this paper, however, is with aspects of the chair system which influenced its response as a system of institutions to the expansion of the bio-medical sciences, especially bacteriology, during the nineteenth century. Close attention will therefore be paid to the relationship between the chairs and the clinics, laboratories and institutes which grew up at that time partially as a result of greater specialization and professionalization in medicine. As for the chairs themselves, they rarely if ever included more than a single professor and a handful of students before the nineteenth century. But the growth of science changed all that dramatically. By the time Kitasato arrived at the Berlin University Medical School in 1885 a chair in medicine commonly had one or two associate professors, several lecturers, teaching assistants and research assistants in addition to the full professor and the graduate students. Ben-David and Zloczower have argued that in Germany this complex of organizations affected patterns of interpersonal relations among medical scientists in ways which were detrimental to the progress of medicine, including bacteriology.[47] And because the behavior of Japanese medical scientists resembled that of medical scientists in Germany in certain ways, Kitasato argued that the chair system must have been responsible in each case. Given the similarities between his argument and the functionalist approach to science, the question of whether he was right or not is a matter of some interest.

Establishment of a chair system at Tokyo University was first suggested by the school's president in a letter to the Education Ministry in September 1890 although no action was taken on this request until the summer of 1893 when Inoue Kowashi became minister. The new minister seems to have viewed the chair system as a means of alleviating the financial burden of paying large salaries to the foreign professors still teaching at the university.[48] The foreign teachers cost three times as much to employ as Japanese and he believed their number could be substantially reduced if more efficient use were made of the native faculty. Establishment of a chair system would further this aim, as he saw it, because the teaching staff would not be required as before to teach any course in their department but could now teach exclusively in their respective fields of specialization.[49] From this point of view the reform was clearly a success because it enabled the government to reduce spending on the university by twelve percent while actually increasing the number of professors.[50]

Financial considerations relating to the chair system had one other important effect, which was to suggest adoption of the French chair system in preference to the German one.[51] While the German system permitted only one chair per discipline, the French system allowed multiple chairs as needed; so Tokyo University also came to have a multiple chair system. That financial considerations were behind this decision is indicated by an interview which another Minister of Education gave to a medical journal called the *Ikai Jiho* in 1913. Its reporters asked Dr. Okuda Kijin why Tokyo University Medical School had so many professors in view of the fact that German medical schools managed to carry on with far fewer. In replying the Minister explained that Germany had considerably more medical schools that Japan and emphasized that no one of them had to accommodate as many students as those in Japan did.[52] The clear implication of the Minister's remarks was that it was cheaper to establish new chairs at existing universities than to establish new universities from the ground up.

There was probably another reason as well for adopting the French chair system rather than the German one. Unlike German higher education, Japanese higher education was highly centralized, reflecting differences of political organization in the two countries. Japan was a centralized empire in which Tokyo controlled education at all levels for the entire country. Germany after 1871, by contrast, was a federalized empire each of whose formerly independent states retained extensive autonomies in educational matters. A more appropriate model for Japan than Germany, in consequence, must have been France where centralization was typical of all areas of administration, education among them.[53] This influence of French higher education on Japanese higher education is important to note in connection with Kitasato's implicit assumption that German educational models were the only ones employed by the Japanese in the biomedical sciences.

The chair system itself had assumed the following pattern at the Tokyo University Medical School by 1908:[54]

BASIC MEDICINE - Number of Chairs		CLINICAL MEDICINE – Number of Chairs	
Anatomy	2	Internal Medicine	3
Physiology	1	Surgery	2
Pharmacology	1	Orthopedic Surgery	1
Pathology	2	Obstetrics and Gynecology	1
Biochemistry	1	Pediatrics	1
Hygiene and Bacteriology	2	Ophthalmology	1
Legal Medicine	1	Dermatology and Urology	1
		Psychiatry	1
		Oto-rhino-laryngology	1

By contrast, the Berlin University Medical School's chair system looked like this at that time:[55]

BASIC MEDICINE – Number of Chairs		CLINICAL MEDICINE – Number of Chairs	
Anatomy	2	Internal Medicine	1
Physiology	1	Surgery	1
Pharmacology	1	Orthopedic Surgery	1
Pathology	1	Obstetrics and Gynecology	1
Biochemistry	1	Pediatrics	1
Hygiene and Bacteriology	1	Ophthalmology	1
Legal Medicine	1	Dermatology	1
		Pathological Anatomy	1
		Psychiatry	1
		Oto-rhino-laryngology	1

Two interesting differences are immediately apparent from the charts. First, Berlin had only one chair for hygiene and bacteriology while Tokyo University by this time had two. Secondly, no discipline at Berlin except anatomy had more than one chair while five disciplines enjoyed that distinction at Tokyo University. With these differences in academic organization in mind, the larger social context in each of the two countries may be considered in order to show ultimately that the formal organization of the Tokyo University Medical School could not have had nearly so much influence as Kitasato claimed on the status of bacteriological research there.

Sociologically speaking, the hallmark of the biomedical sciences in Germany during the period of their greatest fruition was competitive interaction. Among the twenty-eight university medical schools there was a constant flow of professors and students with each university endeavoring to attract the most capable.[56] Berlin University in particular, because it occupied the preeminent position, made an effort to secure the best students and the most distinguished professors for its faculty. There especially it was required that anyone hoping for an appointment first make a reputation at another university.[57]

Beginning in the last quarter of the nineteenth century, however, the German medical schools, including Berlin, began to deviate from the system in certain ways. Professors and students still moved from one university to another and competition still took place for distinguished faculty members, but a process of "compartmentalization" began to inhibit these interaction patterns. Accompanying the rise of the medical research institutes most medical schools began to duplicate facilities, became reluctant to share them with members of other sections, and in the clinical disciplines, showed a frequent unwillingness to allot part of the patient case load to other sections or to let them perform activities claimed by a particular section as its own responsibility.[58] This movement toward "compartmentalization" in Germany is said to have resulted primarily from the inability of the academic system to expand in a manner appropriate to the needs of medical science. Establishment of the institutes in the German medical schools, therefore, resulted partly from a need to differentiate research roles and provide more positions for scientists than the academic system would have created otherwise.[59] While medical science expanded organizationally by creating new chairs during the first three quarters of the nineteenth century, it expanded scarcely at all in the latter decades. Establishment of specialized research institutes became a way of allowing science to expand while maintaining the chair system intact. Such a mode of expansion, however, created certain problems which seriously impaired the effectiveness of the system.

Whereas German academic medicine had displayed a high incidence of competition between the chair-holding professors *(ordinärii)* and the private lecturers (*privatdozenten*) in each discipline during the first three-quarters of the century, competition in the latter years tended to be replaced by various types of patron-client relationships between the two groups. Partly for that reason and partly because the number of attractive positions available to the younger men declined drastically, certain fields lost momentum and eventually experienced declines in research productivity.[60] These changes are said to have resulted from the rise of the institutes which, in turn, were partly a function of the chair system's failure to expand. Earlier in the century only the most rudimentary facilities had been needed for research and those who did research usually had a medical practice or some other source of income with which to support themselves as *privatdozenten* before receiving the call to a professorship. The relatively small number of working scientists, moreover, together with the relatively large number of universities, meant that innovations and reforms were accepted and diffused rapidly through competitive interaction among the universities.[61]

The internal growth of science juxtaposed to the academic system's inability to expand effectively stifled this process. Because the facilities required for research were far more expensive than most scientists could afford and because the academic system was expanding vertically through the establishment of institutes but scarcely at all horizontally through the establishment of chairs, younger men had to enter the institutes to acquire the credentials needed for an academic career. Research facilities at the institutes, however, were monopolized by the directors who usually held professorial chairs concurrently; and control of these facilities by the professors enabled them to curtail and finally eliminate the competition from the private lecturers which had existed earlier. The lecturers, therefore, apprenticed themselves to the professors as research assistants in order to rise in the rigidly structured system. Having eliminated the possibility of direct competition from the younger men, the professors frequently came to value in their assistants such qualities as an ability to take the professor's side in an academic dispute and to avoid disagreement with him. Movement of such "schools" of medical scientists from one university to another under these circumstances, moreover, often involved transferral of the entire group.[62]

Although conditions in Japanese medical science at that time had some similarities to conditions in Germany, the differences must be given far greater weight. Quite apart from what happened in practice, there was a certain ideological emphasis on competitive interaction in Japan just as there was in Germany. Dr. Okuda's remarks in the *Ikai Jiho* interview mentioned earlier implied a favorable view of both the freedom of movement German professors and students enjoyed and the competitive interaction created by the *privatdozent* system. But in the same interview the Minister emphasized that these practices could not be permitted in Japan because the country lacked Germany's large number of university medical schools and professors.[63]

Numerous other persons in positions of influence also stressed the importance of institutionalized competition for the well-being of science. In 1890 Dr. Hasegawa Tai, a physician member of the Diet, requested that body to establish a second imperial university at Kyoto because, in his words: "Observation shows that because of the lack of competition, the professors at Tokyo University have ceased....to discover new scientific theories and the students....to pursue their objectives."[64] And in 1893 when Education Minister Inoue Kowashi introduced the chair system with its incentive payments for research, a desire to promote greater competition among the professors is known to have been among his principal concerns.[65] Similarly in 1918, the Education Ministry's Chief of the Bureau of Professional Education, Matsuura Shinjiro, told the Budget Committee of the Diet's Lower House he agreed with the committeeman who said that "competing chairs" (*kyoso koza*) improve the quality of a university; and in this same testimony he emphasized that only lack of money had prevented more of such chairs from being established.[66]

Matsuura's statements indicate that Japan's four major medical schools were supposed to compete with each other and there are good reasons to believe that they did so. To cite one example of such competition: the Dean of the Tokyo University Medical School in 1910 ordered one of his students to begin research on a disease which had attracted attention from medical research groups at the Kyoto University Medical School and the smaller Okayama Medical College, saying, "Tokyo University Medical School must not fall behind those institutions."[67] And if the predictions of functional analysis are correct, the existence of multiple chairs in several disciplines at each of the four imperial universities would have encouraged other forms of competition, as among professors for students.

However, it is exceedingly unlikely for reasons to be explained later, that the formal organization of the Medical School really encouraged this kind of competition at all. Indeed, the empirical evidence strongly suggests that Tokyo University Medical School had problems of disunity and compartmentalization more severe, if anything, than those at Berlin. An article in the July 18, 1914 issue of *Ikai Jiho* noted that every section at the Medical

School insisted on having its own library, specimen room, equipment room and other research facilities, demands for which in the journal's opinion, were not only wasteful but conducive to discord among the professors and students.[68] Later that year an anonymous physician who either spoke for Kitasato or may even have been Kitasato, told the *Tokyo Asahi Shimbun* that the Medical School had built four chemical laboratories though requiring only one. He denounced this as a superfluous form of "competition against oneself."[69] Certain prominent members of the medical profession who also served in the Diet were equally critical of the Medical School on this point. When the Education Ministry in 1918 requested money for a chair in serology, Dr. Tsuchiya Seizaburo, editor and publisher of the medical journal, *Nihon no Ikai,*[70] objected saying that an existing chair in that field at the Institute of Infectious Diseases (associated with Tokyo University Medical School after March 1916) made a second one unnecessary. He urged the Ministry of Education to follow the practice in European medical schools of teaching serology in connection with a chair of pharmacology or bacteriology and declared that the university had only requested the second chair "because of some enmity among its professors."[71] Dr. Yagi Itsuro was another member of the Diet who apparently believed this. Yagi had graduated from the Tokyo University Medical School and studied several years in Germany at the University of Rostock before entering private practice in Nara Prefecture.[72] Despite what one might assume to be his loyalty to Tokyo University, he opposed a government appropriation bill for his alma mater in 1914 and offered the following explanation for doing so: "The professors of the Tokyo University Medical School," he asserted, "do not even deserve to be called scientists [because they] confine themselves to small domains and will not cooperate with each other in research."[73]

Precisely why Tsuchiya and Yagi made these remarks is open to a variety of interpretations. Tsuchiya had vested professional interests opposed to those of the "University Medical School Faction" or *Daigaku Ha* as it was called. Moreover, he was a well-known admirer of Kitasato, whose attitude toward the Medical School has already been indicated.[74] Yagi, on the other hand, had not even met Kitasato at this time and was also attacking people with whom he had once been associated.[75] It is possible that his views and to some extent those of Tsuchiya, reflected the influence of the traditional Western ideology of science. Certainly Yagi's experience in Germany suggests this possibility. On the other hand, both physicians may have been describing the affairs of the Medical School professors more or less accurately.

If Ben-David and Zloczower are right, the German medical schools, including Berlin, exhibited certain characteristics which might permit application of the same description to them: 1) There was a kind of "roping-off" process occurring in the German medical schools whereby a particular section or institute would claim exclusive rights in certain fields of research or assert that it alone had the right to perform autopsies or care for certain kinds of patients; 2) Many professors resisted the establishment of a second chair in their discipline because this might have required them to share the patient case load and certain research facilities or because it might have reduced their income from student fees;[76] 3) The tendency of many sections to lay claim to a particular field of research inhibited the career possibilities of younger men working in the same field who found themselves in the "wrong" section.[77] Collectively, these practices are said to have created serious tensions within German academic medicine, not only creating factions or "schools" but undermining its productivity in the process.

But the real problem lies not in determining whether such tendencies, termed "compartmentalization," actually existed or not. Certainly there are very good reasons, including the evidence cited here, to believe that they did. Considerably more important is the question of why Ben-David and Zloczower have argued that in Germany these developments resulted from changes in the formal organization of the biomedical sciences, specifically from the creation of the institutes and clinics. If one accepts this as a working hypothesis, the question of whether it could also account for similar, even identical, kinds of behavior in Japan immediately arises. In other words, were factions there a product of formal organizational arrangements as Kitasato claimed? This question, of course, can only be answered by comparing formal organization in the two medical schools and the two university systems. Such a comparison, it is argued, indicates that while compartmentalization with its factions or "schools" did emerge in Germany as a product of organizational changes, in Japan compartmentalization existed all along because of the prior existence of factions! This conclusion seems to follow from the fact that Tokyo University and the Japanese university system were organizationally flexible at precisely the points where Berlin University and the German university system were rigid.

Evidence presented earlier indicates, first of all, that Tokyo University often had two or even three chairs in a single discipline whereas Berlin almost never did. This meant that chances of obtaining a chair were greater at Tokyo than at Berlin and, moreover, that potential for competition in the formal organization of the Medical School was also greater in the former. Secondly, it was much easier to create new chairs in the Japanese system than it was in the German. As Bureau Chief Matsuura said in 1918: "In the imperial universities the establishment of chairs depends on the development of science itself."[78] At any particular German university, by contrast, the number of chairs could usually be increased only by dividing an existing field into smaller fields, not by adding a second or third chair in the same field.[79] And thirdly, it is important to note that Japanese higher education experienced considerably more horizontal expansion during this period than did German higher education, as the Japanese government between 1885 and 1920 built three new imperial universities while in Germany the last prewar foundation was made at Strassburg in 1874.[80]

Medical Scientists and Medical Practitioners

Investigation of the effects of professionalization in the medical systems of the two countries tends, if anything, to reinforce the basic conclusion that compartmentalization in Japan was not the result of formal organizational arrangements as such. The object here will be to show that the movement toward professionalization in Japanese medicine had far less influence generally than it did in Germany; and consequently, that Kitasato's insistence on the influence of the basic-clinical medicine dichotomy at Tokyo University was largely misplaced.

So far as bacteriology in Germany is concerned the principal effect of professionalization was to establish a sharp differentiation of medical roles based on the classifications of researcher and clinician. First of all, it promoted a gradual differentiation of professors in medical schools from the physicians who practiced medicine. Before about 1850 large numbers of German scientists in all fields had earned their livings by practicing medicine. But when medical research emerged as a fulltime occupation, a distinction came to be made not only between physicians and professors but between those in basic medicine and those in the clinical disciplines.[81] Ordinary physicians, moreover, as a result of this change, were effectively deprived of the right they had once had to utilize the facilities for research in the medical faculties and the public hospitals. With professionalization both came to be monopolized by the professors.[82] Finally, opportunities for communication between professors and physicians became much less frequent when they occurred at all because the professors withdrew from the ordinary medical societies to form their own professional associations.[83] The principal effect of these changes on bacteriology would seemingly have been to obstruct the very unification of clinical practice and research which had led to its creation in the first place.

In Japan, by contrast, these problems were certainly much less acute if they existed at all. While a sharp division between basic medicine and the clinical specialties was instituted at the Tokyo University Medical School under German influence, a considerable amount of evidence strongly suggests no sharp differentiation was made in Japanese medicine as a whole between the roles of medical scientist and physician. Assuming they had the proper clique affiliations, physicians were given access to university and hospital research facilities until at least 1917 and possibly even thereafter.[84] Moreover, all the professors of clinical medicine at Tokyo University themselves maintained substantial private practices and usually their own hospitals, largely because academic salaries were so low. In certain cases, professors were said to be earning between 40,000 and 50,000 yen (approximately 20,000 to 25,000 dollars) each year from treating private patients outside the university setting. Even the Dean of the Medical School, Dr. Aoyama Tanemichi, as one of the university's three leading internists – who also did bacteriological research – earned 20,000 yen annually despite his administrative functions.[85]

Tokyo University professors of clinical medicine, of course, were not atypical in maintaining private practices as such, for Berlin University professors in the clinical specialties also saw patients outside the academic framework. To that extent treatment of patients by Tokyo University professors outside the Medical School setting does not in itself demonstrate that Japan lacked a sharp role differentiation between scientists and physicians. However, it must be emphasized that Tokyo University professors seem to have devoted the *major* portion of their time and energies to seeing private patients whereas the Berlin professors generally remained loyal to the academic ideal.[86] The very small number of medical specialists in Japan at the time, presumably, was one reason for the inability of the Japanese professors to do this. While German patients had a relatively large number of such specialists available to them, the Japanese did not; so there was an important social need for the services of the well-trained Tokyo University professors. "Considering the present state of Japanese culture," the Vice Minister of Education declared in 1920, "it may not be such a bad thing for professors of clinical medicine to maintain large private practices."[87] And the inability of married professors with families to live on a professor's salary, acknowledged even by the Education Ministry, must have been a second.[88] At Berlin, by contrast, a professor in the Medical School could enjoy a comfortable standard of living, especially when income from student fees, nonexistent in Japan, was taken into account.[89]

Yet, it is perfectly true, as Kitasato might have argued, that most of the work in bacteriology at Tokyo University took place in two laboratories on the basic side. And here conceivably there might have been a problem; for professors in the basic disciplines were not permitted to maintain private practices and had to supplement their meager incomes by additional, part-time lecturing or by writing textbooks.[90] Actually, however, it seems unlikely that bacteriology could have suffered on that account for denial of the right to practice medicine by no means implied denial of access to the necessary clinical facilities. Dr. Miura Moriharu and Dr. Yamagiwa Katsusaburo in the Pathology Section were permitted to carry on clinical tests and observations in the main Tokyo University Hospital[91] while Dr. Ogata Masanori and his associates in the Hygiene and Bacteriology Section used the clinical facilities of the Komagome Hospital, a university affiliate.[92] Dr. Nagayo Mataro in Pathology, moreover, had access both to the main university hospital and later to the clinical facilities of the Institute of Infectious Diseases.[93]

That Japanese bacteriology did not suffer from the formal organizational divisions between basic and clinical medicine which may well have plagued this science in Germany is also suggested by two other considerations. One is a campaign, partly ideological, partly political and partly economic in motivation, directed against the professors of clinical medicine which aimed to enforce just such a division. As early as 1893 the clinical professors were attacked by the Great Japan Medical Association (*Dai Nihon Ishi Kai*) for taking work away from general practitioners by treating private patients. The *Ikai Jiho*, which participated editorially in these attacks, declared that the Medical School professors were supposed to serve as "models" for the Japanese medical profession and demanded they "resign and take some other job" if they could not live on their salaries.[94] Certain other vehicles of public opinion and professional medical opinion also found these deviations from professional standards reprehensible, emphasizing that professors must not neglect the teaching and research responsibilities for which they had been hired. In 1900 a former Vice Minister of Education, concerned that too many of the scientific papers appearing in the *Daigaku Kiyo* (University Annals) were written by foreign scientists, exclaimed in a speech to the House of Peers: "What research have these professors done? What discoveries have they made? What have they written?"[95] From all indications this movement reached a crescendo after the First World War broke out since the Anglo-French naval blockade of Germany removed that country as a possible source of medical, scientific and technical information for the Japanese. Thus the *Tokyo Asahi Shimbun* published a series of articles in November 1914, vigorously attacking the professors for the attention they gave to treating private patients: "The professors of clinical medicine at the Tokyo University Medical School treat patients in their homes, operate hospitals and say they are doing scientific research."[96] One physician member of the Diet even declared in a speech delivered the following month that if the professors did not do more research, they should be forced into early retirement in order to make room for those who would![97]

It may be suggested that the very vehemence of this campaign and the rather considerable period of time during which it persisted suggests that Japan could not have institutionalized a lasting role differentiation between the physician and the medical scientist much before 1920, if then.

Still another aspect of the Japanese medical profession pointing to this conclusion is the way membership in the leading medical societies was determined. It was noted that in Germany medical scientists and physicians by this time generally did not belong to the same professional organizations. In Japan, however, membership in professional medical societies seems to have been based primarily, though not exclusively, on clique affiliations. Thus, medical men joined either the Meiji Medical Association (*Meiji Ikai*), created in 1894, or the Japan Federation of Medical Societies (*Nihon Rengo Ishi Kai*), which existed under various names from 1893, according to whether they identified themselves with the so-called University Faction (*Daigaku Ha*) or the Anti-University Faction (*Min-i Ha*).[98] Each association included medical scientists as well as physicians among its members. Kitasato for many years refused to participate in the activities of either one because this would be "improper for a scientist."[99] But even his German-style professionalism gave way to social reality when colleagues in the Federation's predecessor persuaded him to accept the presidency of their organization in 1916. His attitude, however typical in Germany, was not typical in Japan; most other Japanese medical scientists were very active in these two physicians' organizations and despite their numerical inferiority often dominated them.

Informal Organization in the Tokyo University Medical School

If the tendencies toward compartmentalization criticized by Kitasato and other Japanese scientists did not result from the Medical School's formal organization, they could only have resulted from its informal organization. Interestingly enough, while Kitasato himself appears never to have charged the Medical School publicly with factionalism as such, there are good reasons for believing that he did recognize its existence and that he took an unfavorable view of it, at least so far as bacteriology was concerned. Certainly persons with whom he enjoyed close association spoke out against factionalism in the Medical School often enough.[100] And there is little doubt but that

Kitasato himself had more than sufficient motivation to confine his public statements to criticisms of formal organization and pass over informal organization, as it were, unnoticed.

Existence of the highly influential "Kitasato Faction" (*Kitasato Batsu*),[101] it is suggested, gave him reason enough to maintain silence on this point. It was noted earlier that Kitasato had built his original Institute in Tokyo with private support and that later on he managed to fend off several attempts by opponents within the bureaucracy to undermine his authority over it. These efforts succeeded only because he enjoyed the support of an extensive network of strategically placed friends, associates and former pupils in various parts of the government, particularly in the Home Ministry. Not only was the Bureau of Public Health completely under his influence but the overwhelming majority of Japan's prefectural and other local public health officials were graduates of the special course in health administration which he offered at the Institute of Infectious Diseases.[102] These persons were a ready-made pressure group on whom he relied with striking success to protect his interests as needed. So influential was the "Kitasato Faction" that *Munsey's Magazine* in 1907 suggested Kitasato might be among the eleven most powerful men in Japan, a judgment confirmed by Japanese sources as well.[103] Indeed, it was precisely this concentration of power and influence which prompted the Education Minister, Dr. Ichiki Kitokuro, and the Prime Minister, Count Okuma Shigenobu, to change the Institute's administrative relationship to the Cabinet in 1914.[104] Small wonder, then, that Kitasato avoided discussing factionalism by name in public.

Even when he did say or write things which implied an unfavorable view of factions, he confined himself to deploring the influence they supposedly had on inter-group cooperation. However, contemporaries of Kitasato and more recent critics have also accused the Medical School's factions of excessive particularism in recruitment of faculty and of suppressing free discussion among their members. It therefore seems appropriate here to consider these criticisms as well.

Among the accusations leveled against the Medical School was that its faculty was excessively "inbred" due to particularistic recruitment and promotion procedures. It was said that only by graduating from the Medical School and having a relative on its faculty could a talented young biomedical scientist become a Medical School professor at all.[105] In fact there is a good deal of evidence to support this assertion. Hardly ever was anyone from outside the University invited to join its faculty. So rare was the occurrence that the *Ikai Jiho* in 1905 claimed the appointment of Dr. Suto Kenzo, graduate of a private medical school, to an associate professorship in biochemistry heralded a major change in recruitment policies.[106] But the prediction proved false.

The question, of course, is what one chooses to make of all this. Certainly the traditional ideology of science has always stressed the ill effects among scientists of particularism in any form. Thus Theodor Billroth, professor of surgery at the University of Vienna, warned in 1876 against "forming a faculty exclusively of natives and making professorships hereditary in certain families" on the grounds that such practices "always have baneful results."[107] But even if this and similar statements are justified in a general way, their applicability to the practices of the Tokyo University Medical School during this period is not at all clear; as certain evidence suggests that the recruitment system there was quite universalistic in content, however particularistic in form. The principal mechanism for recruitment of faculty at the Medical School was the comprehensive examination given students upon completion of the regular M. D. course. This examination accomplished three things. It determined a student's rank in class; it determined the sections of the graduate school he might enter and those from which he would be excluded; and it eliminated all but the select few from whom the professors would choose their future sons-in-law and successors. Whenever possible the professors made these selections from those placing first, second or third in the examination.[108] The talented young biomedical scientist, as defined by the examination, then married a professor's daughter and eventually acquired a chair, though usually not the one occupied by his father-in-law.

The examinations naturally stimulated keen competition among the students which presumably insured that any potential recruit to the faculty had attained a certain standard of excellence. One successful veteran of the examinations, Dr. Manabe Kaiichiro, recalled that when he graduated at the top of his class in 1904, the competition was "unbelievably severe" as the examination "determined a person's fate for the rest of his life."[109] Nor was competition confined to the students; the professors also are said to have competed for the most promising son-in-law. In general professors on the clinical side whose sections enjoyed greater prestige had the advantage.[110] Dean Aoyama, for instance, was able to get the number two man in the class of 1907, as his Internal Medicine Section was particularly well regarded.[111] On the other hand, Dr. Ogata, whose Hygiene and Bacteriology Section on the basic side ranked considerably lower in student estimation, tried but failed to marry his daughter to the top man in the class of 1902.[112]

Among the five senior men doing bacteriology during this period, only the youngest, Nagayo, had a relative closely associated with the Medical School. (His father had been its dean from 1874 to 1879).[113] The four older men were among the first generation of professors so naturally did not enjoy that advantage. Two of the four senior professors, Miura and Yamagiwa, had graduated first in their respective classes of 1881 and 1889;[114] while Aoyama and Ogata, as *ryugakusei,* must of necessity have been in the upper ten percent of theirs. And Nagayo himself, apart from whatever career benefits his father's achievements gave him, was certainly as well qualified in a formal sense as the others, having graduated second in 1904.[115] Thus, if even a minimally universalistic character is attributed to the examination, it seems unlikely the quality of the faculty could have suffered greatly from such "inbreeding."

Tolerance of criticism and free discussion, by contrast, is a matter about which generalization is slightly more difficult for the Medical School as a whole. Among the three sections where bacteriology was done, free-ranging discussion and criticism seem to have been actively encouraged in two, the Pathology Section and Ogata's Hygiene and Bacteriology Section; but in Aoyama's Internal Medicine Section both were probably inhibited. Concerning the Pathology Section there exists a remarkable unanimity of opinion on this point. One of its members said that Dr. Miura, the senior professor by date of appointment, strongly encouraged his students to formulate opinions of their own;[116] while another said he carefully avoided use of status language in order not to discourage free expression of views.[117] In fact Miura seems to have developed a special technique (which was probably not unusual at all) for eliciting opinions from students and junior members of the academic staff. His procedure was to share an *o-bento* (box lunch) with them every Saturday afternoon, followed by a long walk and usually a visit to the Yukokuro Restaurant where all imbibed freely. "On these occasions," said Dr. Yamagiwa, who had himself been a student of Miura, "reserve between professor and students was cast aside."[118] Equally so was this the case when Nagayo took over active direction of the laboratory from the two older professors in 1906. Nagayo made a practice of levying fines on members of the laboratory group who used honorific forms of address when speaking to him and generally shared tea and cakes with his junior associates every afternoon.[119]

Much less is known about the interpersonal relations of the Hygiene and Bacteriology Section as it had considerably fewer members than the other two laboratories and remaining descriptions of its internal affairs are accordingly quite scarce. However, Ogata is said on at least one occasion to have accepted from two of his students criticism described as "direct and unreserved" about a matter of some scientific importance;[120] and surviving general descriptions of his personality are consistent with this assertion.

Tokyo University's senior bacteriologist is described as "modest," "taciturn" and "living in an ivory tower," hardly the sort of traits associated with an authoritarian personality.[121]

Quite the opposite traits apparently characterized Dr. Aoyama Tanemichi, the Medical School's powerful dean and most influential of the three internists. Most accounts agree that he was "arrogant," "haughty," overbearing at times and susceptible to flattery.[122] His students were afraid of him and whenever possible avoided expressing opinions contrary to his. One former student recalled that the surest way to pass one of the dean's oral examinations was to "expound eloquently to Dr. Aoyama nothing but his favorite opinions about the pathology of a disease."[123] Though referring in this case to Aoyama's manner of instruction in the hospital ward, his procedure in the laboratory was apparently much the same. While in 1959 several of his former students attempted to show that he was really very tolerant of dissenting opinions, their descriptions are congruent neither with specific details of his style of leadership nor with the description provided by his own biographer in 1930.[124] "Whenever a student wrote a paper and submitted it to Aoyama," wrote Dr. Uzaki Kumakichi, "he would scrutinize it with great care and criticize it sharply. He seldom accepted a new thesis at first reading. In the event that a student presented a particularly bold idea, Aoyama would scold him, saying, 'Are you certain you want to write something so audacious?' Moreover, in the event the student had contradicted a leading authority, Aoyama always warned him he must reconsider that part of the argument."[125]

Despite such relatively authoritarian behavior, or perhaps because of it, this and other factions in the Medical School generated a deep loyalty to the senior professor and strong solidarity among their respective members. So the question naturally arises as to what influence either may have had on the amount of free discussion. Most critics of factionalism in Japanese science have stressed the influence of the senior professors in inhibiting criticism. But the possibility that certain kinds of group solidarity were also detrimental should be considered as well. In fact it appears that solidarity was a negative factor in the Aoyama group. One source states that if any student appeared to question Aoyama's judgment of a patient's condition too openly during a bedside diagnostics session, other section members were certain to reprimand him for it later.[126] And on one particular occasion students of Aoyama's were responsible for disrupting what existing accounts suggest was a legitimate student protest movement against the Medical School administration so their professor, the Dean, would not "lose face."[127]

Still, none of this permits the generalization that solidarity inhibited criticism and free discussion in and of itself. In the Pathology Section, for instance, there was quite as much solidarity and exclusivity toward other groups as in the Aoyama Section. But within the group Miura tried and largely succeeded in instilling a "spirit of harmonious cooperation and mutuality."[128] When we note that frequent drinking and socializing with the members of his section was one of the ways he used to accomplish this, it seems significant that Aoyama rarely did either of these things. Except for an occasional glass of wine in his home, the Dean was a teetotaler who even lectured his students and colleagues on the evils of excessive carousing.[129] Considering the remarkable extent to which Japanese society in general relies on informal socializing with alcohol to ease tensions between persons of different status, the absence of this socializing or its inhibition in the Aoyama Section could only have had a deleterious effect on its morale and effectiveness.[130] In short, this comparison suggests that the personality of the senior professor *did* largely determine whether a faction encouraged new ideas or resisted them. If this inference is justified, then the answer to the question of whether group solidarities in the Medical School obstructed free discussion or not probably depends on which pattern of social relations one thinks was more typical; and on that point, it is argued, most evidence favors the pattern of social relations in the Pathology Section.

In the analysis of the problems of free discussion and recruitment procedures at the Medical School one important assumption has been made – that neither changed significantly through time. Since both patterns were congruent with fundamental aspects of Japanese tradition, such an assumption is probably justified. But in reference to the problem of inter-group cooperation it probably is not since this pattern involved a major change from traditional behavior. Moreover, the empirical evidence suggests the problem of inter-group cooperation is unresolvable unless significant change through time is assumed from the beginning.

Consider the following pieces of evidence. In 1894, one year after the chair system was established and a mere eighteen months after several associates of Aoyama had attacked Kitasato in print on purely personal grounds,[131] the same two scientists from different *institutions* led a joint research expedition to Hong Kong seeking to determine the cause of plague.[132] As Kitasato's ensuing paper, which appeared in *The Lancet,* left certain scientific issues unresolved, a lively controversy arose in Japan over the validity of his claim to have isolated the offending bacillus. Accordingly, the Medical School dispatched several research expeditions to Taiwan and the Kobe-Osaka area in the late 1890's to resolve them and included in each were professors from different *sections,* usually Ogata and Yokote from Hygiene and Bacteriology, and Yamagiwa from Pathology.[133] Yet by the second decade of the twentieth century one finds these same two sections working for an extended period of time on precisely the same disease with no indication of cooperation between them at all.[134] Moreover, secondary accounts of the history of bacteriology in prewar Japan never mention any cooperative research involving members of different groups after these particular expeditions.

This, of course, does not prove conclusively that cooperative research had ceased among Japanese bacteriologists by this time; but there are good reasons for believing that it had and that consequently Kitasato's general description of the situation at Tokyo University should be accepted even if his public explanation for it should not be. In all probability there was not very much willingness within any of the relevant sections on either side of the Medical School to cooperate with the others. Members of Aoyama's section on the clinical side probably took a very exclusivist attitude toward the members of the Pathology and Hygiene Sections on the basic side and *vice versa.* While the evidence in general supports this assertion, the more difficult problem as before is to formulate a convincing explanation for it.

Logically, there are only three agents to which the decline of cooperation could be attributed: the professors, the students, or both. Two of these, involving the professors, it is suggested, can be eliminated from consideration on the following grounds. First, the professors were all on amicable terms with one another and remained so throughout this period so far as can be determined. Secondly, there were no changes in personnel among them except for the promotions of Yokote in 1908 and Nagayo in 1911; and there are no reasons at all to suppose either event made any substantial difference. Thirdly, the professors did in fact cooperate with each other before about 1900 but not thereafter. In short, the problem is to explain not merely the decline of cooperation but its timing as well.

To that end it is argued, first of all, that cooperation among different sections was not inhibited to any extent by the professors but rather by the often intense feelings of solidarity which developed among the students and younger section members. Suppression of actions which their peers defined as disloyal to the group was one manifestation of this solidarity and cooperation with other groups, one suspects, was often defined in that way. Consider the following event which occurred at the Medical School during this period. In 1916 Dean Aoyama secured the prior agreement of his two internal medicine colleagues to establish an institute for hydro-therapeutics and X-ray treatment. As a cooperative venture the institute was supposed to serve the needs of all three sections but did not because members of two of the sections refused to work there. The younger men apparently objected to the Dean's appointment of a former student as director and not even pressure from their own professors managed to change their attitude.[135] While one can never hope to know all relevant aspects of this situation, the account of it which survives does suggest not only the failure of formal organizational arrangements to dictate behavior but the limited ability of the professors to stimulate cooperation among their respective sections.

That being the case, the principal reason for a decline in cooperative research about 1900 would simply have been that membership in each of the three sections began to increase sharply about that time. Consider the following chart:

Membership In Tokyo University Medical School Sections Relating To Bacteriology

Section	1897	1908	1917
Internal Medicine (Aoyama)	5	10	21
Hygiene and Bacteriology	3	5	15
Pathology	8	24	46

In no case does it show anything less than a three-hundred percent increase in the membership of each section during the twenty years between 1897 and 1917.[136] Thus, even if a professor wished to stimulate certain kinds of cooperation with members of other groups, the effect of these increases would have been to limit his ability to do so because peer group influence on each member would have been much greater than before, assuming constancy of solidarity feelings between individual section members. Moreover, not even the opposing effects of growth in numbers beyond a certain point would necessarily have enhanced prospects for cooperation since loyalty to the group remained the standard by which social action was legitimated whatever the centrifugal effects of factions within the larger faction may have been.

But even if Kitasato and other critics were correct in recognizing a near absence of inter-group cooperation at the Medical School, one must still consider the more basic issue of whether that really mattered. Were the exclusivist behavior patterns of its research groups really detrimental to creative research or might they in some respect have promoted creativity? Stated in this form the question obviously admits of no definite answer as factionalism undoubtedly had both effects on different occasions. However, the problem here is considerably more limited as it is only required to demonstrate that the Medical School's informal organization *could* benefit creativity and that it actually did so in important ways.

Two accomplishments of the Pathology Section provide empirical evidence for this argument. In 1915 Dr. Nagayo Mataro began research with several members of his section on the cause of a disease called scrub typhus or *Rickettsia tsutsugamushi.* This disease had first been reported by Japanese physicians around 1900 but within a few years was also found to exist in the Malayan peninsula, the Dutch East Indies, Australia and India. The large area over which scrub typhus was dispersed, together with its apparent links to a large number of other diseases, thus attracted wide attention among Japanese scientists even though the affected areas in Japan itself consisted only of a few sharply defined river valleys in three mountainous prefectures.[137] Accordingly, members of Ogata's Hygiene and Bacteriology Section along with investigators from several lesser institutions also began studying this disease, thereby creating a highly competitive research situation.

From what is known of the incident it seems likely that factionalism benefitted the investigation in two ways. Its competitive pressures, first of all, stimulated Nagayo to begin studying the disease himself. In 1915 the Medical School was the target of bitter criticism from the recently displaced Kitasato Faction which claimed the University's contributions to bacteriology had been few. Specifically to refute these accusations, Nagayo became the first scientist anywhere to study the highly contagious disease in the field and led the first of many expeditions to Yamagata Prefecture in July of that year.[138] One may also suppose the same competitive pressures from other groups kept him there. Within a year the Nagayo team managed to link the disease to a specific pathogenic agent. However, they were not able to explain its life cycle completely until 1924; and even then had to carry out many more expeditions and laboratory tests before the medical profession bestowed its unanimous approval on their findings in 1930.[139] In short, competition among factions encouraged replication and independent testing of claims based on research findings and in that way benefitted science. Secondly, when the unusual amount of work and the extraordinary investment of time required for resolution of the scrub typhus problem are taken account of, it seems reasonable to suppose a well integrated research group would have a natural advantage over one less integrated. Indeed, that was precisely the sort of group Nagayo had tried to create in the first place by socializing with his younger colleagues and by discouraging their use of status language when speaking to him.

In the work of Dr. Yamagiwa Katsusaburo and his associates on cancer still another potentially beneficial effect of factionalism on scientific research can be seen. In 1915 Yamagiwa achieved one of the most important advances in the entire history of cancer studies with a "classical" paper demonstrating that tumors could be produced in experimental animals by the application of coal tar to the skin over prolonged periods. His work was important both theoretically because it placed Rudolph Virchow's doctrine of chronic irritation as a cause of cancer on a sound experimental basis and methodologically as well since it enabled researchers to induce tumors in host animals far more easily than had been possible earlier.[140] In the words of *The Lancet:* "It is impossible to over-estimate the importance of Yamagiwa's discovery for the study of cancer."[141] Indeed, the 1915 paper made him a leading candidate for the 1926 Nobel Prize in Medicine, awarded, however, to Johannes Fibiger in an action now widely acknowledged to have been an error.[142]

For present concerns the point of greatest interest is the process by which Yamagiwa managed to achieve these results. Essentially they owed as much to the intense loyalty of his younger colleagues and students as they did to his own brilliance. Not only did Yamagiwa himself specifically say this,[143] the facts of the matter seem to admit of no other interpretation. The reason is simply that Yamagiwa was a semi-invalid who suffered from pulmonary tuberculosis for nearly all his professional life.[144] Because of his physical condition it is highly unlikely he could have accomplished anything unassisted; for even with help his 1915 paper represented ten years of work.

III. THE VALUES OF JAPANESE SCIENCE IN HISTORICAL PERSPECTIVE

Individualism and Science: Europe and Japan

Both because of the frequency with which they are cited by Japanese critics and because of their theoretical significance in functional analysis, discussion in this paper has centered on the fourth category of arguments: formal organizational arrangements and their relationship to creativity. The basic claim of these critics is that Japanese science was epiphenomenal and uncreative first because of its "feudalistic" apprentice system of recruitment and socialization and secondly, because of its failure to permit free exchange of views within research groups and cooperation among them. For the most part it is alleged that the chair system was responsible for these deficiencies. But was it? Ben-David and Zloczower argued that similar shortcomings appeared in German medical science because of the one-chair rule, the difficulty of establishing new chairs and the lack of horizontal expansion in higher education during the late nineteenth century. These factors, they suggested, diminished the ratio of academic positions to those seeking them, created obstacles to the horizontal movement of students and younger scientists, and therefore encouraged the formation of patron-client relationships between scientists of higher and lower status which eventually lowered research productivity. So far as bacteriology specifically was concerned, the differentiation of scientists from physicians implied by the basic-clinical separation is said to have been harmful because it denied research facilities to the latter and direct access to clinical facilities to the former while segregating each in different professional organizations.

But was the Japanese situation really similar? Clearly it was not. First, the Japanese chair system, partly because of its French antecedents, permitted multiple chairs per discipline; secondly, it was not particularly difficult to establish new chairs; and thirdly, Japan's higher educational system was expanding horizontally much more than Germany's was. Thus the ratio of positions available to the number seeking them was higher in Japan; the students were objectively freer to move horizontally within the university, and their objective motivation to become the client of a senior scientist was therefore less. In the particular case of bacteriology, the objective situation was also more favorable in Japan since the differentiation of scientists from physicians had not progressed nearly so far, research facilities were not denied to the latter (until about 1917) nor clinical facilities to the former, and both belonged to the same professional organizations. Formal organizational arrangements, therefore could not have been responsible for the "deficiencies" of Japanese science attributed to them.

The alternative claim is that the scientists themselves were responsible for the alleged deficiencies. Rather than dispute this assertion directly, it is argued that, for the most part, these practices were not quite the deficiencies they might seem. Whether they married professors' daughters or not, for instance, Tokyo University Medical School professors were selected from among the better qualified. Moreover, criticism and free discussion were not only tolerated but encouraged except in Aoyama's Internal Medicine Section. Even the decline of cooperation, it is suggested, did not necessarily mean competition and creativity were compromised. Indeed, the work of Nagayo and Yamagiwa suggest that solidarity, loyalty, integration and competition, the results of distinctly Japanese value orientations, were not only compatible with creative science but probably gave it a highly positive stimulus. Thus, while some of the practices described as "deficiencies" actually existed, they were not what critics have made them out to be. Others, moreover, did not exist at all. Thus, the problem for functional analysis, it would seem, is that it could not have predicted this combination.

From this analysis two conclusions can be drawn and one of them answers the first question posed in this paper. Since Japanese science had certain fundamental value orientations markedly different from those of Western science at both the level of organizational or institutional norms and the level of personal value orientations, it *was* epiphenomenal. These different values in the Japanese case, according to functional analysis, ought to have been the result of inflexible organizational arrangements as they were in Germany. But the preceding discussion has shown this could not have been the case. Functionalism also maintains that because certain necessary values were absent, whatever science was done under the circumstances could not have been creative by definition. But the Pathology Section's contributions to knowledge indicate that was not the case either. In short Japanese science was not necessarily uncreative because it was epiphenomenal. When, therefore, the ability of the Japanese to do creative science in the absence of certain allegedly fundamental values is taken into account, a second conclusion follows: that any theoretical explanation concerning the relations of science to culture and social structure in which values are assumed to be non-problematical at all levels of generality is empirically false and theoretically unsound.

In order to answer the second question posed earlier, it is necessary at this point not only to state what values were lacking in Japan but to indicate their place in the series of assumptions underlying functional analysis and criticisms by Japanese who define science in terms of values. It has been pointed out that Japanese values appear to have stressed solidarity, loyalty, affectivity and integration in addition to inter-group competition. Affectivity and solidarity in particular seem to clash with the emphasis Parsons places on affective neutrality and specificity. Given his definition of those terms, this is tantamount to saying the Japanese were not sufficiently individualistic.

The term "individualism" or "individuation" merits closer examination as it is central to the discussion here. Definitions vary according to context and the philosophical predilections of the writer. Theodorsons' *A Modern Dictionary Of Sociology* applies "individuation" to "the breakdown of group ties and the emergence of individuals who lack strong feelings of group loyalty..."[145] while Bernard Barber describes "individualism" as "a moral preference for the dictates of individual conscience rather than for those of organized authority" and declares it "an attitude....most congruent with science."[146] It is scarcely accidental that connotations so divergent have attached themselves to the same term, as the weakening of primary group ties and other intermediate-level associations has often been attributed in Western countries to the rise of science (among other things).[147] But is science really responsible? If it is, how can one explain the absence of such a process in Japan whose modern history has also been dominated by the rise of industry, technology and science?[148] More likely, individualization or individuation as an influence on social structure or individualism in science should be attributed to the impact of ideological forces and the specific historical conditions within which science first arose in Europe.

These considerations lead to the second of the three questions posed at the beginning of this paper: Why was Japanese science "epiphenomenal"? Why did it lack the individualistic values of science in the West? One potential explanation advanced by Japanese critics was the frequency of "copying" stemming from the government's policies toward research. This argument appears to have a certain superficial plausibility since it is true that the Japanese government generally did not encourage scientific research, at least before 1914 and in some ways not before 1940. (Bacteriology was the single exception, partly because of its relationship to the well-being of the military). Nevertheless, the copying thesis should ultimately be rejected because it assumes these values would have become institutionalized if the government's policies had been different. Yet there is no reason at all to suppose such values as affective neutrality or specificity are inculcated by the mere act of *doing* research. One supposes rather that they become accepted because scientists are exposed to a cultural environment in which great ideological stress is placed upon them.

Peculiarities of language and thought processes are a second factor which several Japanese critics have suggested. Certainly the formation and acceptance of values is affected by language and ways of thinking. But the precise mechanisms by which they make their influence felt are matters of great controversy lying far outside the scope of this discussion. It is sufficient for present purposes to say they must have been important in undetermined ways.

Japanese critics defining science in terms of ideas, it was noted, have often linked science's epiphenomenality to the specific historical circumstance of its dependence on government patronage. Though surely wrong in attributing epiphenomenality to lack of government support for research, their emphasis on the importance of the government's role seems entirely plausible in itself. Much of Western science's individualism has commonly been explained by the self-supported, amateur status of its early practitioners and their lack of sustained patronage from the state.[149] By contrast, modern science in Japan was almost completely dependent on government support (such as it was) from the beginning. One might therefore suppose this unusual degree of dependence on government did obstruct in Japan those *ideological* forces which created the individualistic ethos of science in Europe. In this sense science's identification with the "collectivistic" aims of the state may also have impaired the ability of these same forces to undermine family loyalties and other primary group associations.

There are at least three other historical factors in Japan which probably inhibited the creation of a more individualistic ideology of science, if the European experience is any indication. One of these has already been mentioned: the fact that historically, it was the biomedical sciences rather than physics which in Japan formed the official, public image of science. Not only did medicine receive far greater attention than the other sciences in the Tokugawa Period, its extreme predominance continued well into the twentieth century. Science degrees conferred by Tokyo University provide one index of medicine's greater influence. Between 1876 and 1916, 2,613 degrees were conferred in medicine but only 814 in all other sciences. At the doctoral level the figures are similar in magnitude: 200 to 82 for the years 1888-1910.[150] This fact is important because physics projects a much more individualistic image than any of the biological sciences do. Roger Krohn found that physicists (and "academic" scientists generally) are noticably more likely than any kind of biomedical scientist to stress the importance of "personality" and "the individual" over "situation" and "the team" for creativity,[151] providing empirical support for the more impressionistic conclusions of earlier investigators. Why these differences exist is a matter which cannot be explored here. However, it seems significant that physics (optics and mechanics) attained intellectual maturity in the seventeenth century when science was very much an amateur activity, while medicine's maturity was delayed until the nineteenth century when professionalization was already beginning to reshape the social bases of science in fundamental ways.[152]

That Japan therefore acquired modern science in the nineteenth century *after* its professionalization was already well along is the third historical factor meriting emphasis. Professionalization marked an important change for science in general because it involved greater emphasis on the functions and responsibilities of the professional peer group and less on those of the individual practitioner. Of course, this earlier tradition of individualism retained considerable influence in the West where science had existed for several hundred years. But Japan had no such ideological heritage; and a newly deindividualized professional science could hardly compensate for its absence.

Japan lacked still another important historical experience associated with the rise of an individualistic ethos for science in the West: the long tradition of often intense conflict with religion. Not that scientific explanations for natural phenomena went totally unchallenged in Japan. They did not. Some eighteenth-century Buddhists opposed the replacement of Sumeru cosmology by the newly-acquired heliocentric view on religious grounds[153] and conflict was rife between Christians and the defenders of science in the Meiji period. But all of this pales by comparison with the history of such conflicts in Europe. It is scarcely novel to observe that a principal reason for the conflict between science and religion was the heritage of Aristotelean scholasticism which combined supernatural beliefs and empirical information in a synthesis so intricate that an attack on one part necessarily appeared to endanger the whole. Thus, scientists could hardly avoid controversy with religion; and one aspect of their response was the creation of appropriate values concerning the ways in which information was obtained. For ideological and political reasons scientific evidence had to be overwhelming against the theological opposition; and the values which emerged presumably contributed to that end. If Kenneth Downey is correct in assuming that values appropriate to an age of warfare between science and theory (e.g. "organized scepticism" or "individualism")[154] may no longer be needed in the West, what reason is there to assume they were *ever* needed in Japan, given its relative lack of experience with such warfare?

Having presented the case for rejecting theoretical explanations of science's relationship to culture and social structure in which values are thought non-problematical, it is now appropriate to consider briefly the problem of creativity in prewar Japanese science. This complex subject cannot be discussed in any depth here but one important point can be made about it in view of the general argument advanced in this paper. That is that creativity in science is far more a matter of ideas than it is of values.

Earlier it was observed that many critics who define science in terms of ideas nonetheless believe it was not very creative in Japan. If that was the case, one can only conclude that any acceptable explanation would have to base itself on factors of language, irrationalism and the absence of a tradition of scientific thought before modern times. Other arguments that have been advanced in connection with creativity seem to offer little. "Copying" is synonymous with the problem itself and is therefore not an explanation. Similarly, the arguments about organizational arrangements and behavior also have serious defects which the previous discussion has presumably made apparent.

The Accommodation Theory of Science and Culture

The analysis presented here thus would appear to require an alternative approach to the problem of science, culture and social structure, one in which for the Japanese case and presumably for others, questions of language, thought patterns and historical factors would have a central place. The approach suggested is in no sense original, having been proposed in a somewhat different form by the late John Peter Nettl and others.[155] Its principal recommendation is that science be investigated by proceeding from cognitive structures to institutions rather than the reverse. From this perspective science would be seen to seek social attachment, gathering the necessary force or power to influence, encourage or even dictate the conditions permitting it to flourish. It is suggested that science may do this by defining itself as an *ideology* and that it has, in fact, done precisely this in the past. With science defined as ideas, its principal impact on culture and social structure would be limited to producing changes in information. Thus, particular values held by a culture "receiving" science would be substantially affected only in so far as they were closely linked to some natural phenomenon concerning which a change in information was taking place. This description of what Downey has called the Accommodation Theory of science, culture and social structure[156] seems far more applicable to the Japanese case than that embodied in the Destruction Theory. Were it not for the corrosive effects of certain ideological influences it seems entirely likely the same would be true for Western countries as well.

FOOTNOTES

1 Erwin Baelz, *Awakening Japan* (New York: The Viking Press, 1932), p. 149.
2 *Ibid.*, p. 150.
3 Sakurai Joji, *Omoide no Kazukazu* (Tokyo: Herald Sha, 1940), pp. 19-20.
4 Sawayanagi Masataro, "Gakumon Dokuritsu no Shin Undo to Sono Kompon Mondai", *Shin Nihon*, 5:2 (1915).
5 Nakamura Hajime, *The Ways Of Thinking Of Eastern Peoples* (Honolulu: The East-West Center Press, 1964), pp. 531-576.
6 Yukawa Hideki, "Modern Trend Of Western Civilization And Cultural Peculiarities In Japan", in *The Japanese Mind*, ed. Charles A. Moore (Honolulu: The East-West Center Press, 1967), pp. 54-55.
7 Kikuchi Toshihiko, "Introduction to Chapter XII", *Nihon Kagaku Gijutsu Shi Taikei*, (Tokyo: Nihon Kagaku Shi Gakkai, 1962), vol. I, Tsushi I, p. 489.
8 Yoshida Mitsukuni, "Meiji no Kagakushatachi", *Jimbun Gakuho* XXIV (March 1967), pp. 230-231.
9 Morito Tatsuo, *Kagaku Kenkyu Jo Ron* (Tokyo: Kurita Shobo, 1939), p. 15 and

Yuasa Mitsutomo, *Kagaku Shi* (Tokyo: Toyo Keizai Shimpo Sha, 1961), pp. 228, 282. Hereafter cited as Yuasa, *Kagaku Shi.*

10 Otsuki Shoichiro, Nojima Tokukichi and Maki Jiro, "Nihon ni okeru Kagaku, Gijutsu to Kagakusha", in *Kagaku, Gijutsu to Gendai,* ed. Sakata Shoichi (Tokyo: Iwanami Shoten, 1963), p. 283. Hereafter cited as Otsuki, Nojima and Maki, "Kagaku, Gijutsu". Judging from these statements there appears to be some disagreement within the Marxist camp over whether scientific ideas really belong to the realm of "superstructure" or not.

11 "*Formal organization* refers to the organizational pattern designed by management or some other agency. *Informal organization* refers....to the actual organizational relations as they evolved as a consequence of the interaction between the organizational design and the pressures of the interpersonal relations among the participants." Cf. Amitai Etzioni, *Modern Organization* (Englewood Cliffs, New Jersey: Prentice-Hall, 1964), p. 40.

12 *Nihon Igaku Hyakunen Shi,* ed. Tamura Masao (Tokyo: Rinsho Igaku Sha, 1957), p. 4.

13 Yuasa Mitsutomo, *Kagaku Goju Nen* (Tokyo: Jiji Tsushin Sha, 1952), p. 262 and Hiroshige Tetu, "Social Conditions For The Researches Of Nuclear Physics In Pre-War Japan", *Japanese Studies in the History of Science* No. 2 (1963), p. 84.

14 Sakata Shoichi, "Kenkyu to Soshiki", *Shizen* (September 1947), p. 10.

15 John Desmond Bernal, *The Social Function of Science* (London: Routledge and Kegan Paul, 1939), pp. 267-278.

16 Sakata, "Kenkyu to Soshiki", p. 13.

17 Otsuki, Nojima and Maki, "Kagaku, Gijutsu", p. 310.

18 "*Values* are modes of normative orientation of action in a social system which define the main directions of action without reference to specific goals or more detailed situations or structures." Cf. Talcott Parsons, *Structure and Process, In Modern Societies* (New York: The Free Press, 1960), p. 171. Hereafter cited as Parsons, *Structure And Process.*

19 Cf. Norman Storer, *The Social System of Science* (New York: Holt, Rinehart and Winston, 1966). Hereafter cited as Storer, *Science.*

20 Talcott Parsons, *The Social System* (Glencoe, Illinois: The Free Press, 1951), p. 343. Cf. pp. 60-67 for definitions. Hereafter cited as Parsons, *Social System.*

21 Cf. Parsons, *Structure and Process,* ch. IV for discussion of values and levels of generality, pp. 132-168.

22 Parsons, *Social System,* p. 342.

23 Alex Inkeles, "Industrial Man: The Relation Of Status To Experience, Perception and Value", *The American Journal of Sociology,* (July 1960), p. 29.

24 Kenneth Downey, "Sociology And The Modern Scientific Revolution", *The Sociological Quarterly,* 8:2 (Spring 1967), pp. 246-249. Hereafter cited as Downey, "Sociology".

25 Talcott Parsons, *Essays in Sociological Theory* (rev. ed.; New York: The Free Press, 1964), p. 23.

26 This contradiction appears in Professor Parsons' own writing. In contrast to the emphasis he placed on the "autonomy" of scientific ideas in 1938, in 1951 he wrote: "Science....requires quite definite conditions in the structure of the social system, as well as the cultural prerequisites in the form of the adequate state of existing knowledge. Knowledge....gets applied only through the mechanisms of institutionalization of roles within which the *requisite* (my emphasis) combinations of motivational and cultural

elements can develop. Only by becoming in this sense incorporated into the structure of the social system, thus coming to constitute more than a body of 'ideas,' does empirical knowledge [i.e. science] acquire the basis for a major influence on action." Cf. Parsons, *Social System,* p. 348.

27 *Ibid.*, pp. 152-153.

28 Among those which will be cited elsewhere see especially: Joseph Ben-David and Awraham Zloczower, "Universities and Academic Systems in Modern Societies", *Archives Europeenes de Sociologie,* III:1 (1962), pp. 45-84. Hereafter cited as Ben-David and Zloczower, "Universities".

29 Joseph Ben-David, "Roles And Innovations In Medicine", *The American Journal Of Sociology,* 65:6 (1960), p. 562. Hereafter cited as Ben-David, "Roles And Innovations".

30 Abraham Flexner, *Medical Education in Europe* (Boston: D. B. Updike, 1912), pp. 237-238. Hereafter cited as Flexner, *Medical Education in Europe.*

31 In this instance the term "institutionalized" refers to the fact that formalized occupational niches reserved for bacteriologists had been established at Tokyo University and elsewhere.

32 "Eiseigaku no Reimei o Kataru", *Nihon Iji Shimpo,* 1956 (October 21, 1961), p. 30. Hereafter cited as "Eiseigaku no Reimei".

33 Richard Bochalli, *Robert Koch* (Stuttgart: Wissenschaftliche Verlags-gesellschaft, 1954), p. 77.

34 "Ogata Masanori Sensei Tanjo Hyakunen Kinen Zadankai", *Nihon Iji Shimpo,* 1507 (March 14, 1955), pp. 1005-1028. Hereafter cited as "Ogata Masanori Sensei".

35 *Tokyo Daigaku Igaku Bu Hyakunen Shi,* ed. Tokyo Daigaku Igaku Bu Hyakunen Shi Henshu Iinkai (Tokyo: Tokyo Daigaku Shuppan Kai, 1967), p. 298. Hereafter cited as *Igaku Bu Hyakunen Shi.*

36 "Eiseigaku no Reimei", p. 31.

37 For a brief account of these matters in English see John B. Blake, "Scientific Institutions Since *the* Renaissance: Their Role *in* Medical Research," *Proceedings of The American Philosophical Society,* 101:1 (February 1957), pp. 57-58.

38 George B. Sansom, *The Western World and Japan* (New York: Alfred A. Knopf, 1958), pp. 199-205.

39 Albert M. Craig, "Science And Confucianism In Togukawa Japan", in *Changing Japanese Attitudes Toward Modernization,* ed. Marius B. Jansen (Princeton: Princeton University Press, 1965), pp. 149-151, and Shigeru Nakayama, *A History of Japanese Astronomy* (Cambridge: Harvard University Press, 1969), pp. 226-231. Hereafter cited as Nakayama, *Japanese Astronomy.*

40 Donald Keene, *The Japanese Discovery of Europe* (rev. ed.; Stanford: Stanford University Press, 1969), pp. 16-31.

41 The figure 108 is given by Watanabe Minoru, "Japanese Students Abroad And The Acquisition Of Scientific And Technical Knowledge", *Cahiers d'Histoire Mondiale,* IX:2 (1965), p. 291. Seventeen percent is an estimate based on the numbers of Tokyo University students graduating in various subjects during this period. Cf. Yuasa, *Kagaku Shi,* p. 161.

42 Miyajima Mikinosuke, *Kitasato Shibasaburo Den* (Tokyo: Iwanami Shoten, 1931), pp. 3-25. Hereafter cited as Miyajima, *Kitasato.*

43 Takano Rokuro, *Kitasato Shibasaburo* (Tokyo: Nihon Shobo, 1965), pp. 29-35. Hereafter cited as Takano, *Kitasato.*

44 *Milestones in Microbiology* ed. Thomas Brock, (Englewood Cliffs, New Jersey:

Prentice-Hall, 1961), pp. 138-140.
45 See the following sources for Kitasato's criticisms of the Tokyo University Medical School: Miyajima, *Kitasato, p. 101;* Kitasato Shibasaburo, *"Shorai no Igaku"*, Yomiuri *Shimbun. No. 11998 (October 11, 1910),* p. 5; *Kitasato Kenkyu Jo Nijugo Nen Shi* ed. Kitasato Kenkyu Jo. (Tokyo: Kitasato Kenkyu Jo), pp. 5-6. *Kitasato Shibasaburo,* Statement to the *Yomiuri Shimbun,* No. 13468 (October 20, 1914) p. 4; and Kitasato Shibasaburo, "Chinjosho", letter of October 30, 1914 to Count Hijikata Hisamoto, Chairman of the Daj Nihon Shiritsu Eisei Kaj, (Great Japan Hygiene Society), in *Dai Hygiene Society), in Dai Nihon Shiritsu Eisei Kai Zasshi, 379, Supplement Sanjuni-ji*
46 *Nihon Shiritsu Eisei Kai Zasshi,* 379, *Supplement Sanjuni-ji Sokai, I (1914), pp. 2-3.* Nihon Teikoku Gikai Shi Kanko Kai, 1926-30), vol. II. Cf. "Yosan Sainyu Saishutsu So Yosan An Mombusho Jokan, 1893 Fiscal Year", 4th Diet, Representatives, January 11, 1893, p. 762. Hereafter cited as *DNTGS* plus volume number.
47 Ben-David and Zloczower, "Universities", pp. 45-84.
48 Terasaki Masao, "Koto Kyoiku," in *Inoue Kowashi no Kyoiku Seisaleu,* ed. Kaigo Tokiomi. (Tokyo: Tokyo Daigaku Shuppan Kai, 1969), p. 378. Hereafter cited as Terasaki, "Koto Kyoiku."
49 Kimura Tadasu, *Inoue Kowashi Kun Kyoiku Jigyo Sho Shi* (Tokyo: 1894), pp. 73-74.
50 Terasaki, "Koto Kyoiku", p. 378.
51 *Ibid.*, pp. 358-361.
52 Okuda Kijin, "Okuda Bunsho to Kataru", *Ikai Jiho* 990 (June 14, 1913), pp. 1098-1099. Hereafter cited as "Okuda Bunsho to Kataru".
53 Herbert Passin, *Society And Education in Japan* (New York: Bureau of Publications, Teachers College, Columbia University, 1965), p. 69.
54 *Igaku Bu Hyakunen Shi,* pp. 293-314.
55 Flexner, *Medical Education in Europe,* pp. 331-332.
56 Twenty-eight was the number of German-speaking universities, not the number in the German Empire itself.
57 Theodor Billroth, *The Medical Sciences in the German Universities* (New York: The Macmillan Company, 1924), p. 179. Hereafter cited as Billroth, *Medical Sciences.*
58 Awraham Zloczower, *Career Opportunities and the Growth of Scientific Discovery In 19th Century Germany; With Special Reference To Physiology.* Unpublished M. A. Thesis at the Hebrew University of Jerusalem, 1960, p. 42. Hereafter cited as Zloczower, *Career Opportunities.*
59 Ben-David and Zloczower, "Universities", pp. 49-50.
60 Zloczower, *Career Opportunities,* pp. 6, 89.
61 Joseph Ben-David, "Scientific Productivity And Academic Organization In Nineteenth-Century Medicine", *American Sociological Review,* XXV:6 (December 1960), pp. 828-843. Hereafter cited as Ben-David, "Scientific Productivity".
62 Zloczower, *Career Opportunities,* p. 89.
63 "Okuda Bunsho to Kataru", pp. 1098-1099.
64 Quoted in Shigeru Nakayama, "The Role Played by Universities in Scientific and Technological Development in Japan", *Cahiers d'Histoire Mondiale,* 9:2 (1965), p. 348.
65 Terasaki, "Koto Kyoiku", p. 377.
66 *Teikoku Gikai Shugiin Iinkai Giroku,* ed. Shugiin Jimukyoku (Tokyo: Shugiin Jimukyoku, 1920), vol. 42. Cf. "Daigaku Tokubetsu Kaikai Hoan hoka Ikken", House of Representatives Budget Committee, Forty-second Diet, Second Session, February 13,

1029, p. 9. Hereafter cited as *TGSIG* plus volume number.
67 Miyagawa Yoneji, "Densembyo Kenkyu Jo", in *Omoide no Aoyama Tanemichi Sensei,* ed. Kumagaya Kenji (Tokyo: Aoyama Sensei Tanjo Hyakunen Sai Jumbi Iinkai, 1959), p. 322. Hereafter cited as *Omoide,*ed. Kumagaya.
68 "Daigaku Igakka no Komponteki Kaisei", *Ikai Jiho,* 1047 (July 18, 1914), pp. 1248-1249.
69 "Bo Igaku Hakase Dan", *Tokyo Asahi Shimbun,* 10154 (October 24, 1914), p. 5.
70 *Gikai Seido Nanajunen Shi: Shugiin Giin Meikan,* ed. Shugiin Jimukyoku (Tokyo: Okurasho Insatsu Kyoku, 1962), p. 318. Hereafter cited as *Shugiin Giin Meikan.*
71 *TGSIG,* 40:1. Cf. "Tokyo Teikoku Daigaku oyobi Kyoto Teikoku Daigaku Rinji Seifu Shishutsu Kin Kuriiri ni kansuru Horitsuan hoka Ikken", House of Representatives Budget Committee, Fortieth Diet, Second Session, February 1, 1918, p. 5.
72 *Shugiin Giin Meikan,* p. 516.
73 *TGSIG,* 32-35. Cf. "Yosan Iinkai Giroku Sokki", House of Representatives Budget Committee, Thirty-fifth Diet, Seventh Session, December 16, 1914, p. 82.
74 "Ikai Dantai Undo Shi", Parts 15 and 16, *Ikai Jiho,* 1215-1216 (October 6-13, 1917), pp. 1757, 1801. Tsuchiya was a stanch proponent of a national federation of physicians and frequently attacked the "University Faction" for allegedly blocking its creation. He was associated with the loosely-knit *Min-i Ha* or "Private Physicians' Faction", referring to the fact that its adherents were neither members of the Tokyo University Medical School faculty nor government employees of any kind. In numbers *Min-i Ha* greatly exceeded the *Daigaku Ha.* Cf. "Ikai Dantai Undo Shi", Part 4, *Ikai Jiho,* 1200 (June 23, 1917), p. 1152.
75 Miyajima, *Kitasato,* pp. 304-305.
76 Zloczower, *Career Opportunities,* p. 20.
77 *Ibid*., p. 43.
78 *TGSIG,* 40:1. Cf. "Tokyo Teikoku Daigaku oyobi Kyoto Teikoku Daigaku Rinji Seifu Shishutsu Kin ni kansuru Horitsuan hoka Ikken", House of Representatives Budget Committee, Fortieth Diet, Second Session, February 1, 1918, p. 8.
79 Ben-David and Zloczower, "Universities", pp. 49-50.
80 Sumeragi Shido, *Daigaku Seido no Kenkyu* (Tokyo: Yanagihara Shoten, 1955), p. 338 and Flexner, *Medical Education in Europe,* p. 387.
81 Ben-David, "Roles And Innovations", p. 558.
82 Billroth, *Medical Sciences,* p. 29 and Flexner, *Medical Education In Europe,* pp. 145-166.
83 *Medical Education,* ed. Commission on Medical Education (New York: Commission On Medical Education, 1932), p. 344. Hereafter cited as *Medical Education,* ed. Commission.
84 "Ikai Dantai Undo Shi", Part 12, *Ikai Jiho,* 1211 (September 8, 1917), p. 1608.
85 "Daigaku Kyoju no Naishoku", Part V, *Tokyo Asahi Shimbun,* 9747 (September 12, 1913), p. 5.
86 Flexner, *Medical Education in Europe,* p. 148 and Abraham Flexner, *Medical Education* (New York: The Macmillan Company, 1925), p. 40.
87 *TGSIG,* 42. Cf. "Daigaku Tokubetsu Kaikei Hoan hoka Ikken", House of Representatives Budget Committee, First Session, Forty-second Diet, February 12, 1920, p. 2.
88 *TGSIG,* 40:1. Cf. "Tokyo Teikoku Daigaku oyobi Kyoto Teikoku Daigaku Rinji Seifu Shishutsu Kin Kuriiri ni kansuru Horitsuan hoka Ikken", House of Representatives

Budget Committee, Fortieth Diet, Third Session, February 4, 1918, Testimony by Tadokoro Yoshiharu, Vice Minister of Education, pp. 19-20. Hereafter cited as *TGSIG,* 40:1, Tadokoro.

89 *Medical Education,* ed. Commission, p. 337.

90 *TGSIG,* 40:1, Tadokoro, p. 21.

91 *Tokyo Teikoku Daigaku Byorigaku Kyoshitsu Gojunen Shi,* ed. Nagayo Mataro (Tokyo: Teikoku Daigaku Igaku Bu Byorigaku Kyoshitsu Goju Shunen Kinen Kai, 1930), vol. I, pp. 97-98. Hereafter cited as *Byorigaku Kyoshitsu.*

92 Ogata Norio, "Kitasato, Ogata Ryo Sensei", *Nihon Iji Shimpo,* 1415 (June 9, 1951), p. 1566.

93 *Nagayo Mataro Den,* ed. Nagayo Hakushi Kinen Kai (Tokyo: Nagayo Hakushi Kinen Kai, 1954), pp. 257-260. Hereafter cited as *Nagayo Mataro Den.*

94 "Aki no Mizu", *Ikai Jiho,* 1003 (September 13, 1913), pp. 1712-1713.

95 *DNTGS,* IV. Cf. "Gakusei Chosa Kai Setchi ni kansuru Kengi An", Fourteenth Diet, Peers, January 31, 1900, p. 111.

96 "Daigaku Kyoju no Naishoku", Part IV, *Tokyo Asahi Shimbun,* 9745 (September 10, 1913), p. 5.

97 *DNTGS,* IX. Cf. "Tokyo Teikoku Daigaku Ika Daigaku Kyoju no Shokuseki no kansuru Shitsumon", Thirty-fifth Diet, Representatives, December 15, 1914, pp. 960-961.

98 See footnote 74.

99 Kitajima Ta-ichi, *Kitajima Ta-ichi Jiden* (Tokyo: Kitajima Sensei Kinen Jigyo Kai, 1955), p. 92. Hereafter cited as *Kitajima Ta-ichi Jiden.*

100 Kitajima Ta-ichi, Deputy Director of the Institute of Infectious Diseases under Kitasato, stresses the latter's close ties to the *Ikai Jiho* and claims this journal, which frequently attacked "factionalism" in the medical profession, was virtually his mouthpiece. Cf. *Ibid.,* p. 99.

101 Eric R. Wolf provides a description of the term "clique" or "faction" which is essentially identical with the Japanese term *batsu* or "faction" as used in this paper: "Compared to the....friendship relation which covers the entire role repertoire of the....participants, clique relations tend to involve primarily the set of roles associated with the particular job. Nevertheless, the clique still serves more purposes than are provided for in the formal table of organization of the institution. It is usually the carrier of an affective element, which may be used to counterbalance the formal demands of the organization, to render life within it more acceptable and more meaningful. Importantly, it may reduce the feeling of the individual that he is dominated by forces beyond himself, and serve to confirm the existence of his ego... But it also has important instrumental functions, in rendering an unpredictable situation more predictable, and in providing for mutual support against surprise upsets from within or without. This is especially true in situations characterized by a differential distribution of power. Power superiors and inferiors may enter into informal alliances to ensure the smooth prosecution of their relationship, to guard against unbidden inquiries from the outside or competition from the inside, to seek support for advancement and other demands." Cf. Eric R. Wolf, "Kinship, Friendship, and Patron-Client Relations in Complex Societies," in *The Social Anthropology Of Complex Societies,* ed. Michael Banton (London: Tavistock Publications, 1966), pp. 15-16.

102 *Kitajima Ta-ichi Jiden,* p. 58.

103 Henry George, Jr., "The Strong Men of Japan", *Munsey's Magazine,* (October

1907), pp. 103-111.
104 "Denken Ikan no Hishi", *Ikai Jiho*, (June 19, 1915), p. 9.
105 "Batsu no Ika Daigaku", Part II, *Tokyo Asahi Shimbun*, 10159 (October 29, 1914), p. 5. Hereafter cited as "Batsu no Ika Daigaku".
106 "Gakubatsu Daha no Yoi Ichirei", *Ikai Jiho*, 554 (January 28, 1905), p. 131.
107 Billroth, *Medical Sciences*, p. 174.
108 "Batsu no Ika Daigaku", Part II, p. 5.
109 *Manabe Kaiichiro*, ed. Manabe Sensei Denki Hensan Kai (Tokyo: Nihon Onsen Kiko Gakkai, 1950), p. 105. Hereafter cited as *Manabe Kaiichiro*.
110 Arima Eiji, "Tamashi ni Oeru", in *Omoide*, ed. Kumagaya, p. 367. Hereafter cited as Arima, "Tamashi".
111 Arai Tsuneo, "Mikake ni yoranu Shojikisha", in *Omoide*, ed. Kumagaya, p. 141.
112 "Ogata Masanori Sensei", pp. 1013-1014.
113 *Igaku Bu Hyakunen Shi*, p. 289.
114 *Byorigaku Kyoshitsu*, I, pp. 169, 228.
115 *Nagayo Mataro Den*, p. 96.
116 *Byorigaku Kyoshitsu*, I, p. 193.
117 Yamagiwa Katsusaburo, "Ko Nihon Byori Gakkai Meiyo Kaicho Miura Moriharu Sensei Tsuito no Ji", in *Byorigaku Kyoshitusu*, I, p. 211.
118 *Ibid.*, p. 208.
119 *Nagayo Mataro Den*, pp. 102, 121-122.
120 "Ogata Masanori Sensei", p. 1021. Statement by Dr. Ogata Norio. The matter in question concerned the disease *Rickettsia tsutsugamushi* for which Dr. Ogata Masanori incorrectly claimed to have discovered the pathogenic agent.
121 *Ibid.*, pp. 1010, 1013, 1021.
122 Mitamura Takujiro, "Tanemichi no Sekai Dotoku", in *Omoide*, ed. Kumagaya, p. 163. Hereafter cited as Mitamura, "Tanemichi".
123 Yamada Jiro, "Karuisawa Sanso", in *Omoide*, ed. Kumagaya p. 348.
124 Imamura Yoshio, "Mangakyo", in *Omoide*, ed. Kumagaya p. 61 and Mitamura, "Tanemichi", p. 168. One possible explanation for this is that authoritarian behavior in a Japanese scientist was ideologically much less acceptable in 1959, due to post-war "democratic" reforms, than it was in 1930.
125 Uzaki Kumakichi, *Aoyama Tanemichi* (Tokyo: Aoyama Naika Doso Kai, 1930), p. 201.
126 Fujita Shuichi, "Aoyama Tanemichi", in *Kinsei Iretsu Den*, ed. Umezawa, Hikotaro (Tokyo: Chugai Igaku Sha, 1954), ch. 21, p. 274.
127 Arima, "Tamashi", p. 362.
128 *Byorigaku Kyoshitsu*, I, p. 193.
129 Yada Zennoshin, "Ketto Sokutei Kushin", in *Omoide*, ed. Kumagaya, p. 82; Sakai Tanibei, "Genkan Ban Nikki", in *Omoide*, ed. Kumagaya, p. 397; and Takahashi Akira, "Kanrei no Shukuji", in *Omoide*, ed. Kumagaya, p. 183.
130 Ezra Vogel, *Japan's New Middle Class* (Berkeley: University Of California Press, 1963), pp. 104-105.
131 *Kitajima Ta-ichi Jiden*, pp. 24-25.
132 Takano, *Kitasato*, pp. 72-80.
133 "Eiseigaku no Reimei", pp. 33-34.
134 This statement is an inference, not empirically demonstrable, based on the fact that both sections were working on the same problem (*Rickettsia tsutsugamushi*) at the same

time and in the same institution. Such a state of affairs in the United States or Europe usually indicates a complete lack of cooperation and there is every reason to believe it indicates the same thing in Japan as well!

135 *Manabe Kaiichiro,* pp. 141-142.

136 *Igaku Bu Hyakunen Shi,* pp. 352, 356; 388-390. Unlike the other sections, the Aoyama Section was located at the Dai-ichi Clinic until 1902, not at the Medical School itself. Moreover, the figures given here for its membership in 1897 and 1908 are an estimate based on membership figures for the other two sections of Internal Medicine.

137 Joseph E. Smadel, "Scrub Typhus", in *Viral and Rickettsial Infections of Man,* ed. Thomas M. Rivers (2nd ed.; Philadelphia: J. B. Lippincott Company, 1952), p. 638.

138 *Nagayo Mataro Den,* pp. 160-161.

139 *Ibid.,* pp. 162-168.

140 W. Cramer, "The Late Professor Yamagiwa", *The Lancet,* CCXVIII (May 24, 1930), p. 1155 and Kanematsu Sugiura, "Katsusaburo Yamagiwa", *The Journal of Cancer Research,* XIV:4 (October 1930), pp. 568-569.

141 *Ibid.,* p. 1155.

142 *Nobel: The Man and His Prizes,* ed. H. Schuck et. al. (Amsterdam: Elsevier, 1962), p. 247.

143 *Byorigaku Kyoshitsu,* I, p. 261.

144 *Ibid.,* p. 261.

145 *A Modern Dictionary Of Sociology,* ed. George A. Theodorson and Achilles G. Theodorson (New York: Thomas Y. Crowell, 1969), p. 199.

146 Bernard Barber, *Science And The Social Order* (Glencoe, Illinois: The Free Press, 1952), p. 65. Hereafter cited as Barber, *Science.*

147 Cf. Don Martindale, *Social Life and Cultural Change* (Princeton: D. Van Nostrand Company, 1962), pp. 437-502.

148 William Caudill and Harry Scarr, "Japanese Value Orientations And Culture Change", *Ethnology,* I:1 (1962), pp. 53-91 and Ronald P. Dore, "Mobility, Equality And Individuation In Modern Japan", in *Aspects of Social Change in Modern Japan,* ed. R.P. Dore (Princeton: Princeton University Press, 1967), pp. 113-150.

149 Roger Krohn, "Science *and* The Practical Institutions", *Proceedings Of The Minnesota Academy Of Science,* 28 (1960), p. 165. Hereafter cited as Krohn, "Practical Institutions".

150 Yuasa, *Kagaku Shi,* p. 161 and *Who's Who: Hakushi in Great Japan,* ed. Iseki Kuro (Tokyo: Hattensha, 1922-30), vol. II.

151 Krohn, "Practical Institutions", pp. 165-168.

152 It may also be significant that the major advances in physics had occurred in Britain and France while the biomedical sciences reached maturity in Germany where the individualistic ethos was probably somewhat less potent.

153 Nakayama, *Japanese Astronomy,* pp. 204-205.

154 Downey, following Storer, uses the term "organized scepticism" which Storer himself states is essentially the same as Barber's term "individualism" Cf. Downey, "Sociology", p. 253; Storer, *Science,* pp. 79-80; and Barber, *Science,* p. 65.

155 John Peter Nettl, "Ideas, Intellectuals and Structures of Dissent", in *On Intellectuals,* ed. Philip Rieff (Garden City, New York: Doubleday Anchor, 1970), pp. 57-134.

156 Downey, "Sociology", p. 248.

Western Science in Republican China:
Ideology and Institution Building

WESTERN SCIENCE IN REPUBLICAN CHINA: IDEOLOGY AND INSTITUTION BUILDING

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It has proven singularly easy to portray the pronouncements of Chinese scientists and promoters of science in the early part of the twentieth century as a variety of crypto-Confucianism.[1] There is no doubt much that is peculiarly Chinese in the views of science which these men held and tried to propagate and in their conceptions of what sorts of things had to happen for modern science to take hold and flourish in China. But this sort of analysis can be overdone. The primary concerns of these men can also usefully be related to problems that are not so uniquely Chinese but rather occur as part of the emergence of modern science in any given social and cultural setting. For in early twnetieth-century China the emergence of modern science was, and was perceived to be, a matter of the formation of a new type of community of thinkers. But much the same thing can be said about the emergence of modern science elsewhere. In very general terms, it seems that no matter how "modern science" has been brought into being, whether as an indigenous phenomenon in late Renaissance Europe or as part of some kind of cultural borrowing outside of Western Europe, the process has always involved attempts to bring into being a fairly well-defined scientific community. Invariably this has occurred, at least in part, outside of those institutions within which the given society's men of knowledge traditionally worked and has therefore involved the creation of new institutions, as well as the modification of old ones.[2] This observation, too, applies to China; as I shall show, it was the felt need to provide for the new types of institutions and social roles for men of knowledge which that community seemed to require that in no small measure gave direction to the activities of Chinese scientists and promoters of science at the time.

By suggesting that there is this sort of uniformity in what the emergence of modern science involves, I do not mean to imply that the new institutions or the new social roles for men of knowledge which are brought into being will turn out to be the same in every cultural and social setting, that scientific institutions and roles in China will be identical to those in Great Britain or the United States, for example. Nor do I mean to imply that the processes whereby they are created will necessarily be everywhere the same. It is of course clear that many of the particular emphases and pre-occupations of

Chinese scientists in the early part of the twentieth century cannot be understood apart from the specific circumstances in which they found themselves. But, by using such terms as ideology, social role, and institution building to describe and to explain these emphases and pre-occupations, I do mean to suggest that there is more to them than just some sort of Confucian residue.

* * * *

As Joseph Needham is in the process of showing, the Chinese have had a very considerable scientific and technological tradition of their own for a very long time.[3] By and large, however, science and technology were traditionally seen in China as being somewhat peripheral to the main concerns of men of knowledge, and during the late nineteenth century, this attitude was carried over and applied to Western science and technology when China was confronted with it in the form of Western military hardware. The Chinese men of knowledge, the Confucian scholar-officials, identified Western science with technology, and because within the confines of traditional Confucian civilization as they interpreted it, there had been no place for men of knowledge whose primary concerns were with Chinese technology, there most assuredly was no place in their view for men of knowledge interested in Western technology and sciences. The traditional negative assessment of the relevance of science and technology to the proper business of men of knowledge was reinforced by the obviously alien character of the Western versions of these subjects. For the scholar-officials, Western science and technology were not only largely outside of their normal purview as men of knowledge, but also were part of a more general cultural aggression which, by threatening the pre-eminence of traditional scholarship, was itself a direct challenge to their power and prestige.[4]

This conflict was crucial for the fortunes of modern science in China in the late nineteenth century because the scholar officials, by virtue of their dominant position in the Imperial bureaucracy, constituted the one class that might really have been in a position to promote modern science. In the first decade of the twentieth century, however, the pivotal mechanism which joined Confucian scholarship to bureaucratic power, the Imperial Examination System, was done away with, and that event signified a major change in the social context into which modern science had to be fitted. For the abolition of the civil service examinations involved a fundamental break with the traditional definition of the scholar class, of its activities, and of its relationship to the rest of Chinese society.[5] It formally broke the close connection between the civil service and a specifically Confucian education, it ended the Confucian scholars' nearly total monopoly of prestigious social roles for men of knowledge, and it left an institutional vacuum. A new Western-oriented school system was established and foreign study programs were inaugurated, both of which were intended to serve as avenues for men with Western knowledge to follow into the traditional social role for men of knowledge, office-holding, but once the old examination system was abolished, there was simply no formal system operating to bring people into the civil service. Decades passed with the new intellectuals which the reorganized educational system was producing "still finding no normal channels through which they might enter government office," even though that still remained their major ambition.[6] Also, since the primary non-official employments traditionally open to scholars had been closely connected to the examination system and to the scholar-official role,[7] the disappearance of that system involved a wholesale disruption of the institutional framework within which the social roles of scholars generally were defined and legitimated, whether the scholar was an office-holder or not. Finally, to compound these difficulties, for Chinese intellectuals in the early twentieth century, this all appeared to be but a part of a larger and "profound social and political crisis . . . the seeming collapse of a culture and a whole system of values" which followed the disintegration of Imperial China in the Revolution of 1911, and the clear failure of Confucian wisdom and principles to deal adequately with China's international and domestic weaknesses.[8]

Thus, the question of whether modern science and technology could somehow be accommodated within specifically Confucian values and institutions was, by the second decade of the twentieth century, formally irrelevant. Chinese scientists and others who wished to promote science and technology now confronted a situation in which the traditional institutions relating men of knowledge to their social environment had broken down and had not yet been replaced and in which the whole ethos which had legitimated social roles for men of knowledge had been swept away. The efforts of Chinese scientists to define a place for themselves and for science in early twentieth century China therefore necessarily involved them in attempts not only to delineate and solve the crises which China faced but also to fill the institutional and ideological vacuum left in the society as a whole by the disintegration of the Confucian order.

From the mid-1910's until the late 1920's, the effort to create an indigenous scientific community in China and to define science's relevance to the general social and cultural crisis which faced the country was, in many respects, centered around the activities of the Science Society of China *(Chung-kuo k'o-hsüeh-she).* Founded in 1914, the Society was one of the first privately created and completely Chinese scientific organizations to be brought into being and, at least until the creation of the Academia Sinica in 1927, probably the most important association of scientists in China.

The founders of the Science Society were part of a distinctly transitional generation in China. They, and most of the early members of the Society, were born in the 1890's and hence had grown up in the midst of the prolonged crisis which began in the last decades of the last of the Imperial Dynasties.[9] Moreover, these men, and other Chinese scientists of the same generation, were born into the class which was initially most immediately affected by that crisis, that of the scholar-officials. Almost all of these men began their education in China before the abolition of the old Imperial Examination System, and, as sons in scholar-official families, they generally seem to have received some classical training in their childhoods. In more normal times, these men would most probably have continued along the traditional paths to official and scholarly careers which their early education and family connections opened up to them, but the first decade of the twentieth century was definitely not a normal time, and especially after the abolition of the examination system in 1905, neither the traditional careers nor the traditional preparations for them quite meant what they had in the past.

The effects of the abolition of the examination system and of the other reforms of the early 1900's on the lives of those men who were to become scientists are quite clear. By the end of the first decade of the twentieth century, virtually every future scientist of this generation had turned to the new non-traditional kinds of education for which the reforms provided. This did not, however, mean that they were all thereby embarked upon careers in science. Few if any of the schools which these future scientists attended in China could give them much more than the most superficial training in the sciences, and in fact there was almost no thought that these schools were in any real sense preparing men to be scientists as such. The emphasis in these schools, as the names of some of them suggest – the T'angshan Engineering College, The Chihli Agricultural School, the Nanyang Institute of Technology, and so on – was distinctly on training technicians, as it had been in the government schools of the nineteenth century, and what instruction was provided was heavily biased toward technology and the applied sciences.[10]

In the nineteenth century, the identification of science with technology had made it a distinctly low class affair. In contrast, it seems that in the first ten or fifteen years of the twentieth century, it was precisely the perceived connection between material technology and the "wealth and power" of the Western nations which, for those Chinese who were to become scientists, made the natural sciences initially more attractive than other branches of

Western learning.[11] But these same men were very soon to become quite critical of the Chinese pre-occupation with technology and to attack the equation of science exclusively with technology as an unduly narrow view of science which did not adequately take into account the intellectual and moral significance of the scientific method and spirit and which would never permit science really to flourish in China.[12]

A more expansive conception of science, one which would encompass more than just its practical uses, was nevertheless not accessible to these men while they were still in China, and certainly no such view informed the education they received there. However congenial a more grandiose understanding of the place of science in China's future turned out to be for them, its articulation was very much a product of their education in the West, and particularly in the United States. There, they found a conception of science as an activity with primarily intellectual value, a conception which emphasized the methods of scientific research rather than any specific discoveries to which such methods had led, as well as the peculiar features of the scientific enterprise which made the advance of knowledge possible.[13]

Given the backgrounds of the early active members of the Science Society, it is not surprising that they readily accepted such a view of science. They had, after all, been born into a culture and a class for which scholarship had been a highly valued activity, and they had received at least the beginnings of a traditionally Confucian education, which was itself built around this judgment. By the time they were students in the United States, the subject matter of their traditional education had been largely discredited in their eyes, to be sure, but they still accepted the orthodox assessment of the worth of scholarship and transferred it to science and scientific research. In fact, as I shall show, what they perceived to be the methods of science and the norms of the scientific community appeared to them to be appropriate replacements for traditional Confucian learning and its attendant values. Consequently, they came to regard the promotion and advancement of science, both as a specialized activity for appropriately trained men of knowledge and as a proper subject of interest for non-scientists, as an urgent necessity to which existing organizations had given insufficient attention, and for which, therefore, new organizations were needed, "to put things in order from the base up."[14] What was needed, indeed, was the creation of some analog to the panoply of institutional arrangements which, in the West, made for the success of science and scientific research. For it seemed to the Science Society that the research ideal, the methods of science, and the normative system of the scientific community were all inseparably bound up with the institutions within which the scientific enterprise was carried out.

The members of the Science Society generally saw scientific organizations as the framework within which scientists in the West enjoyed mutually the "benefits of self-cultivation."[15] Research institutes served as "great and rich refuges" necessary for those who "desired to devote themselves to [science] as their life's work." At the same time, they provided a setting within which students could participate in the research activities of such specialists and thereby have their own interests aroused and be encouraged to become specialists themselves.[16] It was, in fact, the institutional framework made up by such organizations, together with the learned societies of the various disciplines, which allowed the scientist to develop the "characteristic temperament" necessary for "deep and profound research."[17] Moreover, learned societies not only constituted part of the socialization mechanisms of the scientific community, but also, through a "policy of encouragement by rewards" – the awarding of prizes, medals, and so on – directly stimulated and supported those with scientific talent.[18] Too, the journals which such organizations published not only "recorded immediately the advances of science," but also provided specialists with the opportunity to examine and verify the work of other specialists, thus making possible "co-operation in research and the mutual exchange of knowledge" among scientists, which was fundamentally important for the advancement of science and indeed basic to the nature of the scientific enterprise.[19]

The Science Society of China was intended to do all of these things for scientists in China, as well as to promote greater popular understanding of science. It had been founded in Ithaca, New York, by a group of Chinese students at Cornell University who, as "eyewitnesses to the glories of western culture and the retrogression of scientific thinking in China," felt particularly compelled, as one of them was to recall some twenty years later, to "complete the great activity of . . . bringing about innovations in thinking by means of literary agitation," that is, by editing and publishing a journal, to be called *Science (K'o-hsüeh).*[20] Although the Society existed for a time solely as a publishing venture, it soon became clear to its founders that to hope to achieve their goals, to promote science and encourage industry, simply by publishing a single journal was to "indulge in dreaming," and the Society was accordingly reorganized in October 1915 into a more typical learned society.[21] The "literary agitation" was continued, but at the same time special sections were created to deal with problems specifically related to agriculture and forestry, biology, mathematics and physics, chemistry, mining and metallurgy, and various types of engineering; and committees were organized not only to edit *Science,* but also to devise plans for book translations, for establishing uniform Chinese terminologies for the various sciences, and for building and operating a science research library in China.

After the Society's return to China in 1918, despite recurrent financial problems, its membership and the scope of its plans and of its actual activities all grew steadily; and during the course of the 1920's and 1930's, but especially after about 1925, the Society became increasingly more involved in narrowly scientific activities and correspondingly less so in the largely polemical matters that had been implied by its original commitment to "literary agitation."[22] By the 1930's, the bulk of the Society's resources was going to support scientific research, and the non-scientists in the Society had largely ceased to be active in its affairs; by then, too, the Society had moved to hold joint meetings with a number of the more specialized scientific organizations that had come into being in China,[23] and generally had come to see itself as the spokesman for what it took to be a thoroughly professional scientific community.

This community was itself almost entirely the product of the 1920's and 1930's – in 1914, when the Science Society was founded, there existed nothing in China even remotely like such a thing – and its formation was in a very real sense the major goal all along of the Society and its members. These men clearly felt that China's inability to move effectively to acquire Western science and technology was largely due to the country's failure to provide for a scientific community, that they would only be able to contribute significantly, as scientists, to China's future if such a community were brought into being and caused to flourish, and that this therefore, was their primary responsibility. In building up this sort of community, the Science Society's members felt, in other words, that they would necessarily be much more fundamentally involved in the resolution of the crises facing China than they could ever be were they simply to serve as technicians of one variety or another; for, in the process, it seemed to them that they would be contributing, not only to the advancement of science in China, but also to the general reorganization of China's society and culture.

Indeed, it seemed to them that these points were inseparable and that what was needed, both for the sake of promoting science and scientific research as such, and for the sake of China's future as a nation in the twentieth century, was a broader and more fundamental acceptance of the central position which science occupied in the modern world. For the Science Society, the propagation of this "correct" valuation of science was a crucial part of the process whereby a scientific community in China was to be created. In the early years of its existence, the Society therefore devoted a significant proportion of its energies to "literary agitation," giving over fully a quarter of all the articles appearing in its journal between 1915 and 1923 to various aspects of what amounted to a sustained polemic in support of science's claims to be the necessary basis for reforms of education, of political and economic affairs, of morality and culture, and of the Chinese way of life generally.

Although the content of the polemic was distinctly modern, the whole effort was, in many respects, rather traditional. The basic idea of "literary agitation" was itself not at all new – there were numerous periodicals in China, and there was a long tradition, to which the editors of *K'o-hsüeh* duly appealed, of men withdrawing from politics during "exasperating times" and turning to scholarship in order to "guide the world....[and] promote moral behavior in the people" through their writings. Such activities had been efficacious in the past, and, the Society argued, once again what was needed was renewed commitment to learning.

> In the strong nations of the world, the expansion of the people's rights and of national power have had to parallel the progress of scholarly thinking, and those countries which have allowed scholarship to go unattended have not had good fortune.[24]

Thus, because of the deterioration of scholarship, China's economy, as well as those "special excellencies of art and literature peculiar to the country" had decayed, the population had become "mean and careless," and the nation as a whole had "lost its spiritual center." In other words, because the people had become "unaccustomed to exact and profound studies, the society [had] lost its strong center, and the minds of the people [had come] to have nothing onto which to fasten."[25]

This was an analysis on which quite disparate elements of the Science Society's membership could agree,[26] and with its emphasis on the importance of scholarship, learning, and culture, it would have been quite acceptable as a diagnosis of China's ills, even within the confines of relatively orthodox Confucianism. The Science Society was not interested in that, however, but was explicitly concerned instead to show that Confucian doctrine was not relevant to the crises which beset the country.

> To study the writings of the ancients is not sufficient today; [it is necessary instead] to adopt that which has usurped the place of Chinese learning and to which all things owe their existence, and that is nothing other than science.[27]

Such a preference for science over the study of the classical texts was, and was intended to be, in conflict with strict Confucian principles, but even it could find a measure of legitimation within Chinese tradition. For example, a quotation from *Kuan Tzu* to the effect that "the principles of right conduct" would only be understood when material needs were satisfied was repeatedly adduced to show that science, at this point closely linked with technology, was necessary for the improvement of human morality.[28]

The Science Society's spokesmen, however, clearly thought that science's potential contributions to human well-being were more direct and more basic than this view of things made them out to be, and certainly more at variance with tradition.

> We of the Science Society think that the whole culture of foreign countries comes from science and that the decline of our country comes from the absence of science. Therefore, we take scientific research and the promotion of scientific activities as our goal.[29]

Thus, although science, to be sure, would lead to improvements in communication and transportation, to increases in production and national wealth, and to eradication of floods, famines, and plagues, what was important about science was that only it could form, and in the West already had formed, a basis for true morality, for the advance of knowledge, and generally for a truly modern society and culture.[30] It was science, in other words, that would, as Confucianism had proved itself incapable of doing, constitute the "strong center" of Chinese society and provide the "minds of the people" with "exact and profound studies to fasten onto." In the West, as science advanced, "the old habits and customs of societies [had been] fundamentally overthrown, and industry, agriculture, commerce, hygiene, and medicine [had been] all reformed,"[31] with the result that the whole of "modern man's way of life, whether it be his thoughts, his deportment, or the organization of his society" had come to have a "scientific content."[32] Thus, it seemed that in China the advance of science would raise individual morality, strengthen the national character, and generally help to cure the country's "desperate weaknesses."[33]

By the late 1910's and early 1920's, these sorts of claims for science were, among young Chinese intellectuals and students, not at all out of the ordinary. The Science Society's own sustained polemic was itself only a part of a wider effort to popularize science, and, in fact, during the controversies which surrounded the May Fourth Movement, science had come to figure prominently as an ideological entity, to be used – in the same way that it had been used by the Science Society – as a basis for a strident critique of Chinese tradition. Science was advocated as a necessary foundation for all programs of reform and, under the nickname of "Mr. Science," indeed became, along with "Mr. Democracy," one of the chief catch-phrases for China's new intellectual leaders.[34]

Most of the participants in the May Fourth Movement were only vaguely, if at all, interested in the advancement of science as such, and they did not concern themselves with concrete proposals for its development in China. In contrast, the effort of the leaders of the Science Society to promote what clearly amounted to a scientific ideology was directly related to the development of science, in the narrower sense of specialized research, in China. They did, of course, believe that if their pronouncements achieved widespread popular acceptance, then Chinese society would once again have a usable set of core values in terms of which a variety of needed reforms could be defined and legitimated. In this sense, their polemical statements were intended as a direct response to the cultural and social crisis which they saw engulfing China. At the same time, however, they also clearly felt that their polemics were a necessary part of the process whereby support was to be mobilized for the various sorts of scientific institutions which the example of the West had shown them were necessary for science to flourish and which they were trying to bring into being in China. The example of the West not only established the importance of such institutions, but, more generally, proved that their development and that of science itself "requires the positive approbation of society."[35] It seemed to the leaders of the Science Society that it was in no small part because such encouragements and approval for science had not been forthcoming in China that science had not become an integral and valued part of China's traditional culture.[36] Moreover, it was argued that, even in the twentieth century, there was still too little respect for science, and especially for scientific research, among those groups in the government, in education, and in industry and commerce which ought to have been patronizing science, and that until the central importance of science in the broadest sense was established, until China's need for science was made clear, no support for science in the narrower sense of specialized research would appear.[37] The Science Society and its members therefore felt that they had to "give the alarm to the government and arouse the society,"[38] for, as one of the members of its board of directors put it, the Chinese still "often mistakenly think that scientists are of no value to society, and although this is fallacious, nevertheless, the scientist cannot afford not to seek strenuously the trust of society."[39]

In the Science Society's view, the fundamental difficulty was that Chinese scholars traditionally had regarded science, or at least the subject matter which, in the West, had become part of science, as something unworthy of their attention. For them, "there was no question of science being at all profound" or anything other than some sort of "coarse and rude" adjunct to "arts and crafts." What was valued was only China's own literary culture, "the great principles which ruled the kingdom and tranquilized the empire and which were entirely sufficient for self-strengthening."[40]

This view, which did not regard science as a part of scholarship at all, had persisted, it seemed to the Science Society, so that even in the late 1910's and early 1920's, "many people in China [still regarded] science and learning as two distinct things...and therefore [did] not understand the importance of science for thought and for research."[41] This "partial and incomplete" view of culture had kept China's scholars from ever feeling it necessary to have "true and accurate knowledge of the natural world,"[42] or even to develop methods for obtaining such knowledge. Chinese thinkers had not understood that, in contrast to their own empty and fanciful speculations, the pursuit of science was "not a matter of sitting quietly and alone for a hundred years, with science being arrived at and then departing without a trace." Furthermore, because the Chinese had not understood that the methods of science were accessible in a way that enlightenment by inspiration was not, they had become "accustomed to being frightened" by science, imagining that it was "unusually profound and mysterious." They had understood neither that it was the "most ordinary of things" nor that it was "not necessary to be a Newton to be able to speak scientifically."[43]

To the Science Society, all of this implied that to successfully promote science, and especially its research methods, to show that science was not to be treated as "an ornamental and unnatural thing which could accordingly be dismissed and disregarded,"[44] required a basic redefinition of what constituted learning and scholarship, an ideological attack against what the development of science in the West so clearly showed to be the "decay and atrophy" of Chinese intellectual life. This critique of traditional knowledge was not just an exercise in philosophical disputation, but had a very real social dimension as well. It was argued that the Confucian scholars had constituted a "non-agricultural, non-commercial, and non-laboring class," and that although these men "styled themselves as sages," they and their knowledge were essentially without redeeming social value or importance. But despite all the changes and crises that China had gone through, which should have exposed the bankruptcy of such men, it seemed to the Science Society that the country's students and intellectuals still held to the "customs of former scholars."[45] This meant that any critique of traditional knowledge had to include a systematic effort to break the identification of education and scholarship with traditional careers and roles. It was only in that way that careers in science could be established as legitimate for men of knowledge and that non-scientist intellectuals and students could be recruited to the cause of science.

It was to this group, the intellectuals and the students, that the Science Society primarily addressed its arguments about the need to change the definition of the social role of the man of knowledge.[46] The basic contention was quite simple. Science had advanced in the West because scientific research had been regarded as "the calling of students and scholars." These men had been willing to give up "prosperity and happiness in their societies and sacrifice themselves to go deeply into research." The same sort of thing, it was argued, had to occur in China if science were to be made to prosper. Chinese intellectuals had to forego orthodox careers and the status that had gone with them, and had to accept new definitions of the ways in which they could best contribute to the renovation of Chinese society and culture. In particular, they had to come to see the overriding importance of taking scientific research to be their "calling," of making use, that is, of scientific methods in whatever they were doing.[47]

In a sense, this redefinition of the role of the man of knowledge was what the Science Society's extensive polemics were all about. The view of science which the Society advanced, with its emphasis on science's cultural and intellectual significance, directly related science to the crisis that confronted China, at least as it was perceived by the country's new intellectual leaders. It seemed to show that science could serve as the basis for a new system of values to replace Confucianism, that science could, in other words, provide the "firm and stable foundation" that Chinese society needed. For a while, this argument made science ideologically central to the concerns of the new intelligentsia, which was itself pre-occupied with the search for such a foundation; as a result, for the first time, basic science began to be esteemed by a significant fraction of China's educated class, and the number of men who did in fact choose science as their "calling" began to increase.[48]

All this was clearly important, but the leaders of the Science Society were well aware that just attracting men to science was not, in itself, enough to make science flourish in China. The example of the West had shown them that a variety of scientific institutions had to be created, not only to provide scientists with the materials and facilities necessary for research, but also to serve both as "refuges" where the scientist could pursue his research without being distracted by non-scientific claims on his energies, and as settings where the characteristic values and habits of mind of the scientist could be cultivated and reinforced.

Even while the Science Society was still officially located in America, its leaders had regarded this problem as critically important,[49] and their concern was sharpened by their experiences upon returning to China. Although forty of the fifty-one scientists active in the Society during the early 1920's found jobs that ostensibly made direct use of their scientific training, thirty-two of these positions were in the academic world,[50] and, at that time, China's colleges and universities were not places in which any sort of real scientific work could be done. Even in the larger universities like *Peita* (Peking National University) or National Southeast University (later to be called National Central University), facilities for research were generally lacking, while the faculties had little time for such things anyway.[51] And places like *Peita* and National Southeast were themselves atypical. Nearly half of the scientists active in the Society were scattered in ones and twos among a large numbers of colleges in Shanghai, Nanking, and Peking, most of which were quite small, and all of which suffered from chronic financial problems well into the mid-1920's.[52]

In addition to such financial difficulties and the institutional fragmentation which existed in the three major academic centers – Shanghai, Nanking, Peking – where nearly sixty percent of the Society's 276 members resident in China were to be found in 1920, the remaining forty percent were badly isolated from each other geographically, living as they did in provinces where there were usually fewer than ten members all told, and this made for problems in just maintaining contacts between the members.[53] In addition, there was a constant danger that the Society, like a number of other scientific organizations that had come into being during this period, would allow itself to be "circumscribed by the prejudices" which separated the various returned-student communities and thereby become essentially an "assemblage of returned students" and not a true scientific body.[54]

The Science Society did make a conscious effort to overcome the prejudices which divided the returned-student communities and actively recruited scientists trained in Europe as well as those trained in the United States.[55] The Society was also more or less able to overcome the problem posed by geographical dispersion by holding its annual meetings in different cities each year, thereby giving scientists "from various geographical regions an opportunity to meet together and discuss common scientific and occupational issues."[56]

The more fundamental aspects of the problem of maintaining real cohesion and developing a sense of identity within China's small scientific community, in the absence of those institutional mechanisms which held the scientific communities of the West together, could not be solved so directly and quickly. It was clear that it would take some time to create the requisite institutional framework, and it was equally clear that, in the meantime, some substitute for these institutions had to be found. At the very minimum, the "scientific spirit" had to be maintained, even if scientific research itself could not be carried on, and this the Science Society tried to do, even as it also worked to provide, with its library, museum, and Biological Research Laboratory, at least a model for research facilities. Thus, although upon their return to China, the leaders of the Society "took up jobs and consequently had no thoughts for questions of research," they sought to keep "one issue above all others in [their] minds," and that was to sustain "the appetite for research."[57] To this end, during the years immediately following the Society's official move back to China, its polemics came to include a clearly reflexive component, and its leaders became increasingly concerned to define and promote a "scientific style of life," the acceptance of which would, it was hoped, reinforce the commitment of the Society's members to the scientific enterprise at a time when there were both few opportunities for them actually to do science and strong pulls toward more traditional roles for men of knowledge.

The clearest expression of the Science Society's interest in articulating the norms and values of a scientific life-style is to be found, reasonably enough, in the articles which its spokesmen contributed to the sprawling debate on "Science and the View of Life" *(K'o-hsüeh yü jen-sheng-kuan)* or "Science and Metaphysics" *(K'o-hsüeh yü hsüan-hsüeh)*, which kept Chinese intellectuals occupied during most of 1923.[58] The general issues that were raised in the debate were ones, however, that had figured conspicuously in numerous articles in *K'o-hsüeh* over the years before the controversy itself actually erupted. The Society's President H. C. Zen, for example, had discussed, in an article in one of the first issues of the journal, the effects which the adoption of those ways of thinking characteristic of the scientist had on the individual, and he had concluded, not surprisingly, both that scientists were more than reasonably moral men, and that this was because of, not in spite of, their scientific training.[59] Similarly, Ho Lu, a French-trained mathematician, had argued, in a later essay, that science was the "door to virtue" and that it could bring about "happiness and good fortune." Scientific training and the experience of doing scientific research taught men the value of being sincere and honest, of being able to bear suffering, of being self-content and submerging their personal desires, all because the scientific enterprise was itself arduous, did not always produce immediate results, and was not regarded by its practitioners as something from which they could expect private gain.[60]

The point underlying Ho's discussion, that becoming and being a scientist not only involved understanding sets of facts and laws and making use of a rigorous and logical method, but also required the acceptance of a distinctive complex of norms and values, was central to the ruminations which members of the Science Society contributed to the "Science and Metaphysics" debate proper. It seemed to them that the question posed by their main antagonist, Chang Chün-mai, – "can science govern a view of life?" – was somewhat beside the point, since it was clear, to them at least, that as an empirical fact, it did, and that, as the scientist Wang Hsing-kung remarked, scientists had characteristic attitudes toward questions about human life which set them off from other classes of men.[61]

For the scientists who participated in the debate, the crucial distinction which had to be made, and one which Chang Chün-mai did not make, was between science as a systematic methodology and science as a body of established facts and laws. Thus, the leading defender of science, the geologist V.K. Ting – who at this time was, among many other things, a member of the Science Society's Executive Committee – argued that it was not what the scientist knew that made him moral, but rather the habits of mind that he cultivated in the process of obtaining his knowledge.

> The daily search for truth and elimination of dogmatism...not only [gives] the scientifically educated man the capacity to seek true principles, but also a sincere love for them.[62]

The general argument, of which Ting's statement is only an example, that scientific activities give rise directly to a recognizable view of life, was worked out in detail not only by Ting but by several other scientists. In H.C. Zen's view, for instance, the object of the scientist's research was to "seek true principles," but because such principles are "inexhaustible and boundless," the scientist learns always to be prepared for new discoveries, and, as a consequence, his views generally come to be progressive. He will "not believe that the past has gotten hold of the best view of life." Too, because the scientist has learned in the course of his work to use only "skillful and accurate observations" and "careful and detailed theories," he becomes skeptical of beliefs that are supported only by "eminent, ancient, and important tradition" and does not hesitate to make "declarations of war against either the famous theories of his predecessors or the prejudices of society." Moreover, because the "spirit" of scientific research is "deep, extensive, and without bounds," the scientist comes to see that "partial views and private prejudices" which contradict that universalistic animus are to be rejected and that distinctions in terms of "fame, honor, and even social class" have no place in his scheme of things. Finally, because the scientist is in fact dedicated to science, he puts the advance of knowledge ahead of personal gain; and, because his pursuit of truth is, in that sense, disinterested, he is able to "break through the many seductions and temptations of the material world."[63]

This whole argument, that the experience of doing science produces "admirable, great, and high-minded views of life" among equally "high-minded and magnanimous" scientists,[64] was, as the statements of Ho Lu and V.K. Ting discussed above should suggest, not one that Zen hit upon all by himself. In fact, it seems to have been rather common among the spokesmen for the Science Society. Thus, to take one other instance where the reasoning was worked out in detail, in an article on "The Scientific View of Life," which appeared in *K'o-hsüeh* before the larger controversy itself was under way, Yang Ch'üan produced a series of arguments quite similar to Zen's and directed toward the same conclusion, namely that "being in the laboratory truly is sufficient to nourish the greatness of one's soul."[65]

Like Zen, Yang argued that the scientist "takes the search for true principles to be a life-time activity" and expects no other reward for his efforts but "the advancement of science." In his work, the scientist comes to value scepticism, to believe in progress, to be universalistic in his outlook, and to seek truth disinterestedly. As a result, the scientist does not accept beliefs about non-scientific matters on their face, but "maintains a skeptical attitude," making no "premature judgments," and always remains ready to "change his mind in the face of new evidence." In his dealings with other men, the scientist neither has different standards for "the strong and the weak," nor does he judge men and their ideas on the basis of their "religion, class, or nationality." Too, the scientist has "no desire for wealth and glory"; he does not use his discoveries to improve his own station and does not direct his work to achieve fame for himself.[66]

As descriptions of the norms and values that scientists as a group accept, Zen's and Yang's essays are not particularly exceptional. The ethos of science does include "moral as well as technical prescriptions," imperatives toward universalism, organized skepticism, disinterestedness, and a belief in scientific progress,[67] but in established scientific communities, a well-institutionalized socialization process produces scientists who "are so strongly committed to the central values of science that they unthinkingly accept them," and because these commitments are constantly reinforced by equally well-institutionalized mechanisms operating within the community, scientists in such settings generally feel no need to expand on them at any great length in public.[68] Since Yang and Zen, like the active members of the Science Society generally, had been trained in the West and had, as graduate students, no doubt participated in some sort of research projects, since they had, that is, experienced the socialization process which turns out scientists, it is not surprising that they accepted a version of the scientific ethos.[69] What is unusual about Zen and Yang, as well as Ting, Ho, and other spokesmen for the Science Society, thus, is not that they found moral prescriptions in the scientific enterprise, but that they made an issue of them and felt compelled to articulate the norms and values of science in public, that they felt compelled to argue, in other words, that scientific activities were "sufficient to nourish the greatness of one's soul."

There are several reasons for this. For one thing, as I have already argued, those institutional mechanisms which, in the West, reinforce the individual scientist's commitment to the normative structure of science were, by and large, simply not present in China in the late 1910's and early 1920's, and, a related point, there was no adequately defined social role for the scientist either. In this situation, the polemical insistence on the worth of a "scientific view of life" or a scientific life-style served as a sort of ideological discipline for the members of the Science Society and for scientists in China generally. Thus, in the whole controversy, it was only the scientists, and not even their non-scientist allies, such as Hu Shih or Wu Chih-hui, who made the particular claim that it was the methods of science, the norms and values built into the scientific activity, that were important, and not the results of that activity or the imperatives that could be inferred from particular scientific theories and facts.[70] This particular emphasis not only underlined the significance of accepting the norms and values of science, but, taken together with the insistence on the distinctiveness of both these norms and values and the scientific method generally, also helped to bring the members of the Science Society to see themselves as scientists first, and as returned students, academics, government officials, or whatever, only secondarily. That is, in the absence of any institutionalized definition of the scientist's social role, the insistence on the distinctive nature of the scientific life-style served to set the scientist off from other types of men of knowledge, defining science as an activity recognizably different from other intellectual enterprises and, in effect, stating what the scientist should be, what he should do, and where his primary commitments should lie. Given both the persistence of the identification of the social role of any man of knowledge with a political or governmental career and the strength of the "prejudices" which segregated the various "nations" of returned students off from one another, it was obviously very much to the point to dissolve the old allegiances that fragmented the small community of scientists in China and to establish the scientist's commitment to science as primary. That the articulation of the scientific ethos contributed positively to this process and, not incidentally, to strengthening the Science Society as well, was understood quite clearly.

> The Science Society's present activities can be reported to our colleagues as follows. At a time when the importance of science has not yet been understood by the people of our country, [we] ... still hasten to transact our business and to display a few efficacious results. [We] are to be regarded as recognizing our common beliefs, as nursing them, but not taking the present state of affairs as the end result. [71]

The insistence that the acceptance of these "common beliefs," of a scientific view of life, would produce high-mindedness, magnanimity, and other virtues, suggests a second general point. The whole polemic against Chang Chün-mai's position was, of course, explicitly intended to show that science did have moral and ethical implications, and in the context of the early 1920's, this claim was one that it seemed urgently necessary to establish. As Yang Ch'üan put it, a view of life had to define goals for people; without a coherent philosophy, they would be unable to choose among the "myriad callings" that were suddenly being opened up to them. They would be "irresolute and without direction" and would never be able to accomplish anything. Such choices had been brought on and made difficult by China's contact with the modern world, and they thus constituted singularly immediate problems for that generation which had come to maturity in the midst of what promised to be an enduring crisis of basic beliefs. The traditional standards and values which had guided the lives of men in the past were no longer relevant to the lives of that generation, and new ones had not yet been established. "Our ancestors have said that not to know everything is a shameful thing for the Confucian scholar,... but this [attitude] is impossible for us now."[72] It was thus important to show that science, its methods and moral prescriptions, could meet the needs for self-control, discipline, definition of appropriate activities, and so on, that had arisen as traditional controls gave way and old statuses and privileges were called into question. For the leaders of the Science Society themselves, science seems to have done these things, to have, in Yang's felicitous phrase, nourished the natural greatness of their souls.

This all leads to a third and final point. By publicly proclaiming the ethical and moral significance of scientific activity, the Science Society clearly intended to relate science and scientists to what their fellow intellectuals perceived to be the fundamental problem facing China, the collapse of Confucian culture and institutions and the failure of a new "strong center" for Chinese society to emerge. If the norms and values of the scientific enterprise could be shown to be serving effectively as an ideological discipline for scientists, the implication was that they also could so serve for the new intelligentsia generally.[73] Too, by showing that the scientific communities of the West were successful in large part because of the acceptance by scientists of a distinctive system of moral prescriptions, the Science Society's spokesmen were able to contend that if China were to accept that system of values, if, in other words, Chinese society were to model itself on the scientific community, then it would also be able to function successfully.

In all of these ways, the public articulation of the norms and values of the scientific community served as part of the Science Society's general effort to break, once and for all, the association of science with mere technology and to provide an ideological framework within which the specifically scientific activities of scientists could receive certain legitimation from the larger society. In the past, men of knowledge had had, as scholars and as officials, a virtual monopoly on status in Chinese society, and in the twentieth century, the new intelligentsia had inherited some of that prestige, so it was very much to the point both to show that science was a legitimate concern for men of knowledge and not just for technicians of one sort or another and to establish science's claim to be, in fact, the appropriate replacement for Confucian scholarship as the central business of Chinese thinkers.

* * * *

The Science and Metaphysics controversy was something of a watershed for the Science Society and for Chinese scientists generally. Thus, while, as I have already noted, about a fourth of all the articles appearing in the Society's journal between 1915 and 1923 were given over to a distinctly ideological insistence that science was the only source for a satisfactory view of life, that it was the only possible "strong center" for a truly modern society, and so on, in succeeding years, interest in such subjects fell off precipitously. By 1935, only about ten percent of the articles dealt with general issues related to science's place in society and culture, and even in these, the emphasis was markedly different from what had gone before. As early as 1924, immediately after the end of the Science and Metaphysics controversy, the Science Society had announced that it was time

> to destroy the blind following of science and the low practices of adherents to science and instead [to cause the true spirit of science] ... to be clarified greatly, to invite those who have promoted science to comprehend their own failings, to understand that empty theorizing encourages mere talk, argument, and useless investigations...[74]

What was needed was for scientists to stop "amusing themselves with empty talk" and to devote themselves to "scientific experimental research."[75]

This was a theme that, by the 1930's, dominated the Science Society's official pronouncements. Between that time and the early 1920's, what had happened was that the quasi-ideological issues about the nature of science and its methods and about its place in Chinese society and culture had been at least temporarily resolved with the emergence of an organizational complex that could support the scientific activity and give some institutional definition and legitimation to a social role for the scientist as scientist. As science became an established and recognized activity, with its own socializing mechanisms, it became less important constantly to define and emphasize in public what a commitment to science meant, to use an explicit ideology, that is, to keep alive the spirit of scientific research among those with scientific training and to recruit those without it to science.

FOOTNOTES

1 See, for example, D.W.Y. Kwok, *Scientism in Chinese Thought, 1900-1950* (New Haven: Yale University Press, 1965), *passim.*

2 This argument is developed in detail in Everett Mendelsohn and Peter Buck, "The Formation of Scientific Communities," unpublished draft, Harvard University, spring 1969.

3 Joseph Needham, *Science and Civilization in China* (Cambridge, Cambridge University Press, 1954-).

4 See Knight Biggerstaff, *The Earliest Modern Government Schools in China* (Ithaca, New York: Cornell University Press, 1961), ch. I; John K. Fairbank, "China's Response to the West: Problems and Suggestions", *Journal of World History,* III:2 (1956), 384; and Edwin O. Reischauer, "Modernization in Nineteenth Century China and Japan", *Japan Quarterly,* X:3 (1963), 303. These issues will be treated at greater length and with appropriate caveats and qualifications in my Ph.D. dissertation (Cambridge, Mass.: Harvard University, 1972).

5 For a general discussion of the Imperial Examination System, see Wolfgang Franke, *The Reform and Abolition of the Traditional Examination System* (Cambridge, Mass.: Harvard University Press, East Asian Monograph, 1963).

6 Chow Tse-tsung, *The May Fourth Movement: Intellectual Revolution in Modern China* (Stanford, Calif.: Stanford University Press, 1960), p. 147.

7 Those men of knowledge who were not themselves in the government civil service generally owed what positions they did have, as teachers, secretaries in government yamens, or private tutors in scholar-official households, to the favor of some local official and were almost totally dependent on his continued patronage for their livelihood. See David S. Nivison, *The Life and Thought of Chang Hsueh-ch'eng, 1738-1801* (Stanford, Calif.: Stanford University Press, 1966), pp. 10-11.

8 Benjamin Schwartz, "The Intelligentsia in Communist China: A Tentative Comparison", in *The Russian Intelligentsia,* ed. Richard Pipes (New York, N.Y. : Columbia University Press, 1961), pp. 176-77.

9 This and other generalizations about the social origins, educational backgrounds, and

subsequent career patterns of Chinese scientists are based on an examination of biographical information on fifty-one active members of the Science Society of China in the early 1920's. This material is discussed in detail in my Ph.D. dissertation.

10 Wu Cheng-lo, "Ch'üan-kuo k'o-hsüch chiao-yü she-pei kai-yao" (A General Survey of Scientific Laboratory Equipment in Different Schools and Colleges in China), *K'o-hsüeh* (Science), IX:8 (1924), 950-52 (hereafter, cited as *KH*).

11 Hu Shih, for example, has written that he had originally intended to study agriculture, in accordance with "the belief then current in China, that a Chinese [had to] learn some useful art" (Hu Shih, "My Credo and its Evolution", *Living Philosophies* [New York: Simon and Schuster, 1931], p. 251), and Chiang Monlin, who was later to be a prominent educator, recalled that his early decision also to study agriculture, although soon abandoned, "was not a happy-go-lucky move." Agricultural improvements, he had felt, "would bring happiness and prosperity to the largest number of people in China," so that for "national as well as personal reasons . . . agriculture seemed the most appropriate study to pursue" (Chiang Monlin, *Tides From the West* [New Haven: Yale University Press, 1947], pp.70-71). Similarly, but more generally, out of 168 members of the Science Society in 1916, seventy-three listed their primary field of interest as one of the applied sciences, mining, metallurgy, or engineering, and another fifteen, agriculture and forestry, while only forty listed themselves as primarily interested in the more basic sciences ("Shu-chi pao-kao" [Report of the Secretary], *KH*, III:1 [1917], 100).

12 Even with this attack, a very real concern for the relationship between science and the material well-being of the nation remained. During the years between 1915 and 1925, some twenty to twenty-five percent of all the articles in the Society's journal, *K'o-hsüeh,* were devoted to questions having to do with the applied sciences, and remarks about the "mutual interdependence" of such things as science, commerce, and industry were almost a commonplace, as were complaints about China's continuing failure to make that "interdependence" a reality.

13 I should emphasize that I am only claiming here that this view of science was current within the American universities and not that it was widely held in the larger society. See Laurence R. Veysey, *The Emergence of the American University* (Chicago, Ill.: Phoenix Books, 1970), ch. III.

14 Jen Hung-chün (H.C. Zen), "Chung-kuo k'o-hsüeh-she erh-shih-nien chih chiung-ku" (Reflections on Twenty Years of the Science Society of China), *Chung-kuo k'o-hsüeh erh-shih-nien*(Twenty Years of Chinese Science) (Shanghai: 1937), p. 1.

15 Jen Hung-chün, "Hsüeh-hui yü k'o-hsüeh" (Learned Societies and Science), *KH,* I:7 (1915), 710.

16 Ping Chih, "Ch'ang she hai-pin sheng-wu shih-yen-suo shuo" (A Proposal for the Establishment of a Marine Biology Experiment Station), *KH,* VIII:3 (1923), 307-9.

17 Ts'ai Yuan-p'ei, Chang Chien, Ma Liang, Wang Ching-wei, Fan Yuan-lien, and Liang Ch'i-ch'ao, "Pen she ch'ing po p'ei-k'uan kuan-shui shang cheng-fu shuo-t'ieh ping chi-hua-shu" (Budget of the Science Society and a Request that the Government Allocate Funds to the Society from the Indemnity and from Customs Duties), *KH,* VIII:2 (1923), 192-93. These six men were all members of the Science Society's Board of Directors.

18 Jen Hung-chün, "Hsüeh-hui yü k'o-hsüeh", p. 710.

19 Jen Hung-chün, "Chieh-huo" (To Allay Suspicions), *KH,* I:6 (1915), 608-9.

20 Jen Hung-chün, "Chung-kuo k'o-hsüeh-she erh-shih-nien chih chiung-ku", p. 1.

21 Jen Hung-chün, "Wai-kuo k'o-hsüeh-she chi pen she chih li-shih" (The History of

Foreign Science Societies and of Our Society), *KH,* III:1 (1917), 15-16.
22 The Society, which from its inception had been dominated by American returned students, made a conscious and generally successful effort to bring in returned students from European nations as members and thus make itself a truly national organization. Membership in the Society rose from 55 in 1914 to 503 in 1920, 728 in 1925, 1,005 in 1930, and 1,949 in 1937. In 1922 the Society opened a Biological Research Laboratory and Museum in Nanking, and with it as a model, drew up further plans "to establish all kinds of research centers for scientific experiments and tests designed to aid progress in crafts, industry, and public affairs" (Jen Hung-chün, "Chung-kuo k'o-hsüeh-she chih kuo-ch'ü, chi chiang-lai" [The Past and Future of the Science Society of China], *KH,* VIII:1 [1923], 3-4). Only a few of these schemes ever really came to anything, but in 1930, the Society did establish, at the request of the Nanking government, a Bureau for Scientific Information. Too, a major new science research library was built in Shanghai in 1931, which by 1937 had over 10,000 science books and was receiving more than 100 foreign and Chinese learned journals, and in 1934, the Biological Research Laboratory – which by then was already a major reserach center – was expanded to include departments of physiology and bio-chemistry ("Chung-kuo k'o-hsüeh she ta-shih chi-yao" [The Science Society of China: Summary Record of Important Events], *KH,* XX:10 [1936], 842-43).
23 For these other organizations, see Ts'eng Ch'ao-lun, "Chung-kuo k'o-hsüeh hui-she kai-shu" (Summary Account of Chinese Scientific Societies), *KH,* XX:10 (1936), 798-810.
24 "Fa-k'an-tz'u" (Forward), *KH,* I:1 (1915), 3.
25 *Ibid.,* 6.
26 See, for example, the speeches by Hu Ta, a mathematician, Ma Liang, a former Ch'ing scholar, and Liu I-sheng, an historian, made at the Society's fourth and seventh annual conventions, *KH,* V:1 (1919), 107; *KH,* VII:9 (1922), 990, 999. See also articles by the Society's President, Jen Hung-chün (H.C. Zen), "K'o-hsüeh yü kung-yeh" (Science and Industry), *KH,* I:10 (1915), 1099, and by another of its founders, Yang Ch'üan, "K'o-hsüeh ti jen-sheng-kuan" (A Scientific View of Life), *KH,* VI:11 (1921), 119.
27 "Fa-k'an-tz'u", p. 6.
28 See, for example, "Fa-k'an-tz'u", pp. 5-6, where the quote is used to explain the argument that "the direct effects which science has on the material world have in turn an indirect effect on morality." Similarly, Yang Ch'üan cited it in the context of his contention that real "humanitarianism" had only come about with the development of western material culture, while all that China's vaunted humanism had been able to produce was "an oppressive morality." See, Yang Ch'üan, "Kung-ch'eng-hsüeh yü chin-shih wen-ming" (The Study of Engineering and Modern Culture), *KH,* VIII:2 (1923), 112.
29 Hu Ta, Speech at the Fourth Annual Convention, 107.
30 "Fa-k'an-tz'u", pp. 3-6. It seemed to the members of the Science Society that the history of the West since the birth of modern science proved that the success a nation had, not only in "ordering its affairs of state" and in providing for the prosperity of its people but also in achieving a high level of culture all depended upon its ability to produce and make use of science (Jen Hung-chün, "K'o-hsüeh-chia-jen shu yü i kuo wen-hua chih kuan-hsi" [The Relationship of the Number of Scientists which a Nation has to the Level of its Culture], *KH,* I:5 [1915], 487-88). Thus, "human progress ... at

all times and in all places has relied on science," and the "accumulated weaknesses" of China were therefore a result of "not understanding science." Without the scientific spirit, "the sound basis for organizing anything," it was inevitable that innumerable difficulties would continue to arise and that "the successful completion of large-scale undertakings would continue to elude China" (Hu Tun-fu and T'an Chung-k'uei, Speeches at the Fourth and Seventh Annual Conventions of the Science Society, *KH,* V:1 [1919], 106, and *KH,* VII:9 [1922], 982). That is, it seemed to the Science Society that there was simply "no question" but that "without scientific knowledge" it would be impossible to establish the nation in the modern world" (Chu K'o-chen, Speech at the Fourth Annual Convention of the Science Society, KH, V:1 [1919], 110).

31 Chao Ch'eng-ku, "K'o-hsüeh chih shih-li" (The Power of Science), *KH,* VIII:6 (1923), 581-84.

32 Jen Hung-chün, "K'o-hsüeh yü chin-shih wen-hua" (Science and Modern Culture), *KH,* VII:7 (1922), 629-40.

33 Ping Chih, Speech at the Opening of the Science Society's Biological Research Laboratory, *KH,* VIII:8 (1923), 846.

34 Chow Tse-tsung, *The May Fourth Movement,* 58-61.

35 Jen Hung-chün, "Chung-kuo k'o-hsüeh she chih kuo-ch'ü chi chiang-lai", p. 2.

36 For example, "from ancient times science has not been given a happy welcome in China. Despotic rulers have used their great powers [against science] and have caused a policy of treating morality as superior to great skills to be transmitted and proclaimed to the people. Since only the insane, who treat their lives lightly, would run counter to the wrath of the rulers [and thereby] suffer unfathomable calamities, even those with the natural gifts of the scientist took up different pursuits ... [and] popular opinion came to regard science as something with which a gentleman should not be concerned. Consequently, the number of people who have devoted themselves to science has been very small" (Yeh Chien-po, "K'o-hsüeh ying-yung lun" [The Applications of Science], *KH,* III:2 [1917], 135).

37 Hu Ta, Speech at the Fourth Annual Convention of the Science Society, p. 107, and Jen Hung-chün, "Chieh-huo", p. 607.

38 Chu K'o-chen, "Wo kuo ti-hsüeh chih tse-jen" (The Responsibilities of our Country's Earth Science), *KH,* VI:7 (1920), 674.

39 Chang Chien, Speech at the Seventh Annual Convention of the Science Society, *KH,* VII:9 (1922), 991.

40 Liang Ch'i-ch'ao, "K'o-hsüeh ching-shen yü tung hsi wen-hua" (The Scientific Spirit and the Cultures of East and West), *KH,* VII:9 (1922), 860.

41 Ching Tzu-yuan, Speech at the Fourth Annual Convention of the Science Society, *KH,* V:1 (1919), 107.

42 Jen Hung-chün, Speech at the Sixth Annual Convention of the Science Society, p. 1059.

43 Yeh Chien-po, "K'o-hsüeh ying-yung lun", pp. 137-8.

44 *Ibid*., 140.

45 Fan Ching-shen, Speech at the Seventh Annual Convention of the Science Society, *KH,* VII:6 (1922), 623-24.

46 The Science Society was quite conscious of the need for the "abundant sympathy" and support of non-scientists. Such men as Ts'ai Yuan-p'ei, Wu Chih-hui, Liang Ch'i-ch'ao, Ma Liang, Wang Ching-wei, and Sun Fo were recruited as early members, and in 1922, Ts'ai, Liang, Ma, and Wang, together with Chang Chien, Hsiung Hsi-lin, Yen

Hsiu, Fan Yuan-lien, and Hu Tun-fu were brought together to form the Society's first Board of Directors. The backgrounds of these nine directors gives clear evidence of where the Society was seeking "abundant sympathy," for, with the exceptions of Hsiung and Chang, all of them were either academicians or noted intellectuals, and Hsiung and Chang were both important financial patrons of educational reform. Too, in one way or another, the careers of all of these men represented major breaks with tradition and provided examples of how significant contributions could be made to China's future by men who did not limit their vision to bureaucratic occupations. As a group, these men, all prominently associated with important reform movements, could not help but give the Science Society, as well as science, added status and prestige with the new academic and intellectual elite that was emerging in China and from which the Science Society in fact did draw the large majority of its non-scientist members.

47 Jen Hung-chün, Speech at the Sixth Annual Convention of the Science Society, p. 1062.

48 Chang I-tsun, "Chung-kuo ti hua-hsüeh" (Chinese Chemistry), *Chung-hua min-kuo k'o-hsüeh chih* (Science Record of Republican China) (Taipei: 1955), vol. I, p. 2.

49 See, for example, Yang Ch'üan, Speech at the Seventh Annual Convention of the Science Society, *KH*, III:1 (1917), 120-21.

50 I discuss the careers of the early members of the Science Society in detail in my dissertation. See above, note 4.

51 Li Shu-hua, *Chieh-lu chi* (Records from the Hut on Chieh-shih Mountain) (Taipei: Chuan-chi wen-hsüeh ch'u-pan-she, 1967), p. 48.

52 For this and other information on China's colleges and universities, see Earl Herbert Cressy, *Christian Higher Education in China: A Study for the Year 1925-26* (Shanghai: China Christian Educational Association, 1928), pp. 299-301. See also Alan Bernard Linden, "Politics and Higher Education in China: The Kuomintang and the University Community, 1927-38" (Unpublished doctoral dissertation, Columbia University, 1969), pp. 42-46, and *Ch'üan-kuo kao-teng chiao-yu t'ung-chi* (National Higher Education Statistics) (Nanking: Ministry of Education, 1931), *passim*. The faculties of all these institutions were also quite small. According to Cressy's data, only six of forty-one of these places had more than 100 faculty members, and if Cressy's statistics on the number of semester hours taught are any guide, it would seem that considerably less than one-third of the teaching staffs were involved with science at any of the institutions. Li Shu-hua, for example, indicates that out of eighty professors at *Peita* in 1922-23, only twenty-six were in the science departments (Li Shu-hua, *Chieh-lu chi*, pp. 80-82).

53 The failures of other organizations similar to the Science Society suggest how difficult it was to hold together an association of would-be scientists. For example, at the same time as the Science Society was organized, Chinese students in England had formed a general scientific organization of their own, whch initially had more members than did the Science Society. When its members returned to China, however, "they scattered themselves like stars," their organization rapidly lost all of its cohesion and was soon disbanded, and its members joined the Science Society. See Chien Pao-tseng, Speech at the Twelfth Annual Convention of the Science Society, *KH*, XII:11 (1927), 1620.

54 Ts'eng Ch'ao-lun, "Erh-shih-nien lai chung-kuo hua-hsüeh chih chin-chan" (Advances of the Last Twenty Years in Chinese Chemistry), *Chung-kuo k'o-hsüeh erh-shih-nien*, p. 112.

55 See above, note 22.

56 Jen Hung-chün, Speech at the Eighth Annual Convention of the Science Society, *KH,* VIII:10 (1923), 1108. The idea of peripatetic meetings was probably borrowed from the American Association for the Advancement of Science.
57 Hu Shih, Speech at the Eighth Annual Convention of the Science Society, *KH,* VIII:10 (1923), 1109-10.
58 The term "view of life" (*jen-sheng-kuan*) is one of those protean notions that means something different to everyone and nothing very precise to anyone. Carsun Chang (Chang Chün-mai), who provoked the debate and whose term it was, had been a student of Rudolf Eucken, and he intended *jen-sheng-kuan* to be a translation of the German word *Lebensanschauung,* as in Eucken's book *Lebensanschauungen der grossen Denker.* What Eucken meant by *Lebensanschauung* is not much clearer than what Chang meant by *jen-sheng-kuan.*
59 Jen Hung-chün, "K'o-hsüeh yü chiao-yü" (Science and Education), *KH,* I:12 (1915), 1348.
60 Ho Lu, "K'o-hsüeh yü ho-p'ing" (Science and Peace), *KH,* V:2 (1919), 119-24, and *KH,* V:4 (1919), 325-29.
61 Wang Hsing-kung, "K'o-hsüeh yü jen-sheng-kuan" (Science and the View of Life), *K'o-hsüeh yü jen-sheng-kuan* (Science and the View of Life) (Shanghai: 1923), vol. II, article 10, p. 16. For Chang Chün-mai's views, see "Jen-sheng-kuan" (View of Life), *K'o-hsüeh yü jen-sheng-kuan,* I, article 1.
62 Ting Wen-chiang (V.K. Ting), "Hsüan-hsüeh yü k'o-hsüeh" (Metaphysics and Science), *K'o-hsüeh yü jen-sheng-kuan,* vol. I, article 2, pp. 20-21.
63 Jen Hung-chün, "Jen-sheng-kuan ti k'o-hsüeh ho k'o-hsüeh ti jen-sheng-kuan" (A Science of Life-views or a Scientific View of Life), *K'o-hsüeh yü jen-sheng-kuan,* vol. I, article 6, pp. 6-8.
64 *Ibid.*, p. 5.
65 Yang Ch'üan, "K'o-hsüeh ti jen-sheng-kuan", p. 1118.
66 *Ibid.*, pp. 1112-17.
67 Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Illinois: The Free Press, 1968), pp. 606-615, and John Ziman, *Public Knowledge: The Social Dimension of Science* (Cambridge, England: Cambridge University Press, 1968), pp. 96-101. See also Warren O. Hagstrom, *The Scientific Community* (New York: Basic Books, 1965), ch. 1.
68 Hagstrom, *The Scientific Community,* pp. 9, 12.
69 This indicates in a particularly concrete fashion how important differences in educational experience can be. For it is clear that, in general, Chinese students in the West did not go through this specific socialization process, while very many of the members of the Science Society did. Thus, out of fifty-one active members living in China in the early 1920's who had some scientific training and about whom I have been able to find information, all of those who had gone to America, thirty-nine, had received at least a bachelor's degree by the time they had returned to China. In addition – and this is what is significant – two-thirds of them had gone on to do graduate work, obtaining among them thirty-one advanced degrees, including eleven Ph.D's. (The records of those who studied in Europe were about the same. All took degrees, and eight of the ten had advanced degrees of one sort or another. Neither of the two Japanese-trained members received more than the equivalent of an American bachelor's degree.) In contrast, it seems that at least half of the Chinese studying in America before the 1930's received no degree at all, and certainly the great majority of them never proceeded beyond the bachelor's degree (Y.C. Wang, *Chinese Intellectuals and the West, 1872-1949* [Chapel Hill: University of North Carolina Press, 1966], pp. 167, 185). The

academic accomplishments of the Science Society's members are even more striking when they are compared to those of American students as a whole. In 1900, there were 237,592 candidates for undergraduate degrees but only 5,668 graduate students in the entire United States. If, as has been argued, these statistics on American students show "how extremely few" of them had any inclination to associate themselves with the work of academic institutions, then the comparable figures for members of the Science Society, it would seem, lead to the opposite point, that a disproportionately large number of them ended their undergraduate careers sharing the values of those Americans who were committed to specifically academic work, and in particular, of those who were committed to scientific research. See Laurence R. Veysey, *The Emergence of the American University,* p. 269.

70 For a discussion of Hu's and Wu's contributions to the great debate, see Charlotte Furth, *Ting Wen-chiang: Science and China's New Culture* (Cambridge, Mass.: Harvard University Press, 1970), pp. 118-26.

71 Jen Hung-chün, "Chung-kuo k'o-hsüeh-she chih kuo-ch'ü chi chiang-lai", p. 8.

72 Yang Ch'üan, "K'o-hsüeh ti ien-sheng-kuan", p. 1111.

73 Thus, Ting Wen-chiang wrote that his essay was meant "not to save Chang Chün-mai but to awaken the youth before they succumb to the same malady" that had already got Chang. See, Ting Wen-chiang, "Hsüan-hsüeh yü k'o-hsüeh", p. 2.

74 "K'o-shüeh yü fan-k'o-hsüeh" (Science and Anti-science), *KH,* IX:1 (1924), 2.

75 *Ibid.*

Institutional Settings for Scientific Change:
Episodes from the History of Nuclear Physics

INSTITUTIONAL SETTINGS FOR SCIENTIFIC CHANGE: EPISODES FROM THE HISTORY OF NUCLEAR PHYSICS

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"For the modern rapid development of science and in particular for the adventurous exploration of the properties and structure of the atom, international cooperation of an unprecedented extension and intensity has been of decisive importance."[1] Niels Bohr, who had played a central role in atomic science for four decades, made this statement in 1950. It introduced his "Open Letter to the United Nations," which was an unheeded plea for international cooperation in the applications of atomic energy through full exchange of technical information by all nations. The extraordinary World War II application of atomic energy – the Bomb – had been based on fundamental research conducted in the 1920s and 1930s. This research had been characterized by close personal relationships among scientists from many different countries who shared a deep interest in the study of the atom and its nucleus. The nature of the intellectual and technical problems in this new and rapidly developing field had made it fruitful for them to maintain close communication with one another. Through frequent letters and personal visits they exchanged ideas and experiences and rapidly communicated news of research results and of work in progress. These personal interactions supplemented the formal, impersonal communication channels of the scientific journals which made findings available to scientists all over the world.

Niels Bohr's institute in Copenhagen had been one of the key research centers where physicists from many different nations met and worked together in the 1920s and 1930s. It was the richness of this experience that contributed to Bohr's hope for an "open world" solution to the pressing international problems raised by the destructive reality and peaceful potential of atomic energy. The close cooperation among individuals pursuing the same intellectual problems gave rise to hopes for full international cooperation in the applications of science. However, these aspirations were not to be fulfilled in the realm of relationships between states.

It is my purpose in this essay to examine the nature and effects of international cooperation in the study of nuclear physics in the 1930s when the field developed into a position of central importance within the physics discipline. This international growth of the field was spurred by several research centers, and I will attempt to relate their special roles to their traditions, institutional status and needs during the period.

The illustrations that I have developed from the history of nuclear physics show how formal and informal meetings, discussions, and personal visits played an important role in relating research at individual institutions to the world complex of research centers which constitute what is generally called "the international scientific community." The examples also illustrate how attitudes and policies regarding international cooperation among scientists influenced the development of nuclear physics as a research field. They reveal some of the social processes which tend to focus the attention of the members of a scientific discipline on a particular field of inquiry and on fruitful means of conducting that inquiry. Further study of these processes will help to determine the meaning and validity of scientists' retrospective statements that it was "obvious" to all knowledgeable individuals what the most important problems were and which ones were ripe for attack.[2]

My hope is that this historical approach can increase understanding of the nature and effects of international cooperation among scientists at the level of creative scientific activity in specific institutional settings.

I

For a week in the middle of October 1931 the University of Rome's normal population of physicists was increased many times over. Ten of the visitors came from other parts of Italy, and they were joined by 27 of their colleagues from eight other countries. They had been invited to Rome by the Reale Accademia d'Italia for a conference on nuclear physics, the first full-scale international meeting specifically devoted to this field of research. Many of the world's most renowned physicists participated along with younger men already recognized as top rank within the discipline. The candid photographs and motion pictures made and preserved by some of the participants evoke a special nostalgia when viewed by physicists today, including the younger members of the profession. It is as if they were leafing through the pages of their family album. Other photographs, taken during the preceding few years at various physics research institutions throughout the world, show sub-groupings of many of the same people.[3] They had been together at one another's institutions either as visiting lecturers, students, observers, participants in laboratory work, or to take part in formal and informal conferences and seminars. The mobility they enjoyed seemed a natural and necessary aspect of learning physics, doing research, and communicating the results. They also were linked through extensive personal correspondence and by the scientific journals in which they published their work and read the related reports of others.[4]

Most of the visitors had been involved for many years in research which contributed, either directly or indirectly, to knowledge of the atomic nucleus. The largest single group of foreign participants was from Cambridge University where, in Rutherford's Cavendish Laboratory, research on radioactivity directed at the study of nuclear properties had been under way for more than a decade. Rutherford, who had developed the concept of the nuclear atom in 1911, was not at the Rome conference, but the work under way at Cambridge was well represented by five of his younger colleagues. Also at the meeting was the venerable Madame Curie whose Paris laboratory specialized in radioactivity, extending the work that she and her husband, Pierre, had initiated when they made their important discoveries of natural radioactivity at the turn of the century. Other participants were involved in related experimental work on radioactivity in several different institutions in different countries, and a number were specialists in cosmic rays. Among the theorists whose work influenced all fields of physics were Arnold Sommerfeld and Werner Heisenberg of Germany, Wolfgang Pauli from Zurich, and Niels Bohr from Copenhagen. The credentials of the other foreign participants in the Rome conference similarly attested to their contributions to physics research.[5]

The convergence of this group in Rome in 1931 was not precipitated by new discoveries which promised rapid advance in the study of the nucleus, either in techniques of investigation or in theoretical concepts. On the contrary, the experimentalists at the meeting stressed the difficulty of obtaining reliable information on the structure of the nucleus, and the solid data they did present could not be interpreted by the theorists. The latter group lamented that, despite the fact that they had powerful theoretical concepts which enabled them to discuss the properties of electrons and protons, they were stymied in their attempt to make sense of the closely meshed system of protons and electrons apparently confined in the nucleus. A major difficulty was that since electrons were supposed to move within the nucleus at velocities almost that of light, the effects of relativity had to be taken into consideration. Quantum mechanics, which had been developed in the mid-1920s and had since been brilliantly successful in many domains of physics and chemistry, appeared to be inadequate to the task. The physicists saw the need for a relativistic quantum mechanics but had made only limited progress in formulating such a theoretical method.

The papers, discussion, and general atmosphere of the Rome conference reflected the frustrating state of nuclear studies at the time, a situation aptly summarized many years later by Léon Rosenfeld, one of the participants: "The conference in Rome was a muddle in the sense that one could present all kinds of problems but no acceptable solution. One only saw the difficulties."[6] But the meeting did help to clarify the nature of the difficulties and to focus attention on the problems.

Why was the meeting called at that time and in that place? The answer relates to the needs and interests of the Rome physicists who convened the conference. They were preparing to turn their attention to nuclear problems and desired to learn as much as possible from physicists in other countries. In a sense, the conference was the public announcement that 30-year old Enrico Fermi and his young group of Italian physicists at the University of Rome were entering this area of research. Fermi was the general secretary of the conference and its president was Orso Mario Corbino, the highly respected and politically influential physicist. Corbino was a Senator and had held cabinet posts before and after Mussolini's rise to power. Emilio Segrè, who was a member of the Rome physics group at the time, has documented the role of Corbino in the development of the environment for research there.[7] Segrè has shown that Corbino, as director of the university's physics institute, encouraged Fermi to develop it into a research center of international stature. Corbino had long been committed to the goal of improving the relative world position of Italian physics, and the outstanding talents and ambitions of Fermi provided the opportunity.

Italy's first chair of theoretical physics was created at the University of Rome in 1926. Corbino had urged this step and he advanced the candidacy of Fermi, who subsequently won the competition for the position. The report of the committee which judged the candidates was written by Corbino, and it discussed the high quality and wide scope of Fermi's work in terms of physics and national prestige, concluding:

> "He moves with complete assurance in the most difficult questions of modern theoretical physics, in such a way that he is the best-prepared and most worthy person to represent our country in this field of intense scientific activity that ranges the entire world. The committee thus unanimously finds that Professor Fermi highly deserves to have the chair of theoretical physics, the object of this competition, and feels it can put in him the best hopes for the establishment and development of theoretical physics in Italy."[8]

Corbino was concerned with the contributions and reputation of Italy in relation to international scientific developments, and felt that during the preceding two decades Italian physics had contributed little to scientific progress. He was determined to remedy this situation, and he campaigned publicly and privately toward this end. In a remarkable public address delivered at the Florence meeting of the Italian Association for the

Advancement of Science in September 1929, Corbino boldly argued that nuclear physics would be the one live field for physics research in the years ahead.[9] His address, which provoked a spirited discussion within the Italian scientific and technical community, was a perceptive analysis of current trends in physics and an attempt to predict their future development. He felt that such predictions could be made "if one keeps in mind the world organization of scientific work."[10] The changes in this organization which had taken place during the preceding 20 years were characterized by Corbino as the rise of modern theoretical physics; the expansion in experimental physics made possible by well equipped, well staffed new laboratories; and the increasing collaboration of theoretical and experimental physicists. Relating these changes to the situation in Italy, he observed:

> "Considering that collaboration between theoretical and experimental physicists in Italy is only beginning now [through the work of Fermi and his students and collaborators] and that we are far from having the lavish means that laboratories in other countries have, it is not surprising that Italian physics was able to contribute so little to scientific progress in this great renewal period. It will suffice to correct these two deficiencies and Italy will gain back with honor the lost positions."[11]

Corbino stressed that proper work in physics required not only a close relationship of theorists to experimentalists and the provision of ample laboratory facilities, but also awareness of the scientific problems which would provide the greatest interest and potential. He argued that, aside from some filling and refining of detail, physics had already found its definitive theoretical order and that it provided no room for new forces or for essentially new phenomena. Existing branches of physics would be depleted and new branches would only arise through possible artificial modifications of atomic nuclei through bombardment by artificially accelerated projectiles. Corbino, expressing views shared by Fermi, concluded that "while great progress in experimental physics in its ordinary domain is unlikely, there are many possibilities open in attacking the atomic nucleus. This is the most attractive field for future physics."[12]

In the course of his talk, Corbino also raised questions regarding social, psychological, and cultural factors which might influence the choice of a specific research field. Among the questions he asked but did not answer was whether it was possible or useful to make deliberate efforts to influence the orientation of individuals toward various forms of scientific activity. He was concerned in general with the need to make conscious choices in the allocation of social and intellectual resources within science, and his specific interest was in the re-orientation of physics research in Italy so that it would have an international impact.

Implementation of these plans began with the establishment of the chair of theoretical physics and the appointment of Fermi to the post. With Corbino's support, Fermi took the next steps to build up a first-rate research group. However, the experimentalists in the developing Rome group faced major problems because the limited facilities there did not provide an adequate opportunity for research and for learning new experimental techniques. It was at this stage that the Rome physicists benefited from the international tradition of open laboratories. They made deliberate use of facilities in foreign countries in order to form and build a strong physics group in their own country. The Rome group's awareness of what they needed and how they obtained it during this tooling-up period has been concisely described by one of its members, Emilio Segrè:

> "By 1929 it was clear that, whereas the theoretical situation was well in hand, it was necessary to strengthen our experimental activities. In order to import new experimental techniques to Rome, members of the group had to work in different laboratories to learn them on the spot. Thus Rasetti went to Pasadena, to Millikan's laboratory, where he did important work on the Raman effect; I went to Amsterdam, to Zeeman's laboratory, to study forbidden spectral lines; and Amaldi went to Debye's laboratory in Leipzig, where he worked on X-ray diffraction of liquids. Initially we used laboratory facilities that were not available in Rome for the completion of work we had already started at home; later, we used foreign laboratories for experimenting in entirely new fields. In this second phase, Rasetti worked on radioactivity in Lise Meitner's laboratory at the Kaiser Wilhelm Institut in Berlin-Dahlem, and I worked on molecular beams in Otto Stern's laboratory in Hamburg."[13]

The Rome physicists would have been limited in their ability to take advantage of the tradition of free access to laboratories in other countries had it not been for the availability of fellowships from the Rockefeller Foundation which provided support for travel and subsistence. These fellowships were specifically designed to enable young scientists to study and work at research centers in other countries with the aim of strengthening scientific work in their home countries.[14] Not only did the fellowships help the Rome physicists to work in foreign laboratories, but they also enabled outstanding young men to visit and work at Rome and thus to enrich the scientific atmosphere there. Fermi's reputation was already well enough established by 1930 to make Rome a place of interest, especially for theoretical physicists.[15]

By 1931 the Rome group was prepared to turn its attention to nuclear problems, and by convening the first international conference on the subject they brought these problems right to the doorstep of their laboratory. The conference itself helped to focus international attention on nuclear physics and to familiarize physicists from other countries with the strong research group that had been systematically developed at Rome during the preceding few years.

Although many of the participants had been in contact with one another through visits, letters and the published scientific literature, the conference provided a unique opportunity to review the current state of the entire field. During the course of the week, scientists from the leading centers of relevant research reported on their own work and they questioned, criticized and amplified one another's results and ideas. By the end of the week it was clear to all of the participants that specific major experimental and theoretical problems still had to be solved before real progress could be made in understanding the structure and properties of the nucleus. The extreme difficulties in obtaining experimental information about the structure of the nucleus were outlined and hopeful possibilities were described. This state of the field was expressed in the concluding sentence of the paper by C. D. Ellis: "Experimentally it is not easy but in our present absence of knowledge almost any measurement is of value."[16] Theoretical difficulties were discussed by N. F. Mott who ended his paper with this statement: "In conclusion we may say therefore that the spin of the electron is still not properly understood and that it is not possible to use the Dirac equations to describe the behavior of the electrons in the nucleus."[17] Goudsmit's paper, "Present Difficulties in the Theory of Hyperfine Structure," focused on the disagreements between theory and experimental results due to insufficient knowledge of nuclear structure.[18] Gamow's paper, "Quantum Theory of Nuclear Structure," distinguished between problems which had to be treated by the "at present unknown, relativistical quantum mechanics" and the ones which could be treated by ordinary quantum mechanics, and he showed what could be accomplished with the theoretical means at hand.[19] The international stock-taking helped clarify the nature of these problems and thus provided perspective for the scientists from the various national research centers who were concerned with this difficult but potentially promising domain of physics.

The account that I have given of this development illustrates how the institutional policy and research strategy of the physicists at the University of Rome were influenced by their perception of international trends and opportunities within their discipline and by national ambition. It also shows the important role played by institutions in other countries in enabling members of the Rome group to supplement their training and knowledge. One result of these developments, the 1931 Rome conference on nuclear physics, in turn played an important role in increasing international communication at a critical stage in the development of nuclear physics.

II

Within six months after the conclusion of the Rome meeting in mid-October 1931 the situation in nuclear physics had changed radically. First was the dramatic and unexpected discovery that the nucleus contained a new particle, the neutron. This was soon followed by the successful demonstration of powerful new experimental techniques for exploring the internal structure of the nucleus. As a result, a host of new possibilities for experimental research were created and there were new needs and opportunities for theoretical conceptions. And these developments occurred before the proceedings of the Rome meeting, which highlighted the difficulties in making progress in this field of physics, were in print! By the end of 1932 there was substantial agreement among physicists throughout the world that nuclear physics was now a most important and fertile field for research. It was also clear that the Cavendish Laboratory at Cambridge University had played a key role in opening up the field.

A little more than a month after the Rome meeting R. H. Fowler of Cambridge wrote to Niels Bohr in Copenhagen and reported that he was struggling with nuclear problems, but without success. The situation had not changed since they had seen one another in Rome.[20] Fowler, who had worked at Bohr's institute as a Rockefeller Fellow in 1925-26, was an important link between the Cavendish Laboratory and the Copenhagen physicists.[21] Of course, Bohr had studied under J. J. Thomson at the Cavendish in 1911. Bohr's close personal ties with Rutherford started in 1912 when he went to Manchester to complete his studies under Rutherford and they continued after Rutherford went to Cambridge in 1919. But Fowler played a special role because he was developing a theoretical group at Cambridge that was oriented toward the experimental work underway at the Cavendish. His approach was unusual for Cambridge where, generally, the mathematically oriented theorists had shown little interest in the experimental research being done. Fowler, who was Rutherford's son-in-law, was professor of mathematical physics at Cambridge and his close ties with the Cavendish enabled him to provide Bohr with a theorist's view of the work in progress. Fowler's view of the nuclear situation at that time was shared by Rutherford who, a few weeks later, described the current state of the experimental work in an address at the University of Göttingen. He stressed the difficulties involved in obtaining good experimental information about the nucleus and the further difficulties of explaining the results obtained.[22]

Despite the frustrations, Rutherford's laboratory had been focusing for more than a decade on experimental work aimed at increasing knowledge of the properties and structure of the nucleus. It was recognized throughout the scientific world as the center of such research, and it served as an informal clearing house for exchange of information among the individuals working on related aspects of radioactivity at scattered institutions in several different countries. During this period large numbers of students from the Commonwealth nations came to study at the Cavendish, not necessarily attracted to nuclear studies but rather because of their desire to learn the methods and techniques of experimental physics by doing research in the renowned laboratory under the leadership of Rutherford, the master experimentalist. The Cavendish played an important international role in the training of physicists, and Rutherford took this responsibility seriously. Thus he was sensitive to the criticisms he occasionally heard in the 1920s and early 1930s from fellow members of the Advisory Council of the government Department of Scientific and Industrial Research. They felt that the laboratory was not sufficiently oriented to industrial needs and was placing too much emphasis on nuclear studies, which were relatively unproductive and unpromising compared to other fields of physics.

As the number of research students at the Cavendish steadily increased, Rutherford and James Chadwick, who was assistant director of research at the laboratory, discussed policy. Although the Cavendish included several small research groups working on problems in other areas of physics, its main emphasis was on nuclear studies. Should the laboratory continue to concentrate on nuclear physics or should it have a wider field? They had to admit that progress on nuclear problems was very slow, and was even more difficult because of the lack of funds for the needed apparatus. The limited number of meters, pumps, and other basic instruments had to be allocated among the research students, and the shortage was a constant problem. Despite these difficulties they determined to continue the emphasis on nuclear research. Rutherford felt that studies of the constitution of matter posed the most interesting and important questions in physics. Besides, Cavendish workers were experienced and successful in this field, and switching to another field did not seem feasible or desirable. A major factor in the decision was their recognition of their role in maintaining an institutional focal point for such work.[23]

The experimental work at the Cavendish had set high standards for all of physics and the emphasis on nuclear research encouraged scientists at other institutions who were interested in the subject. Rutherford felt a personal responsibility for the entire field. His perception of his role can be seen in the following excerpts from a letter he wrote in 1926 to Lise Meitner who was engaged in radioactivity research with Otto Hahn in Berlin:

> "I thank you for sending me the papers of yourself and Professor Hahn so regularly. I always read them with much interest and I congratulate you on the excellent work you have done since the times of peace.
>
> You probably know that only part of our laboratory is devoted to radioactivity, but we still retain a great interest in the problems.
>
> I have read with interest your important paper in the *Zeitschrift für Physik...*"

Rutherford went on to discuss some discrepancies between Meitner's results and those obtained at the Cavendish and concluded:

> "I am sorry to bother you about these matters but it is much better to discuss matters in a friendly way without rushing into print. You probably know that I now have two grandchildren and, in general, take a grandfatherly attitude even in science." [24]

Rutherford was committed to maintaining the international research role of the Cavendish Laboratory. At the same time, he recognized that the major institutional function of the laboratory was to train young researchers. When it came to allocation of the limited resources of the laboratory, priority was given to providing the means for the research students to work on well defined problems that could be completed within a few years to meet the requirements for the PhD. Thus, the international role of the Cavendish as a training center for physicists was also fulfilled.

The payoff came in 1932. Physicists who remember the excitement of those days sometimes sound as if they were relishing a good wine when they smile and comment: "It was a great year." The first startling news came from the Cavendish Laboratory in February. James Chadwick had demonstrated the existence of a new constituent of the nucleus, the neutron. Ever since Rutherford had suggested in his Bakerian Lecture in 1920 that such a particle might exist, Chadwick had been searching for it.[25] His interest in the neutron was persistent but his efforts to find it were sporadic because of the pressure of his other duties at the Cavendish, which increased as the number of research students grew through the years. Following up observations made in 1930 by two German scientists, Walther Bothe and H. Becker, and subsequently extended at the end of 1931 in Paris by Frédéric and Irène Joliot-Curie, Chadwick was finally successful in his search to find the neutron.

Chadwick's letter announcing the discovery was to appear in *Nature* on February 27, 1932 and on February 24 he sent proofs of the *Nature* letter to Bohr in Copenhagen. Bohr was away at the time in Heisenberg's mountain ski hut along with two other physicists, Felix Bloch and C. F. Weizsäcker.[26] There Bohr completed his long-overdue paper for the proceedings of the Rome conference held the previous October. When he returned to Copenhagen and learned the news he invited Chadwick to come and discuss his work at the small informal conference that had been planned for the second week of April at Bohr's institute.[27] These annual week-long conferences had been started in 1929 and they brought together physicists from many different countries to discuss, as Bohr put it, "actual atomic problems."

Although Chadwick was unable to attend the meeting, Fowler was present and he provided an up-to-the-minute account of the experimental work under way by Chadwick and others at the Cavendish in their follow-up of the discovery. The 22 foreign physicists who were at the conference were from 17 different institutions in nine different countries.[28] Seven of them, as well as Bohr himself, had been among the foreign participants at the Rome conference just six months earlier. There were many new things to talk about, because the discovery of the neutron had opened a number of possibilities for experimental work, and provided fresh challenges for the theorists. Bohr's personal style of thinking aloud set the tone for the Copenhagen conferences and stimulated a lively exchange of information, ideas, and interpretations.[29]

Barely a week had gone by since the Copenhagen meeting, when more exciting news came from the Cavendish Laboratory. This time Rutherford wrote to Bohr:

> "I was very glad to hear about you all from Fowler when he returned to Cambridge and to know what an excellent meeting of old friends you had. I was interested to hear about your theory of the Neutron....
>
> It never rains but it pours, and I have another interesting development to tell you about of which a short account should appear in *Nature* next week. You know that we have a High Tension Laboratory where steady D.C. voltages can be readily obtained up to 600,000 volts or more. They have recently been examining the effects of a bombardment of light elements by protons...."

Rutherford went on to describe the work of Cockcroft and Walton in which they achieved the first artificial nuclear disintegrations with the high voltage accelerator that they had been developing at the Cavendish since 1929. He concluded:

> "I am very pleased that the energy and expense in getting high potentials has been rewarded by definite and interesting results....You can easily appreciate that these results may open up a wide line of research in transmutation generally."[30]

Bohr's response reveals that he fully shared Rutherford's evaluation of the significance of this latest development:

> "By your kind letter with the information about the wonderful new results arrived at in your laboratory you made me a very great pleasure indeed. Progress in the field of nuclear constitution is at the moment really so rapid, that one wonders what the next post will bring, and the enthusiasm of which every line in your letter tells will surely be common to all physicists. One sees a broad new avenue opened, and it

should soon be possible to predict the behavior of any nucleus under given circumstances. When one learns that protons and lithium nuclei simply combine into alpha-particles, one feels that it could not have been different although nobody has ventured to think so. Perhaps more than ever I wish in these days, that I was not so far away from you and the Cavendish laboratory, but the more thankful I am for your kind communication and the more eager to learn about any further progress."[31]

Bohr, who was still working on problems of complementarity, saw the significance of these developments for his own work, but did not turn his full attention to nuclear problems until about 1935. However, through the research of other scientists at his institute, his extensive correspondence, and the annual meetings in Copenhagen, he stayed in close touch with the rapidly developing work in nuclear physics and exerted his influence to bring it to the attention of other physicists throughout the world. Interest in the subject was heightened in 1932 not only by the results from the Cavendish but also by dramatic news emanating from laboratories in the United States: the discovery of a "heavy" isotope of hydrogen (deuterium); and the use of Ernest Lawrence's powerful new particle accelerator, the cyclotron, for nuclear disintegration experiments. Thus, when Bohr and the other members of the scientific committee of the Solvay Institute in Brussels met in July 1932 to plan the October 1933 Solvay Congress, they decided that the topic should be nuclear physics because "the most important problems physics currently posed related to the atomic nucleus."[32]

By the time of the Congress a number of international meetings and visits had further diffused the new experiences and ideas relating to the nucleus and had helped to consolidate the new field as it took root in new institutional environments. For example, Fermi talked on "The Present State of Nuclear Physics" at the International Congress on Electricity in Paris in July 1932; Heisenberg discussed his new theory of nuclear structure at the University of Michigan's summer symposium on theoretical physics in 1932; Rutherford and Heisenberg visited Bohr's institute in Copenhagen during the last two weeks of September 1932 and Rutherford gave two public lectures on nuclear physics at the university; Cockcroft and Aston from the Cavendish Laboratory, Bohr, and Fermi participated in the scientific meetings held in Chicago in June 1933 in connection with the Century of Progress Exposition, and during their stay they visited a number of American institutions involved in nuclear physics; 35 foreign physicists attended Bohr's Copenhagen conference in September 1933; and the first Soviet conference on nuclear physics was held in Leningrad later that month.[33] Then, in October 1933, a group of the leading international participants in this field convened in Brussels for the Solvay Congress. During this same period lively exchanges of letters linked the physicists from the increasing number of research centers where work on nuclear problems was being pursued.

The theorists and experimentalists who met in Brussels for the Solvay Congress in October 1933 had much to discuss. New experimental discoveries, new techniques for probing the structure of the nucleus, and promising conceptual frameworks were rapidly being developed in individual institutions in many countries. These developments included the high voltage accelerator work which was being intensely pursued at the Cavendish, at Lawrence's laboratory in Berkeley, and by Merle Tuve at the Carnegie Institution of Washington; new contributions to theories of nuclear structure by Heisenberg in Leipzig, Majorana in Rome, and Ivanenko in Leningrad; further experimental work on the neutron by Chadwick and others; and the discovery of the positive electron, or positron, in cosmic rays by Anderson in Pasadena, which was confirmed by Blackett and Occhialini in England.

Detailed reviews of these developments were presented and were supplemented by reports from individual laboratories. The ideas, techniques, and experimental research programs which had crystallized at the various national centers were discussed and elaborated further at the congress.[34] Paul Langevin, the French physicist who was president of the 1933 Solvay Congress, attached special significance to the international and youthful character of the meeting. He noted in his introductory talk that 13 or 14 nations were represented and that many of the best young people involved in the work were included. Langevin observed that the emergence of these young physicists in all countries was hopeful for physics and was the best justification for international collaboration.[35]

Langevin's evaluation of the conference in his closing address is one that could have been applied to many of the international meetings of the period: "Naturally we have not solved the difficulties, but we have attained a clearer consciousness, and certainly a more vital one. because of the human relationships we have found here."[36]

A year after the Solvay Congress in Brussels, another international conference on nuclear physics was held in London. By that time there were many more important new developments to discuss, including Fermi's theory of beta decay; the discovery of artificially induced radioactivity by the Joliot-Curies in Paris; and the technique of neutron bombardment to produce artificial radioactivity which was systematically applied and developed by Fermi's group in Rome. These and other significant recent developments were outlined by Rutherford in his opening survey of the field for the 1934 London conference, which led him to this conclusion:

"The development of our knowledge of nuclear physics is now at a most interesting and exciting stage and a close collaboration between the theoretical and experimental physicists is important for rapid progress in this most fundamental of problems."[37]

The field provided major intellectual challenges and the possibilities for fruitful work in it had been demonstrated by physicists at institutions in many nations.

III

The flowering of nuclear physics as an international research field was affected not only by important new concepts and techniques and by the commitment of institutional resources to it, but also by political events which affected the careers of European physicists. A brief look at these developments will suggest how they played a role in the subsequent history of the field.

The 1934 international conference in London symbolized more than the emergence of nuclear physics as a field of central importance. The institutional affiliations of many of the participants indicated that some important change had recently occurred in the international character of the field. Max Born from Göttingen was now at Cambridge; Guido Beck had been at Leipzig when he participated in the 1931 Rome conference but the proceedings of the 1934 meeting showed Lawrence, Kansas as his address; Hans Bethe and Rudolf Peierls were no longer at German universities but were at Manchester; Leo Szilard, who had been part of the Hungarian circle of scientists in Berlin, was now at Oxford.[38] These physicists were among the many who were displaced because of the policies of the Nazi regime which went into effect in the spring of 1933. The institutional affiliations of all of these emigrés at the London conference would soon be changed again, because the positions they had were only temporary ones that had been provided through efforts of their colleagues on an individual basis and through groups formed to aid the displaced German scholars. Their situation was not mentioned specifically in the welcoming address by Royal Society president Sir Frederick Gowland Hopkins, but it must have been in the minds of all of the participants as they listened to his remarks:

"The extraordinary increase of knowledge which we are experiencing, including the very avalanche of new facts which the last few months has brought, has not come from the work of any one nation but from those of many. Science, after all, is always international in its progress."[39]

He noted that the papers to be presented at the nuclear physics conference and the solid state conference being held at the same time emanated from nine different countries, and he added:

> "It is very remarkable also that this great advance in those two important aspects of science with which you are to deal has gone on in spite of political unrest, financial stringency and more important than all, perhaps, anti-intellectual movements in the world. This remarkable progress, we rejoice to realize, has occurred in spite of all such hindrances. It is due to that sheer thirst for knowledge, to the divine curiosity which rise superior to external circumstances."[40]

However, in order to satisfy this curiosity through creative scientific work, it was necessary to have facilities for research and an environment that would encourage it, as well as basic subsistence for the scientist and his family. These needs were immediately recognized within the scientific community, and the close international links that had been established through the intense personal interactions of the preceding years were employed to aid displaced colleagues. Here again, in line with their past traditions, Bohr's institute in Copenhagen and the Cavendish Laboratory in Cambridge played key roles as international centers. They became temporary havens and information clearing houses for the displaced physicists who were searching for opportunities to continue their work.[41]

In addition, Rutherford and Bohr played important personal roles in finding new positions for the emigrés. Rutherford was president of the Academic Assistance Council which was established in England in May 1933 and soon became an international focal point for aiding displaced scholars by providing fellowships for them at various universities with funds contributed by other scholars. His public appeals and private letters and discussions were especially effective because of his enormous prestige and his wide personal contacts.

Similarly, Niels Bohr was one of the organizers of the Danish committee to aid displaced scholars and personally traveled through Germany to obtain a first hand knowledge of the problems. He was able to provide a refuge for many scientists in the traditionally international environment of his institute, and he and his brother, Harald, made strenuous efforts to help other emigrés find positions elsewhere. Niels Bohr went to the United States in May 1933, to lecture at several institutions and to discuss current research with a number of physicists. His trip took place just when the full implications of the Nazi policies were being felt within the German academic community, and he used the opportunity to provide his American colleagues with a personal account of the situation and to explore how their institutions could help. In addition, he visited Max Mason, the president of the Rockefeller Foundation, to enlist support.[42] His trip coincided with the establishment of the Emergency Committee in Aid of Displaced German Scholars which was the major American group in this effort. Soon after his return to Copenhagen, Bohr was host to 35 foreign physicists in September 1933 at his institute's annual conference where current work in nuclear physics was prominent among the topics discussed. Eleven of those who attended had recently lost positions in Germany.[43]

The uprooting of the physicists caused severe personal anxiety and hardships. But, in many cases, their involuntary wanderings provided them with opportunities to become involved in the research under way at the institutions in which they found temporary havens, and they learned new techniques and became familiar with different styles of research. At the same time, they were often able to contribute their own considerable talents to enrich the scientific work and atmosphere at their host institutions. Such personal interaction for extended periods came at precisely the time when new concepts and techniques were rapidly developing in nuclear physics.[44] These dual effects of being uprooted were expressed by the leading German theorist, Max Born, in a letter written to Rutherford shortly after his arrival in England:

> "I wish to thank you very much for your and Lady Rutherford's kind welcome to my wife which as well as Prof. Fowler's friendly help made her forget to be in a foreign country. For her whose family lived since three generations in Göttingen it is much harder to go abroad than it is for me who belongs to the great international family of physicists. I am so happy to have got the lectureship at Cambridge. Since many years I ventured the idea to take leave of absence for one term or two, and to go to Cambridge as a student of nuclear physics. But my duties in Göttingen did not allow me to do so. Now the loss of my position has turned out as a very fortunate event by giving me the opportunity to go to Cambridge for a long time. I hope I may come into a closer contact with you and your collaborators, and learn the facts of nuclear physics which can not be done thoroughly by reading papers. I hope that some theoretical ideas on which I am working at present will turn out useful for the experimental research."[45]

A few months later, in October 1934, Born's paper was the first on the program at the London international conference on nuclear physics. Although his talk dealt with quantum electrodynamics and not with nuclear physics *per se*, he did suggest a possible use of his theoretical ideas to explain not only the evidence of the electron, but also the proton, neutron and other particles.

The international links among physicists were essential to the efforts to assist the emigrés. They had been placed at institutions with the aid of colleagues who had first hand knowledge of their abilities and interests. The emigrés who had worked in nuclear physics were able to participate in related work at their new institutional homes in Copenhagen, Paris, at a number of institutions in England, and especially in the United States where the field was flourishing. Others were attracted to nuclear physics for the first time because of their new contacts and institutional affiliations. As a result, the work in this growing field of research was enriched in the countries which welcomed the emigrés.

IV

International scientific relations at the formal, official level were a sad contrast to the vital international cooperation that existed between research workers in the same field during the period between the wars. These contrasts can be seen clearly in Niels Bohr's attitude toward the International Research Council and in the philosophy and functioning of his institute in Copenhagen. Both the Council and Bohr's institute had been founded with the aim of promoting international cooperation in science.

Bohr was not at the 1934 London conference on nuclear physics. The first communication he had regarding what had transpired there was in the form of a telegram from Robert A. Millikan, stating: "Fitting climax to distinguished congress enthusiastic election of Bohr president."[46] Bohr wired back to the conference headquarters in London expressing appreciation for the tribute.[47] He had assumed that Millikan meant that the conference participants had paid him the honor of designating him the president of the next physics congress. However, he was considerably distressed a week later when he learned that, instead, he had been elected president of the International Union of Pure and Applied Physics. The London conference had been jointly sponsored by the International Union and the (British) Physical Society, and a meeting of the general assembly of the Union was held in conjunction with it.

When Bohr realized his misunderstanding, he wrote to Millikan to tell him why he could not accept the presidency of the International Union.[48] Ever since the International Research Council was established during World War I, Bohr had taken no part in its organizational activities, or in those of the International Unions that were established in the various scientific disciplines under its aegis. The scientists from the allied nations who set up the Council had specifically excluded Germany from membership, in an official boycott of German science. Germany was finally invited to join in 1926, but the leaders of the German scientific organizations did not accept, and the estrangement continued.[49] Bohr felt that it was a matter of principle not to participate in the organizational activities of an avowedly international scientific group unless it was truly international. The business of the

International Unions was conducted through representatives of the various national committees of adhering countries, and Bohr had not participated in the work of the Danish national committee in the hope that his abstention would stimulate efforts to include Germany in the Unions. However, he had no objections to participating as an individual working scientist in the substantive scientific meetings sponsored by these groups. Bohr explained to Millikan that he appreciated that other scientists who shared his general attitude regarding international scientific cooperation had chosen "to take their share in the work of the existing organizations in order to help in attaining a common zeal," but his own principles would not permit him to take a part in these matters.[50]

He clarified his position and emphasized his stand in correspondence with Henri Abraham in Paris, who was secretary general of the International Union of Pure and Applied Physics and pointedly addressed Bohr as "Monsieur le Président et cher Collègue."[51] Bohr explained that if he did not resign from the presidency to which he had unwittingly been elected, he would appear to be renouncing his long standing principles and reacting to the current political situation. In the past he had refused to participate in the work of the International Union as a public protest against the mixing of science and politics. He felt that it would be a very inopportune time to accept the presidency in order to take steps to make the Union fully international, because chances for success had been diminished due to "the deplorable political developments in the countries which are not yet represented in the research council."[52] His principles could be better served at this time by refusing the presidency. In the hope that this step would lead to new unofficial efforts to include all nations in the work of the physics union, Bohr asked Abraham to inform Paul Langevin of France, Max Planck of Germany, and Rutherford about his decision and the basis for it.[53] But by 1934 it was a lost cause.

Despite the boycotts, counterboycotts, and nationalistic political bickering that characterized formal international scientific relations during the post World War I period, informal international ties were flourishing at the level of scientific activity itself and, as we have seen, contributed in a major way to the development of nuclear physics.[54] In this realm, Bohr's institute in Copenhagen played a key role. The international nature of the institute was no accident; it was built in from the start. In 1917, while the International Research Council was being established as an expression of the national hostility of scientists, funds were being solicited from Danes to establish an Institute for Theoretical Physics at the University of Copenhagen that would foster international scientific collaboration. The printed solicitation letter specifically referred to the role such an institute could play in restoring the international scientific collaboration that had taken place before the war, and which continued to be accomplished through the connections scientists in neutral lands, such as Denmark, maintained with scientists in other nations. This role was seen as an important responsibility to be fulfilled by the proposed institute under Bohr's leadership. In this way, the institute could make a contribution of real international significance.[55] The campaign for private contributions was successful, and the funds were used to purchase a site on which the Danish government erected a building. At the dedication ceremony on March 3, 1921 the international role of the new institute was again stressed in the address of the rector of the university who congratulated Bohr on his ability to gather both native and foreign researchers about him in such a way as to resume the international work interrupted by the war.[56]

Research grants were provided by the Carlsberg Foundation and foreign scientists were enabled to work at the institute through stipends provided by the Rask-Oersted Foundation. This unique foundation was established by the Danish State in 1919 to support international cooperation in research. A fund of five million Danish crowns (about 1.1 million dollars) was created for this purpose and it was named in honor of two nineteenth century Danish scholars: Rasmus Kristian Rask, the linguist who traveled throughout the world to study the relationships between European and Asiatic languages; and the physicist Hans Christian Oersted who had made major contributions to research in electromagnetism. The funds were part of the proceeds of Denmark's sale of its West Indies islands (Virgin Islands) to the United States in 1917.

In 1923 Bohr applied for a grant of $40,000 from the Rockefeller Foundation-supported International Education Board. In his application to the Board, he stated that the major uses of the grant would be for expansion of the building of the institute "so that it may be in a position to receive the properly qualified foreign physicists who wish to work there and to offer them suitable working conditions."[57] A fourth of the grant would be used to purchase equipment for experimental investigations in new domains of theoretical atomic physics. Suitable instruments for experimental work on infra-red and x-ray spectra would equip the institute for study of the entire electromagnetic spectrum. To emphasize his case. Bohr stated: "The peculiar character of the institute is a close cooperation between theory and experiment which is a necessary condition for productive work in atomic physics, and this explains why many physicists from other countries, and not least the United States, wish to study here."[58] The attached list of foreign scientists who had worked at the institute during the few years since it had opened was impressive documentation.

Two dramatic developments had occurred in December 1922, six months prior to Bohr's formal application to the International Education Board. Experimental work in the institute laboratory by the Hungarian George de Hevesy and by the Dutch scientist Dirk Coster had resulted in the discovery of a new element. The name given to element 72 was hafnium, derived from the ancient name for Copenhagen.[59] At about the same time, Bohr was awarded the Nobel Prize in Physics for his earlier work on atomic theory.

The grant was made in November 1923, and expansion of the institute and improvement of its facilities began at once. In the following years it became increasingly clear that Bohr's institute was truly international in its philosophy, its organization, and its operation. The large number of foreign guests who worked there for extended periods, the annual conferences on "actual atomic problems," and the constant flow of visitors, were central to the life of the institute in the 1920s and 1930s. Rask-Oersted and Rockefeller Fellowships brought many of the most promising foreign physicists to Copenhagen and also enabled several of the Danish scientists to visit other institutions.

The interchanges at the institute were of major importance in the creation of the new theories of quantum mechanics in the mid-1920s and in the development of theory in nuclear physics in the next decade. Experimental and theoretical work were closely linked at the institute and this led to active participation in nuclear physics as it was developing in the 1930s. The experimental work in radioactivity and spectroscopy that had been pursued there since the 1920s was supplemented in the 1930s by work in the biological applications of nuclear physics and in the uses of particle accelerators.[60] No wonder, then, that Bohr's institute played such a significant role in international communication within this field of physics.

The vignettes that I have developed from the emergence and growth of nuclear physics as an international research field in the early 1930s by no means provide a balanced, complete picture of the history of the field. I have said very little about the substantive content of the subject, stages in the development of concepts and techniques, personal style, or intellectual traditions. Instead, I have focused on the social structures and processes which helped to create the environment for this intellectual activity, to establish it on a world scale, and to diffuse the knowledge it attained.

In this realm informal, personal communication was essential, and I have emphasized the role of international meetings, visits, and exchanges of personnel. Far-sighted foundations helped to make this mobility possible and they provided support for the development of facilities at specific institutions to enable them to become international centers in the field. The special roles played by several institutions at important stages in the development of nuclear physics have been described here in terms of their traditions, needs, and self perception of their national and international status and roles. I have also outlined the effects that political difficulties within nations and between them had on international scientific relations in the 1920s and 1930s. All of these factors were involved in the international efforts to explore the fundamental nature of matter. The lasting results of these efforts can be seen not only in the scientific knowledge obtained from them, but also in the personal and institutional relationships that were forged in the process.

FOOTNOTES

This essay is based largely on research conducted as part of the American Institute of Physics – American Academy of Arts and Sciences project on the history of nuclear physics and has been supported in part by these institutions and by grants from the U. S. National Science Foundation. Extensive use has been made of research materials at the Niels Bohr Library of the AIP's Center for History and Philosophy of Physics in New York. The final manuscript was completed while the author was on leave as a Fellow of the John Simon Guggenheim Memorial Foundation at the Niels Bohr Institute in Copenhagen. The Bohr Institute provided historical source materials, facilities for their use, and traditional warm hospitality. Helpful comments on the draft manuscript were made by Henry Small and Beverly Porter of the AIP Center, and by Brookes Spencer at the Niels Bohr Institute. All of this assistance is gratefully acknowledged.

1 Niels Bohr, "Open Letter to the United Nations," June 9, 1950. Reprinted in S. Rozental, ed., *Niels Bohr: His Life and Work as Seen by His Friends and Colleagues* (New York, 1967), pp. 340-52. For a first hand account of the background of the Open Letter see Aage Bohr, "The War Years and the Prospects Raised by the Atomic Weapons," *ibid.* pp. 191-214.

2 These statements were repeatedly made by physicists at the American Institue of Physics – American Academy of Arts and Sciences exploratory conferences on the history of nuclear physics in May 1967 and May 1969, and in separate interviews preceding the conferences. See Charles Weiner ed., *Exploring the History of Nuclear Physics* (American Institute of Physics Conference Proceedings No. 7, New York,1972).

3 Photographs of the Rome meeting are included in a collection deposited in the AIP Niels Bohr Library by Emilio Segrè. Motion pictures of the Rome meeting participants are included in the documentary film on Enrico Fermi produced by Harvard Project Physics. Photographs taken at other physics research centers during the period are part of the Niels Bohr Library's pictorial collections.

4 Fortunately, a significant portion of this correspondence has been preserved and made available for study because of joint efforts by physicists and historians to locate and arrange for the deposit of original source materials to document the development of physics and the physics community in the twentieth century. The correspondence is supplemented by tape-recorded and transcribed interviews conducted with more than

150 physicists who played significant roles at research centers in Europe and the United States. For descriptions of the nature and location of these materials see *National Catalog of Sources for History of Physics. Report No. 1: A Selection of Manuscript Collections at American Repositories,* prepared by Joan N. Warnow (Niels Bohr Library, American Institute of Physics, New York, 1969), and T. S. Kuhn, J. L. Heilbron, P. Forman, and L. Allen, *Sources for History of Quantum Physics. An Inventory and Report* (American Philosophical Society, Philadelphia, 1967).

5 The participants are listed in the proceedings *Convegno di Fisica Nucleare, Ottobre 1931* (Reale Accademia d'Italia, Rome, 1932). The foreign participants included: (England) F. W. Aston, P. M. S. Blackett, C. D. Ellis, R.H. Fowler, N. Mott, O. W. Richardson, H. Townsend; (Germany) G. Beck, W. Bothe, P. Debye, M. Delbrück, H. Geiger, W. Heisenberg, L. Meitner, E. Rupp, A. Sommerfeld, O. Stern; (France) L. Brillouin, M. Curie, J. Perrin; (United States) A. H. Compton, S. Goudsmit, R. A. Millikan; (Switzerland) W. Pauli; (Denmark) N. Bohr, L. Rosenfeld; (Holland) P. Ehrenfest. George Gamow of the University of Leningrad was invited to the conference but was unable to obtain the necessary passport from Soviet officials in time to make the trip. His paper, read at the meeting by Delbrück, was one of the fourteen papers presented.

6 Tape-recorded interview with Léon Rosenfeld conducted by the author, September 3, 1968. Oral History Collection, Niels Bohr Library, AIP.

7 I am indebted to Professor Emilio Segrè whose accounts of the development of the Rome physics group have provided basic information for this section. See Emilio Segrè *Enrico Fermi: Physicist* (Chicago, 1970), pp. 46-68, and Segrè (ed.), *The Collected Papers of Enrico Fermi*, 2 vols. (Chicago, 1962, 1965). These published materials are supplemented by tape-recorded interviews conducted by Thomas Kuhn, May 18, 1964 (Archive for History of Quantum Physics) and by the author and Barry Richman, February 13, 1967 (Oral History Collection, AIP), and by Professor Segrè's contributions during the discussions at the AIP-American Academy of Arts and Sciences exploratory conferences on this history of nuclear physics, May 1967 and May 1969. I have also benefited from information provided by Professor Edoardo Amaldi in a tape-recorded interview, April 9, 1969 (Oral History Collection, AIP). Barbara Buck of the history of science department, Harvard University, is conducting a doctoral dissertation study of the development of the Rome group of physicists.

8 Quoted by Segrè, *Enrico Fermi: Physicist*, p. 45, from Ministero della Pubblica Istruzione, *Bollettino Ufficiale, Atti di Amministrazione,* Anno 54 (March 3, 1927) 1:634.

9 O. M. Corbino, "I Nuovi Compiti della Fisica Sperimentale," *Atti Societa Italiana Progresso delle Scienze,* Vol. 18 (1929): 1157. Excerpts from the address, in English translation, are in Segrè, *Enrico Fermi: Physicist,* pp. 65-67. An English translation of the entire text was prepared in 1970 by Fausta Segrè of the University of California-Santa Barbara in response to a request by the AIP Center for History and Philosophy of Physics, and she has kindly deposited it at the Center. [Subsequently published with an introduction by Emilio Segrè in *Minerva,* (Vol. 9, October 1971), 528-538.]

10 Corbino, *op. cit.*, Fausta Segrè translation.

11 *ibid.*

12 *ibid.*

13 Segrè, *Enrico Fermi: Physicist*, p. 58. It was during the second phase of this tooling

up period that the Rome physicists began to focus on techniques useful for nuclear physics research. At about the same time, 1930, a similar interest in preparing for work in nuclear physics was expressed by outstanding physicists in Florence where Antonio Garbosso was senior professor. The Florence group included Bruno Rossi, Gilberto Bernardini, and Giuseppe Occhialini. However, they soon began to devote their major attention to cosmic ray research, partially because of the difficulty in obtaining the scarce radioactive sources needed for experimental research in nuclear physics. Like the Rome group, they took advantage of the training and facilities provided by foreign laboratories.

14 For an account of the development of the Foundation's policies and programs in the natural sciences during the 1920s and 1930s see Raymond B. Fosdick, *The Story of the Rockefeller Foundation* (New York, 1952), pp. 145-155, and George W. Gray, *Education on an International Scale: A History of the International Education Board 1923-1938* (New York, 1941).

15 Segrè, *Enrico Fermi: Physicist*, p. 59, lists 10 theorists who visited Rome in the early years of the decade. Another member of the Rome group, Ettore Majorana, was also beginning to attract attention as a brilliant theorist.

16 C. D. Ellis, "β–Rays and γ–Rays," *Convegno di Fisica Nucleare*, p. 117.

17 N. F. Mott, "On the Present Status of the Theory of the Electron," *ibid.*, p. 32.

18 Goudsmit, *ibid.*, pp. 33-49.

19 Gamow, *ibid.*, pp. 65-81.

20 R. H. Fowler to N. Bohr, November 27, 1931. Bohr Papers, Niels Bohr Institute, Copenhagen. Microfilms of the Bohr correspondence have been made in connection with the American Physical Society-American Philosophical Society project "Sources for History of Quantum Physics" and are deposited at the American Philosophical Society, Philadelphia, University of California-Berkeley, and AIP Niels Bohr Library, New York.

21 I am presently conducting a study of the organization and operation of Bohr's institute and the Cavendish Laboratory, and the role they played in the development of nuclear physics. The major sources upon which the present preliminary interpretation is based are the Ernest Rutherford Papers and the administrative records at Cambridge University; the Niels Bohr Papers and extensive institute administrative files in Copenhagen; as well as personal papers of and interviews with individuals who studied and worked at these institutions. Lawrence Badash of the University of California-Santa Barbara is at work on a comprehensive biography of Lord Rutherford which will be an important scholarly source for the history of the Cavendish from 1919-1937.

22 Rutherford's December 14, 1931 talk at Göttingen was recorded, and a limited number of copies were made and distributed during the 1930s. A set of the discs and a tape-recorded copy are in the AIP Niels Bohr Library in New York. At one point in the talk Rutherford remarked: "The bother is that a nucleus, as you know, is a very small thing and we know very little about it..."

23 Tape-recorded interview with Sir James Chadwick conducted by the author, April 15-21, 1969 (Oral History Collection, AIP). Rutherford's view of the Cavendish as a training institution is also discussed in Ivor B. N. Evans, *Man of Power: The Life Story of Baron Rutherford of Nelson* (London, 1939), pp. 186-187. The criticism of Rutherford for focusing on nuclear physics is also mentioned by Marcus L. Oliphant in *Hommage à Lord Rutherford. Ceremonies du X Anniversaire de la Mort de Lord Rutherford* (Paris, 1947), p. 18.

24 Rutherford to L. Meitner, October 31, 1926. Lise Meitner Papers, Churchill College Library, Cambridge University.

25 Rutherford, *Proceedings of the Royal Society* A, 97 (1920), p. 374. J. Chadwick, "Some personal notes on the search for the Neutron," *Proceedings of the 10th International Congress of the History of Science*, Ithaca, N.Y., 1962. (Paris, 1964), pp. 159-162.

26 J. Chadwick to N. Bohr, February 24, 1932 and Bohr to E. Fermi, March 12, 1932 (Niels Bohr Papers, Copenhagen).

27 Bohr to Chadwick, March 25, 1932 (Niels Bohr Papers).

28 Institute Guest Book and conference files (Niels Bohr Institute administrative files, Copenhagen).

29 The discussions at the annual conferences are difficult to document since written agendas exist for only a few of the meetings and minutes were not generally made. Some of the discussions can be traced, however, in the correspondence and notebooks of participants and in their recollections, e.g., L. Rosenfeld, "Quantum Theory in 1929: Recollections from the First Copenhagen Conference," In *Institute for Theoretical Physics. The Niels Bohr Institute. 1921-1971,* a brochure published by the Institute on the occasion of the 50th anniversary of its opening (Copenhagen, 1971). Notes on the April 1932 conference were made by Lise Meitner (Meitner Papers, Churchill College). Bohr's remarks on the neutron at the conference are contained in a manuscript, "On the properties of the neutron," preserved among his papers at the Institute in Copenhagen. For a study of the relation of the discovery of the neutron to the major issues concerning theoretical physicists in the period 1929-32 see Joan Bromberg, "The Impact of the Neutron: Bohr and Heisenberg," *Historical Studies in the Physical Sciences 3* (1971), 307-341. The effect of the discovery on nuclear physics is discussed in the papers by Edward Purcell, Norman Feather, Emilio Segrè and James Chadwick in the symposium on the discovery of the neutron in *Proceedings of the 10th International Congress of the History of Science*, Ithaca, N.Y., 1962. (Paris, 1964), pp. 121-162.

30 Rutherford to Bohr, April 21, 1932. Niels Bohr Papers.

31 Bohr to Rutherford, May 2, 1932. Rutherford Papers, Cambridge University Library.

32 Plans for the Congress were discussed by Bohr in a letter to Heisenberg, July 7, 1932 when he returned from the planning meeting (Niels Bohr Papers). The quotation is from Paul Langevin's introductory talk at the Congress, *Structure et Propriétés des Noyaux Atomiques. Rapports et Discussions du Septieme Conseil de Physique*, Brussels, Oct. 22-29, 1933 (Paris, 1934), pp. ix-x.

33 For Fermi's Paris talk see Segrè, *Enrico Fermi: Physicist*, p. 68. Heisenberg's Michigan lectures are discussed by Samuel Goudsmit in a letter to Bohr, November 4, 1932 (Institute administrative files, Copenhagen). The visits to Copenhagen and the United States are documented in the Bohr Papers and Institute files in Copenhagen, and the Rutherford Papers, Cambridge. Invitations to the Leningrad Conference were sent by D. Ivanenko to Bohr, August 12, 1933 (Bohr Papers) and to Meitner, December 31, 1932 (Meitner Papers). The rapid growth of interest in nuclear physics can be seen in the physics journals: nuclear physics accounted for 4% of the articles published in the world's physics journals which were included in *Science Abstracts* for the years 1929, 1930, and 1931, and increased to 7% in 1932 and 1933, and 9% in 1934. The greatest growth was in the United States. These data are part of the preliminary results of the study of the physics literature conducted by Henry Small at the AIP Center for History and Philosophy of Physics in connection with the AIP-American Academy project on the history of nuclear physics. The work was discussed by Small in his paper. "Nuclear Physics in the *Physical Review*" at the Chicago meeting of the History of Science Society in December 1970.

34 See footnote 32 for the proceedings of the conference. John Cockcroft presented an extensive review of work under way in the U.S. and Europe in the construction and use of particle accelerators and the results obtained from the disintegration experiments they made possible. Rutherford, E. Lawrence, and J. Thibaud added to the review and a large number of the participants – experimenters and theorists alike – joined in the discussion. The procedure was much the same for the other papers: Chadwick on anomalous diffusion of alpha particles, transmutation of elements by alpha particles, and on the neutron; the Joliot-Curies on their discovery of artificial radioactivity, supplemented by a report by P. M. S. Blackett on the positron; Dirac on the theory of the positron; Gamow on the origin of gamma rays and the nuclear energy levels; and Heisenberg on general theoretical considerations on the structure of the nucleus.

35 *ibid.*, p. x

36 *ibid.*, p. 345.

37 E. Rutherford, "Opening Survey," *International Conference on Physics. London 1934.* Vol. I. Nuclear Physics (London, 1935), p. 16.

38 Other emigrés were also present, but only individuals who presented papers or who made formal contributions to the discussions are mentioned in the proceedings.

39 F. G. Hopkins, "Address of Welcome," *op. cit.*, p. 3.

40 *ibid.*

41 See C. Weiner, "A New Site for the Seminar: The Refugees and American Physics in the Thirties," in D. Fleming and B. Bailyn, eds., *Intellectual Migration* (Harvard University Press, Cambridge, 1969), pp. 190-233, for an account of the efforts to aid the displaced scholars, the effect on their careers, and the reception they received at various research centers.

42 Max Mason to N. Bohr, April 26, 1933 (Niels Bohr Institute administrative files, Copenhagen) and Bohr to Heisenberg, May 19, 1933 (Niels Bohr Papers).

43 Bohr Institute administrative files.

44 The experiences of several emigré physicists who made significant contributions to nuclear physics in the 1930s are described in the article cited in footnote 41.

45 Max Born to E. Rutherford, July 29, 1934 (Rutherford Papers, Cambridge University Library).

46 Telegram from R. A. Millikan to N. Bohr, October 6, 1934 (Niels Bohr Papers).

47 The response sent by Bohr is noted on Millikan's telegram.

48 An undated draft of the letter is in the Institute administrative files, Copenhagen. It was probably sent on November 30, 1934.

49 For a full study of this episode in formal international scientific relations see Brigitte Schröder-Gudehus, *Deutsche Wissenschaft und internationale Zusammenarbeit, 1914-1928* (Geneva, 1966). Valuable studies of related aspects of the same question were presented at a symposium on "Science, Government, and Internationalism, 1900-1939," University of California-Berkeley, April 3-4, 1970. The forthcoming proceedings, edited by Roger Hahn, will include the papers by Schröder-Gudehus, "Challenge to Transnational Loyalties: International Scientific Organizations After the First World War," Daniel Kevles, "Into Hostile Political Camps: The Reorganization of International Science in World War I," and Paul Forman, "Scientific Internationalism and the Weimar Physicists." Forman describes the Weimar physicists' use and abuse of the ideology of scientific internationalism and shows how even the most nationalistic scientists pictured themselves as the true defenders of internationalism. His account lends additional support to Robert K. Merton's analysis of the function of the norm of

universalism in science and to Merton's 1942 statement: "Thus the exponents of a culture which abjures universalistic standards in general feel constrained to pay lip-service to this value in the realm of science. Universalism is deviously affirmed in theory and suppressed in practice." (R. K. Merton, "Science and Democratic Social Structure," reprinted in *Social Theory and Social Structure*, enlarged edition, New York, 1968, p. 609.) But it is worth noting instances in which the internationalism of science has been specifically repudiated by scientists to advance political positions, e.g., the statements by some British scientists during World War I and by Nazi scientists in the 1930s.

50 Bohr to Millikan, *loc. cit.*

51 Henri Abraham to Bohr, November 19, December 4 and 18, 1934 and Bohr to Abraham, November 30, December 12, 1934. Bohr Institute administrative files, Copenhagen.

52 Bohr to Millikan, *loc. cit.*

53 Bohr to Abraham, December 12, 1934.

54 Paul Forman, *op. cit.*, points out that the intransigence of the German scientists at the level of formal international relations was possible only because of the existence of extensive informal relations.

55 Copies of the printed letter, dated November 1, 1917, and related materials on the Institute's origins are in the Bohr Institute administrative files.

56 Quoted by Aage Bohr on March 3, 1971 in a talk at the Institute on the occasion of its 50th anniversary, and published in a Copenhagen newspaper, *Berlingske Tidende*, March 4, 1971.

57 The application was in the form of a letter to the Board's president. N. Bohr to Wickliffe Rose, June 27, 1923, Bohr Institute administrative files.

58 *ibid.*

59 A study of the circumstances of the discovery and the reaction to it has been completed by H. Kragh as a thesis at Copenhagen University, "Niels Bohrs teori for det periodiske system og opdagelsen af hafnium," (1970).

60 The international relationships that developed among physicists concerned with the construction and operation of particle accelerators were especially significant. These instruments provided powerful new means to probe the nucleus and they changed the pace, the scale, the cost, and the style of experimental research in the 1930s. I am in the process of preparing a full account of these developments. The story involves exchanges of detailed technical memoranda and progress reports, personal visits, and exchange of personnel. The major institutional base for this activity was Ernest Lawrence's laboratory at the University of California at Berkeley. It became an international information center, a training school, a supplier of cyclotron-produced radioactive materials essential for experimental work in physics and biology elsewhere, and a source of skilled physicists who were dispatched around the world to assist in the construction of cyclotrons. For example, in 1937-38 Berkeley-trained physicists were constructing cyclotrons in Copenhagen, Stockholm, Paris, Cambridge, Liverpool, Tokyo, and at least 12 institutions in the United States. Here, too, Berkeley's international role can be related to its own institutional traditions and needs.

The Authority of Science in Politics

THE AUTHORITY OF SCIENCE IN POLITICS

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I. SOME METHODOLOGICAL NOTES

From an historical and philosophical point of view, questions concerning the character of the relations between science and politics have usually emerged as inseparable from fundamental normative questions about the proper principles of government and the collective life, the legitimacy or the inevitability of politics and the possibility of subordinating politics to rational principles. From a strictly analytical point of view, however, an investigation of the relations between science and politics does not have to involve the affirmation or the negation of politics as a mode of discourse or a method of action in public affairs; nor does it have to be concerned with the principles which should govern the common life. Politics can thus be treated as a given phenomenon, and the basic question which then emerges is not what the relation of politics to science ought to be, but rather how these two modes of human discourse and action actually interact. The failure to make this distinction has often led to the pitfall of examining the relations between science and politics on the assumption that they are mutually exclusive or contradictory. For strictly analytical purposes, however, the preliminary task is rather to find a perspective from which science and politics can be seen and studied as parts of a common or related group of phenomena. The following discussion is an attempt at the exploratory formulation of such a perspective and the examination of its usefulness when applied in a particular case study.

For the purposes of the following discussion, we would use the term "politics" to suggest the art of arriving at collectively binding rules and standards of conduct by accommodating, at least temporarily, incommensurable interests or preferences. As such, politics would not be concerned with problems which can be solved but with problems which can at best be settled.[1] The resolution of political problems is not likely, from this point of view, to involve the application of precise quantitative or other technical procedures. Often the use of ideology, bargaining, economic pressure and other modes of inducing and orienting collective action is likely to be more relevant and effective, especially in areas beset by genuine uncertainties and conflicts of interests where a technical approach or a rational argument is seldom useful. In fact, when reason only clarifies the irreconcilability of conflicts to the involved parties it may even be positively destructive. It is partly for such reasons that scientific or "near-scientific" procedures prove more applicable to questions raised in areas of exceptional consensus or at derivative stages of policy making than to questions raised in areas of deep divisions or at primary levels of policy making – usually at the top – where choices are more complex and the internal relation among goals ambiguous.[2] To make the performance of science in relatively apolitical zones of decision-making and conduct the paradigm for its desired and anticipated performance in areas more purely political is to presuppose the possibility of the total elimination of ambiguities and contradictions from the public domain. It is tantamount to asserting that politics is dispensable and in principle reducible to the nonpolitical. To the extent that this perspective presupposes that science enters politics only on its own terms, it has serious limitations in illuminating how science becomes a part of the fabric of politics.

In examining the roles and functions of science in political contexts, the meaning of the term "science" would naturally be different from that which is attached to it by the scientists or philosophers themselves. In the context of politics it is primarily the attitudes and the perceptions of the lay public which count. Scientists or philosophers may argue that the meanings of science to the laymen are but misconceptions of science, but regardless of the validity of this contention, science or scientific ideas as they appear to (or as they are used by) scientists and philosophers hardly have a public dimension. When compared to the popular perceptions, images and misconceptions of science, they certainly have an "inferior" political "presence." This should not be surprising, considering the fact that those aspects of science most relevant in the context of political discourse and action are different from those most relevant in the contexts of scientific or philosophical discourses. From the perspective of the lay public, what science is and what it means is *inseparable* from such questions as: Who are the scientists? How are they organized? Do they gather in exclusive clubs or in open public forums? Who supports science and for which reasons? Who benefits from the services of science and who does not? Does a given scientific theory reinforce or challenge *prevailing* concepts of truth and reality? Which political ideologies benefit from the authority of science and which are victimized? With which social groups or interests do the scientific community or its individual constituents cooperate?

These aspects of science are perhaps external and irrelevant to the structure of scientific ideas and the validity of scientific theories. But since they are more visible to the lay public and play a greater role in shaping its attitudes towards science, they influence the political environment of science and are indispensable data for the analysis of the relations between science and politics. One would learn very little about whether science is integrated into politics as a tool in the political strategy of a ruling elite, or whether it serves as a weapon in the hands of a dissenting group, by looking only at the structure of scientific ideas or even at the professional proficiency of the involved scientists. What needs to be studied are those aspects of science which are visible to the lay public and the ways in which they structure the meanings and functions that science acquires in particular political contexts.

Since the conceptualization of such a theoretical perspective on science and politics is in its incipient stage, it is perhaps premature to begin the systematic documentation of the evidence for the social perceptions of science in particular societies. At this stage we would venture only to speculate on some of the forms that it might take, with particular reference to the American context.

II. THE ASSUMPTION THAT SCIENCE IS A FORM OF PUBLIC KNOWLEDGE

The examination of the social authority of scientists in America and the rhetoric with which it is often justified and defended may suggest that one of the basic assumptions underlying public attitudes towards science in America, and for that matter perhaps in other Western democracies, is that science is a form of public knowledge. Historically, this belief may be related to the rise and spread of notions of empirical epistemology in the seventeenth and eighteenth centuries, particularly in England and France. As expressed, for instance, in the Baconian insistence that experiment and observation are legitimate sources of valid knowledge or in the Lockean critique of speculative rationalism, the paradigms of knowledge which supported the rise of modern science appeared to expand the social matrix of the community of knowers beyond the former limits of a privileged, exclusive elite of scholars. The lay public probably was encouraged to believe in the superior accessibility of the new science by the conduct of scientists such as Galileo and Gilbert, who mingled with craftsmen and navigators and raised the kinds of questions which were more meaningful in the workshops or ship-building yards of Venice than in the libraries of secluded monasteries.[3] This belief was further reinforced when men of knowledge such as Galileo and Descartes, even though their writings were quite esoteric, "decided to write their works in the vernacular rather than in Latin, avowedly for the purpose of appealing against the learned world to an intelligent reading public."[4] This image of scientific knowledge as public in the popular sense was further reinforced by the development of the natural history sciences, which appeared to be, to use

Foucault's language, but the "nomination of the visible." Well ordered and readily accessible zoological collections and botanical gardens symbolized in the popular perception the notion of science as a system of simple classifications.[5]

That science was evolving as a form of public knowledge could be suggested also by the rhetoric of the early scientists in their attacks upon "pre-scientific" modes of knowledge such as religious dogmas, astrology, alchemy, etc. To early chemists like Lavoisier, the progress from alchemy to chemistry was inseparable from the elimination of the numerous private and arbitrary languages of the alchemists and the adoption of a common language which would limit the discretion of individual scholars in certifying ideas or theories in the name of science. "A well made language," he wrote, "will not allow those who profess chemistry to deviate from the march of nature; it will be necessary either to reject the nomenclature or to follow irresistibly the path which it indicates."[6] It was expected that chemical, unlike alchemical truths, would not be secrets whose validity would be a matter between the individual alchemist and supernatural powers,[7] but rather would be transparent and open to validation by a wide community of rational men. Eighteenth century thinkers such as Diderot, who already viewed the mathematical abstractions of Newtonian physics with profound distrust, hoped that chemistry, because its subject matter was more tangible and accessible to the senses, would indeed become a model of science as public knowledge which speaks a "dual language, the popular and the scientific."[8]

Before long, however, chemistry was to join physics in developing as a system of mathematical abstractions. The standardization and universalization of scientific languages turned out to be not a process of developing a uniform language which would be common to scientists and laymen, nor even a process of unifying the scientific universe of discourse. It was rather a process of progressive differentiation and refinement of specialized languages which were "legalized" by a series of agreements on the part of small groups of professionals.[9] As in the case of chemistry, new nomenclatures in various scientific disciplines were not adopted with an eye to facilitating public comprehension but were in fact inseparable from professional considerations of the relative validity of various theories and the virtues of economy, simplicity and quantifiability in professional communications. The transition from alchemy to chemistry was indeed correlated with limiting the freedom of subjective interpretation and the private fantasies of indivdual scholars, but it was not the lay public but the exclusive professional public which in the long run evolved as the social constraint upon arbitrary or subjective explanations of nature.[10] From the point of view of the lay public, the transition from prescientific to scientific knowledge was in fact but the substitution of one form of esoteric knowledge for another. In both cases, the layman could not rely on his own judgment but remained dependent on indirect evidence and on the authority of others. It has been suggested by Michel Foucault that since the end of the eighteenth century, the decline of the concept of knowledge as the representation of the visible has evolved as a concomitant of an epistemological shift which made the invisible properties of things – such as their internal structure and historicity (order in time) – relevant for scientific analysis.[11]

III. THE PUBLIC FACES OF SCIENCE

Despite the inherent discontinuities between the scientific and the social universes of discourse, the persistent belief that science is not a form of private and arbitrary knowledge because it is subject to universal public validation has remained a central social fact of great significance in the political environment of science. It appears that one important source of support for this belief has been the emergence of technology as the visible physical embodiment of science for the layman. If the cognitive content of scientific theories embodied in mathematical equations or conceptual formulations has become progressively more elusive to the layman, their "existence" can at least be perceived by him in the working of tangible and noisy machines. When, on October 15, 1783, the flight of the balloon of Pilatre de Rozier over Metz became a sensation in France, the invisible gases about which Lavoisier was talking in an inaccessible language were made visible to the layman.[12]

Technology has become in relation to scientific theories what corporeal symbolism has been in relation to religious truths – a mode of popular participation in the invisible. Or, from the point of view of the man of knowledge, a form of accountability of the authority of science to the senses and the popular imagination. The use of technology in the social establishment of the authority of science was already anticipated by Francis Bacon when he asserted that "It is by the witness of works, rather than by logic or even observation, that truth is revealed and established."[13] The role of technology as a bridge between the culture of science and the lay public appeared to give a scientific sanction to the nonmathematical languages of artisans, craftsmen and other common men. Bishop Sprat had already testified that the Royal Society preferred "the language of artisans, countrymen and merchants before that of wits or scholars."[14]

Such indications of the growing epistemological role of technology as a means of validating the authority of science to the public could perhaps be associated with the increasing political mobilization of the lower classes in European societies. The rise of the uneducated public, which was inspired by the Reformation and the spread of democratic political ideals, by changing the structure of the social "audience" of science and the sources of its social and political legitimation as an institution, might very well have increased the role of technology and the other socially visible dimensions of science in substantiating and structuring the social authority of science and its uses in politics. The tendency to view technology almost exclusively as the manifestation of the utility of science seems to have obscured this rather independent though related function of technology as a means of the democratization of the social perception of science.

Another support for the social perception of science as a form of public knowledge seems to have been furnished by the belief that even though scientific knowledge may not be directly accessible to the layman, there are nevertheless indirect yet sufficiently public procedures to check on deceptive or arbitrary practices or claims of abusive scholars. Harvey Brooks has pointed to the analogy between public knowledge in this sense and the assumption of the gold base of paper currency. "Just as the value of the currency depends on the fact that any piece of it could be redeemed in gold on demand, so the authority of science depends on the fact that any piece of it could be 'redeemed' by a public process if necessary. Just as in order to retain the value of the currency the gold backing need be only a small fraction of the total value of currency in circulation, so the verifiability of the scientific process need rest on only a few examples."[15]

Public knowledge, in this sense, is not the universal accessibility of the cognitive content of science, but the assumption that knowledge authorized by science is grounded in impersonal non-private reproducible procedures through which it can be certified by anyone who cares to do so, provided he has the competence and the patience. As long as the circulation of scientific ideas or theories is not challenged, it is assumed that it is always possible to make good on the claims that they represent scientific truths. From this point of view, scientists do not have a privileged access to scientific truth, nor do they have exclusive authority. Their authority is grounded in a functional social division of labor and their authority to define the truth value of theories and propositions is based merely on technical competence in the procedures of scientific certification. The authority of the scientist is thus based on the concession of the layman that since he does not have the time or the skills to establish by himself the scientific validity of ideas and theories which are socially circulated as scientific, he is forced to rely on the words of the relevant scientists. The assumption that their authority is not discretionary but technical diminishes the sense that the reliance on the judgment of scientists involves the surrender of the autonomy of individual judgment or a blind dependency on the authority of others. Yet in order to be reassured that the scientists do not violate the terms of their authority to define the limits of scientific truths for social circulation, the layman is forced to rely on indirect evidence about their integrity, especially on the clues that he can get from the actions and the communications of scientists and scientific institutions. Walter Lippman once commented in a discussion on the discrepancies between the range of judgment accorded to the public by liberal-democratic theory and the public's capacity to judge, that in most cases "What is left for the public is a judgment as to whether the actors are following a settled rule of behavior or their own arbitrary desires. This judgment must be made by sampling an external aspect of the behavior of the insiders."[16]

The external, socially visible aspects of the behavior of scientists and scientific institutions are thus intimately related to the ways in which the lay public perceives scientific norms and to where it draws the boundaries of scientific knowledge. This point is worth emphasizing, precisely because the role of scientific societies and associations in the social articulation of scientific norms has been largely ignored in the literature, which focuses primarily on their roles in coordinating research and distributing rewards as well as on other fúnctions internal to the scientific community. Scientific associations function, in a sense, like technology – as a means through which the invisible mathematics or conceptual formulations of science are made socially present. Like other social institutions, they are "visible" to the lay public through their publications, office buildings, conventions and official spokesmen.

This public role of scientific associations in providing the layman with indirect but authorized signals for the direction and scope of scientific truths usually becomes particularly evident in cases of public controversies which involve a challenge to official scientific opinions. In America, the National Academy of Science and the American Association for the Advancement of Science have often acted as authoritative public definers of the scientific consensus.[17] Whether the boundaries of the scientific consensus, as they are delineated within the scientific community, correspond with those which its individual or institutional spokesmen define in the public domain is an intriguing question in itself. For the purposes of this discussion, however, it is sufficient to note that the ability of the representatives of science effectively to define and defend scientific norms in socio-political contexts largely depends upon the absence of visible disagreements among scientists on scientific matters.

Because it lacks the equipment to weigh directly the relative merits of contending scientific opinions, the lay public can acknowledge the "existence" of binding impersonal scientific truths only where such external aspects of science as institutionalized consensus make it socially visible and compelling. When there is no consensus among scientists, the lay public has no way of deciding whether or where scientific truths are "present." Since the lay public cannot make the subtle distinctions between authoritative and unauthoritative scientific grounds or spokesmen, even scientists with little authority within the scientific community can seriously disrupt the indirect "social signal system" through which the scientific community communicates the boundaries of the scientific consensus and its explications to the lay public. The Velikovsky affair and the UFO controversy are typical examples of the vulnerability of this signal system to the initiatives of a few scientists or quasi-scientists who aim at undermining the credibility of what is socially recognized as the scientific consensus.[18] As the UFO controversy in particular indicates, this vulnerability is increased when the relevant body of scientific knowledge suffers from major unresolved uncertainties or is in a primitive stage of conceptual development. In such cases the weakness of the internal theoretical consensus among scientists makes it more difficult for the relevant peer group to discipline its members, and to identify or penalize deviants.

This relationship between degrees of theoretical consensus and the social organization of science has yet to be explored.[19] Preliminary observations suggest that scientific fields with high levels of theoretical consensus are inhabited by more cohesive scientific groups which share a more common educational background, vocabulary, criteria of relevance, methodology, publications, etc. It appears that the decreasing degrees of theoretical consensus on the continuum from theoretical physics through the life sciences to the social sciences is correlated with decreasing degrees of social and organizational consolidation of the respective scientific groups.[20] Within the natural sciences, the social and organizational concomitant of the absence of theoretical consensus is best recognized by comparing the social organization of the discipline before and after it has reached a high level of theoretical and methodological development.[21]

Such differences in the degree of social cohesion and organizational development of scientific disciplines may perhaps be significantly related to the characteristics of their performance in the public domain. Considering the role of scientific societies and other institutional or behavioral expressions of the scientific community in shaping the lay perception of science, such differences are bound to have great significance for the ways in which various scientific fields interact with the political system. In a social context where scientific fields can hardly develop without government financial support and the public legitimation that it presupposes, the capacity of a scientific field to configurate socially as a consolidated group of experts may be inseparable from the effectiveness with which it can make its case in the public domain and mobilize the authority of science as a whole in support of its own interests. Degrees of social and organizational consensus may explain in part the success that some scientific fields have over others in mobilizing public support for science and in becoming its effective social spokesmen.[22] A comparative study of the physical, biological and social science communities with respect to the degree of their coordination and organizational resources relevant to their performance in the public domain is bound to be quite suggestive with respect to these questions.

The role of the social visibility of science in setting the coordinates of the lay perceptions and images of science has, of course, important implications for the foundations of the social authority of science and its political uses in the public domain. Considering the dependence of the layman upon indirect evidence for perceiving the claims of science, it would be foolish to trace the social authority of science to the compelling force of socially disembodied rational truths or theories. Scientific truths and theories have no presence in the public domain without some form of social and institutional "articulation." The belief that science is socially compelling by virtue of its context-free logics and truth value is a concomitant of the assumption that its content is accessible as public knowledge. This belief seems to be particularly functional for the legitimation of the authority of science in a democratic political context, where any dependence upon indirect evidence or exclusive authority is regarded as a threat to the principles of equality and the autonomy of individual judgment. Consequently, in a democracy the authority of scientists, like the authority of statesmen or bureaucrats, though in fact it may be discretionary, may rest on the ability to sustain the fiction that its roots lie in public consensus or participation.

The widespread belief among Americans that everyone shares the capacity to appreciate the truth was documented already by Alexis de Tocqueville, who observed in the middle of the last century that "in a country where the citizens [are] placed on an equal footing...and where...no signs of incontestible greatness or superiority are perceived in any one of them, they are constantly brought back to their own reason as the most obvious and proximate source of truth."[23] Tocqucville is one of the first observers to note the central role of the premises of public rationality and the possibility of public knowledge of truths in the American political culture. The belief that all the citizens are "on an equal footing" was, of course, central to the notion that there are no exclusive zones of knowledge or meaning and that the only legitimate universe of discourse is the one which is universally open and accessible to all the citizens. Contrasting the practices of the stratified continental social systems with the American. Tocqueville observed that in America

> ...the line ceases to be drawn between expressions which seem by their very nature vulgar and others which appear to be refined. Persons springing from different ranks of society carry with them the terms and expressions they are accustomed to use into whatever circumstances they may enter; thus the origin of words is lost like the origins of individuals, and there is as much confusion in language as there is in society.[24]

The rejection of any delineation of exclusive zones of discourse implies that "evidence" is not context-bound and that scientific and social notions of truth and reality are continuous. These norms were, of course, hardly conducive to the professionalization of science in America and the emergence of exclusive expert languages. And indeed the professionalization of American science, especially in the latter part of the last century, forced scientists to seek ways to reconcile the internal organizational postulates of professionalism with the external imperatives for legitimating science as a form of public knowledge.[25]

The American belief in the possibility of public knowledge has been associated with the assumption that knowledge can resolve social differences. This assumption relates to the tendency to trace conflicts among individuals and groups not to basic ideological or moral differences but to more secondary causes, such as lack of information, ignorance or the misapplication of the appropriate rules. Such attitudes, because they assume a basic social consensus on fundamental values, tend also to regard problems of collective social conduct as technical and governed by the application of impersonal rules.

Louis Hartz has made the point that such consensus on basic values is a characteristic of the American system.[26] This may indeed be not only relevant to the place of science in American politics but also to other forms of impersonal technical approaches to socio-political problems, such as the legalistic approach. Hartz indeed makes the point that "It is only when you take your ethics for granted that all problems emerge as problems of technique," and that "the moral unanimity of a liberal society nourishes a legalistic frame of mind."[27] Similarly, the American faith in the "verdict" of the "neutral", impersonal mechanism of the market implies confidence in the harmony of diverse individual and social interests.[28] The presupposition of basic social harmony made it possible to regard science, like law or the market mechanism, as a neutral source of common standards rooted in some form of public legitimation which depersonalizes the coordination of collective behavior and eliminates the need to rely on arbitrary personal or political authority. The recent adoption of a lottery technique for draft selection may be yet another manifestation of the American urge to sustain the illusion of basic harmony and avoid taking human responsibility for deciding difficult political problems by relying on what appear to be apolitical impersonal procedures or criteria for decision-making. *The New York Times* reported on June 25, 1970, that the collective service system took steps "to insure *complete randomness* in the draft lottery." Draft officials said that the procedures "eliminated virtually *any possibility of human error*"[29] (my emphasis.)

Further evidence for the belief that objective codified knowledge can substitute and is in fact substituting for politics and ideology as the matrix of social choices or norms of collective conduct may be found in the writings of a significant segment of observers of contemporary American culture, and in particular among social scientists. Harvey Brooks sees "a close analogy between the preoccupation of science with manageable problems and the decline of ideology and growth of professional expertise in politics and business." "One of the most striking developments of the postwar world," he continues, "has been the increasing irrelevance of political ideology, even in the Soviet Union, to actual political decisions. One sees the influence of the new mood in the increasing bureaucratization and professionalization of government and industry and in the growth of scientific approaches to management and administration."[30]

Most typical of one group of social scientists is Robert Lane in his essay, "The Decline of Politics and Ideology in a Knowledgeable Society."[31] The "knowledgeable society," which Lane appears to identify with the American system, is a society where "more people value knowledge." In such a society, he argues, "theological and metaphysical modes of thought shrink in contrast to scientific modes." In the knowledgeable society "knowledge must be public, its sources indicated and its conceptual boundaries marked by something other than incommunicable experience." Furthermore, it is a society "marked by relatively greater stress on the use of information veridically, relying on its truth value and not on any adventitious defense." It is characterized by a "decline of dogmatism [and] of ideology," and while "the criteria and scope of politics are shrinking... those of knowledge are growing."[32] These may be false assertions about the reality of the relations between knowledge and politics and yet be a correct characterization of widely shared social attitudes.

The social strength of the belief that knowledge can be an apolitical and compelling matrix of public choices is further substantiated by the findings of Robert Merton, who concludes from studies of the European and American perspectives on data and facts that the American equivalent of the European notion of knowledge is "public opinion." The European concept of knowledge applies to more "esoteric doctrines" whose social matrix is the intellectual elite, whereas the American concept of knowledge appears culturally to apply to mass beliefs widely distributed in society. "On the cognitive plane," writes Merton, "where the European refers to knowledge, the American refers to 'information.' Knowledge implies a body of ideas, whereas information carries no such implication of systematically connected facts or ideas. The American variant accordingly [refers to] the isolated fragments of information available to masses of people; the European variant typically thinks about total structure available to a few."[33] These findings are in striking accord with the observations of Alexis de Toqueville. In a society where all the "citizens are placed on an equal footing" and "the origin of words is lost like the origins of individuals"[34] scientific facts or truths can flow in what is referred to in a common metaphor as "channels of information" or be collected in "data banks" as if they were independent entities free of particular cognitive and normative coordinates.

Analytically, the sense that knowledge is context-free and universally compelling is of course an "optical illusion" that emerges from the assumption that scientific facts and truths are defined in reference to universal cognitive and normative frames of reference or to an all-inclusive social reference group. There is a refusal to admit basic ideological or ethical divisions or to acknowledge that sub-universes of different and even incompatible standards of truth and evidence are perhaps inherent in the social condition and may even be legitimate. The significance of the assumption that science is a form of public knowledge for the relation between knowledge and politics in America lies precisely in the implication that scientifically certified facts or "truths" carry the force of socially compelling laws and do not require "translation" or modification when transferred from the context of science to the public domain or from one social context to another.

The social prevalence of the view that scientific knowledge, which is identified with public information, is a legitimate and even superior basis for social choices and public policy is bound to provide scientists with significant resources of influence over the course of public life. The magnitude of this influence is limited of course by such factors as the ability of scientists to reach a socially effective and articulate consensus and make science sufficiently visible in the social context. But even within these limits the social authority of scientists is concealed by one of its main sources – the belief that scientific knowledge is public knowledge in the popular sense; a belief which obscures the particular normative premises and the nonpublic methods which in fact underlie the scientific enterprise.

This influence depends, as we have pointed out, on many extra-scientific factors such as the capacity of scientists to project in the public domain the appearance of scientific consensus and articulate "objective" truths or facts. Thus, though the content of science may have very little bearing on the substance of decisions or policies, the authority of science as a factor in politics does.

Ironically, the political uses of the ritualistic appeal to the authority of science is often but a "cover" for the non-scientific, non-public, narrow political grounds upon which specific decisions or policies are made. Here the pretense that a problem is merely technical is used to convey the impression that the normative questions are already resolved and the remaining problems are merely technical questions of means which can be handled by a few experts.

IV. THE POLITICAL USES OF THE SOCIAL AUTHORITY OF SCIENCE

Once people believe that the objective meaning of scientific "facts" is continuous with their meaning in public discourse and is equally compelling and binding on the layman as on the scientist, scientific authority can easily become an instrument for influencing public affairs. As such, the incentives for appealing to the authority of science and the logic which governs its uses and functions in public affairs could become politically charged. The common belief that where science operates politics terminates is often, of course, just the kind of attitude which makes the appeal to science a useful political strategy.

The possible political uses of the authority of scientists are diverse. It can be used by policy makers in order to depersonalize and relinquish personal responsibility for unpopular decisions. The policy maker can claim to have no discretion or power to violate the "compelling evidence and imperatives of science." Such an appeal to the authority of scientists can function also as a political device for manipulating the timing of a decision or of its publication. Research or investigation committtes can be used to resist external pressures for immediate decisions and to enable the policy maker to delay his decisions until a more favorable political climate is created, or until he can politically bury the "issue" altogether. The appeal to scientists can also be a strategy for conflict resolution in cases where the contending parties cannot reach an accommodation among themselves, but realize that the costs of not making a collective choice are very high. In such cases, the position of the scientist as an "objective outsider" may often render him an obvious choice as arbitrator, even though his professional scientific skills may not be strictly relevant to the substantive issue.

The authority of scientists can also be used as a means for administrative control. Inasmuch as most government programs and most areas of public policy have scientific as well as legal and financial aspects, scientific experts can function like legal or economic experts as a means of legitimating administrative or policy control which cuts across diverse functions and discrete political and bureaucratic jurisdictions.

The political uses of science which are perhaps most relevant to the case study presented later in this paper relate to the potential impact of scientific definitions of facts or reality on social and political behavior. The authority of science can in some social contexts furnish the kind of sanction that may turn certain notions about reality into premises of collective choices and social conduct. This may occur even when what the authority of science appears to establish within the context of public opinion is, from a rigorous scientific point of view, false.

The importance of definitions of reality as building blocks in social behavior has been noted recently by sociologists such as Erving Goffman and Peter Berger. Goffman has shown that social interactions, like games, are world-building activities which involve the selecting of specific constructs of reality and relevance. A game, says Goffman, which involves a "set of rules which tell us what should not be given relevance, tells us also what we are to treat as real. There can be an event only because a game is in progress, generating the possibility of an array of game-meaningful happenings. A schema of expression and interpretation is involved."[35]

As Peter Berger and Thomas Luckmann have pointed out, such schema also underlie social and political systems of interaction.[36] Any changes in the definitions of facts or reality which are thus interwoven into a system of behavior may involve consequences for the relative interests and values of the participants. It is not surprising that governments, parties, trade unions, business corporations, etc., devote much energy and many resources in the effort to orient public perceptions and to project in the public domain "pictures of reality" which are most favorable to their respective interests and actions. The behavioral links between what people believe about reality and what they do means, of course, that regardless of the validity or objectivity of definitions of facts and reality on the cognitive plane, in the public domain they are never neutral. Definitions of facts and reality always appear between coordinates of political space and time and influence social behavior and the social distribution of power and values. Such questions – as whether an effective anti-ballistic missile system is by scientific and technical standards feasible, or whether food additives like cyclamates are a health hazard, whether there is a causal relation between smoking and cancer, or whether a battery additive is in fact a useful device – could have (when raised and answered in public) immediate repercussions for social behavior and for the relative public credibility and persuasiveness of competing groups in the social, political and economic spheres.[37]

The political significance of the power to fix the legitimate vocabulary of the public discourse and determine what is to be taken as given reality was already a major theme in the debate between Plato and the Sophists. Among modern political theorists, Hobbes perhaps devoted more attention than others to the links between what is publicly established as the reality of human and social nature and the modes of social and political behavior which evolve. For example, Hobbes once observed that "If nature... have made men equal, that equality is to be acknowledged; or if nature have made men unequal; *yet because men that think themselves equal, will not enter into conditions of peace, but upon equal terms, such equality must be admitted.*"[38] (My emphasis.) In this exceptional insight Hobbes clearly distinguishes between the truth value and the behavioral consequences of statements about reality. He takes note of the possibility that assumptions about human nature may be scientifically false and yet socially useful and functional as a basis of action, or conversely, that true assumptions may be socially disfunctional as a basis of action. A theorist of "political engineering" who was concerned with the foundations of a stable body politic, Hobbes was acutely aware of the distinction between the cognitive – representational and the instrumental – behavioral functions of ideas.

When we consider the links between socially established definitions of fact and reality and the direction and organization of human behavior, the political significance of the social authority of science becomes more evident. In the American democracy, the acceptance of scientists as legitimate certifiers of facts and reality for social currency and their relatively free access to the organs of mass communication render the public pronouncements of scientists and other public clues to their opinions a central component of the political process. As is often the case in the public domain, the political force of the communications and actions of scientists is only reinforced by the fact that they appear apolitical.[39] The assumption that scientists, as certifiers of public knowledge, do not hold discretionary authority conceals the political significance of the timing and contexts in which scientists choose to make their opinions public and of which aspects of their scientific opinions they choose to make particularly visible to the layman. Moreover, the assumption that scientific and social universes of discourse are continuous conceals the possibility that the combined role of the social visibility of science and the lay beliefs about it can expand the scope and influence of the social authority of science beyond the limits set by the scientific consensus.

Such discrepancies between the intellectual and the social boundaries of scientific authority may be clarified by an analytical distinction between scientific and social definitions of facts and reality. For the purposes of this analysis, we would term as scientific definitions of fact or reality those definitions which are established or disestablished by scientists, on the basis of their utility in scientific investigation or communication. Social definitions of fact and reality would refer to definitions which are established or disestablished by the relevant social actors, deriving their significance and functions from the socio-political context. The former are defined within the cognitive and normative frameworks which are shared by scientists in the context of scientific inquiry, while the latter are defined within the normative and cognitive context of the larger community or its subuniverses of discourse and interaction. Scientific and social definitions of facts or constructs of reality fulfill different roles and presuppose different conditions and procedures of certification. Scientific definitions of facts and reality can become established as social definitions of facts or reality, but as such they require the fulfillment of certain requirements in addition to those which secure their scientific certification within the scientific community. It is necessary that they be made visible in the public domain and that the lay public – or rather the lay publics – accept them as given and indisputable terms of reference. In some contexts, and I believe America to be one example, in order for certain definitions of facts and reality to be socially established, they must have the appearance of being certified or sanctioned by the authority of science. For this purpose they need not necessarily be scientifically valid in fact.

The authority of science can "free" itself from the internal requirements of scientific certitude and acquire political applications beyond the boundaries of the scientific consensus. This is likely to occur in particular where the intellectual consensus among scientists is ambiguous and its policy implications uncertain or controversial. Yet because in such cases it would be easier for each of the involved political interests to find some scientists whose views and interpretations could be presented as proof that science is "on its side," the appearance of public disagreements among the relevant scientists would in the final analysis reduce the political value of scientific authority in that situation altogether.[40]

In controversies such as the one which will be discussed below, where there is a mixture of mutually reinforcing divisions in the scientific community and the political system, it is often difficult to determine whether it is a case of politicians exploiting the authority of science to gain a political advantage or a case of scientists who are bent on exploiting external political forces in order to defeat their rivals within the scientific community. In most cases it is probably a combination of both.

V. WILLIAM SHOCKLEY VERSUS THE NATIONAL ACADEMY OF SCIENCE, OR THE CONTROVERSY CONCERNING THE RELATIONSHIPS OF ENVIRONMENT, HEREDITY AND INTELLIGENCE*

The old controversy about the relative weight of environmental and hereditary factors in the determination of human and social traits has been revived recently around the publications and actions of two scientists: William Shockley and Arthur Jensen. William Shockley, who received a Nobel Prize for his contribution to the invention of the transistor at Bell Laboratories and who is a member of the National Academy of Science, has been trying for several years to bring the American scientific community and the United States Government to revise welfare and educational policies in the light of what he describes as mounting scientific evidence that social and intellectual disabilities are caused by hereditary genetic factors and not by "environmental deprivation."

Shockley's campaign was reinforced in the spring of 1969, when the *Harvard Educational Review* published an article by Arthur Jensen, a psychologist from Berkeley University, entitled "How Much Can We Boost IQ and Scholastic Achievement?" This article received immediate public attention and was widely reported in the press. A section in the publication asserting that the average distribution of IQ is lower among blacks when compared with whites and that the difference is traceable to hereditary genetic factors became the focus of the controversy. Following its publication, Jensen's article was used by Shockley as a principal scientific sanction

*I am grateful to both parties to the debate for furnishing me with useful information.

Considering the strong feelings aroused throughout this controversy, it will probably be wise to emphasize to the reader that the present discussion does not intend, nor is it necessary, to make judgments about the scientific merit of the contending hypotheses. Since it is the controversy itself as a social phenomenon which is the subject matter of this study, our analysis should not be construed as an implicit justification or as a criticism of any position.

Our concern is more with the typical than with the particular aspects of the debate. The individuals and groups participating in the present debate and their positions, like pins which are thrown into a magnetic field for the purpose of studying its structure, do

not interest us in themselves, but only insofar as they activate – and thus help to illuminate – the network of beliefs and institutions which underlie the relations and tensions between science and politics in the United States.

for his arguments in private and public forums.

On the whole, the public reaction to the Shockley-Jensen thesis has been predictably negative and critical. The Government has refused to recognize the evidence and logic of the hereditarian thesis as warranting any change in its policies. The most prestigious official spokesman of the scientific consensus, the National Academy of Science, has annually voted down, by an overwhelming majority, Shockley's proposals that it should give its sanction, and even make its own independent contributions, to studies in the relations between genetic differences and social and intellectual disabilities. The most enthusiastic public support for Shockley and Jensen has come from individuals and groups which are commonly but not exclusively identified as reactionary.

My first contention is that the strategies of the principal participants in the controversy prove that the controversy was not focused on the effort of settling differences of scientific opinion by the application of scientific norms of discourse and proof, but was primarily a contest between rival scientific groups and their respective supporters over which definitions of fact and reality would be certified by the collective authority of science as valid premises of public policy or social interaction. The issue was not one of defining scientific facts but of establishing as facts in the social context certain propositions. I will try to show that as the principal audience of the debate was not so much the scientific community itself but the lay public, the contestants were naturally led to invest much effort in building *indirect evidence* through which science is made socially visible in order to persuade the public that their opinion is more representative of the true scientific consensus than that of their rivals.

If the real purpose of Shockley and Jensen were limited to persuading the scientific community that the "environmental deprivation" thesis is false and that crime, poverty and low IQ stem from inferior genetic endowment, they could have confined themselves to intra-scientific communication accessible mostly, if not exclusively, to specializing scientists. This, however, was not their strategy. The language of their argument, both in their publications and public statements, is not restricted to the language of the disciplined discourse within the boundaries of a professional audience but is, at least in part, deliberately aimed at persuading non-scientists. Both Shockley and Jensen link the question of the scientific validity of the hereditary or environmental thesis with the question of the validity of specific policies, as if the criteria and meaning of validity remain unchanged when one crosses over from science into politics. Shockley unequivocally states that if "mental differences were acceptably established, then *social actions* could be based on sound methodology rather than emotionally prejudiced racism."[41] Viewing scientific definitions of reality as automatically compelling at the level of policy, Shockley does not shrink from pressing his point with Congressmen and Cabinet secretaries, nor from trying to convert public opinion through the use of the national press. His major theme has been that "concentration upon environmental factors cannot solve the important problems of man's future and that adequate solutions to poverty, crime, illiteracy and national security problems demand facing hereditary problems."[42]

Arthur Jensen has in a similar manner introduced his position in the heredity-environmental debate as a matter having direct implications for public policy, especially for federal educational programs. The first sentence in his now-famous article in the *Harvard Educational Review* refers to a major educational program and states that "Compensatory education has been tried and it apparently has failed."[43] In the course of his article, he is more specific in criticizing premises of government policies on the ground that they are inconsistent with scientific evidence, which, he maintains, supports the hereditary thesis. Referring to statements by the United States Department of Labor (1965) and the Office of Education (1966), in which the Government affirms its commitment to the premises of genetic equality among ethnic groups, Jensen asserts that "there is no factual basis for these official pronouncements which I believe are motivated more by political than by scientific considerations."[44] There is no recognition in this attitude of the possible differences in the meaning and functions of facts and constructs of reality between the contexts of scientific and social discourse. Since political considerations are deemed illegitimate, Jensen cannot heed the advice of that great engineer of social and political systems, Thomas Hobbes, to the effect that even if men are unequal by nature, the preservation of the polity may require that the fiction of equality be sustained.

The refusal of Shockley and Jensen to attach legitimacy to the thoughts and beliefs of the citizens if they are not compatible with what they maintain are scientific facts is the essence of an attitude which views science as entering the public domain and the political context on its own terms. According to this attitude, because the validity of social choices and public policies depends on scientific certifications, it is the scientists who ultimately "fix" the terms of reference for the collective life. There is a realization, however, that for this purpose the social authority of science cannot simultaneously back several definitions of facts and reality which appear to support opposing policies or courses of action. If the scientific authority is so divided, it will appear more politicized and lose much of its capacity to be regarded as an unambiguous and apolitical reference point for social choices. Since the behavioral value of the social authority of science resides in the fact that the layman relies upon it as a source of indirect evidence of the position of science which is inaccessible to him directly, this value is diminished when the social authority of science is divided and the layman is thrown back on his own resources. Thus, regardless of the degree of pluralism which is acceptable to scientists in the socially insulated forums of esoteric professional communications, nevertheless, in the public domain – as the strategies of the contestants in the Shockley-Academy controversy indicate – there are strong incentives for each side to undermine the credibility of his rivals and establish himself as the sole representative of the scientific consensus.

Jensen has thus shown a great concern over the fact that the "environmental thesis" was socially established with the support of a "theoretical sanction from social scientists."[45] Shockley has charged that the geneticist community and the National Academy of Science misled the lay public.[46] These attitudes are clearly more plausible in the context of a struggle for the right to represent the authority of science than in the context of research communications. Consistent with the aim of discrediting the scientific authority of other rivals, the "hereditarians" argue that there is a discrepancy between the scientific evidence which supports the hereditary thesis and what the official spokesmen of science communicate to the lay public as the position of science. They have publicly attributed the discrepancy to the political or ideological motives of the "scientific establishment" and have insisted that in thus violating both the ethical code of science – the injunction to be truthful – and the duty to inform the citizens, the recognized spokesmen of science have lost the right to act as representatives of the scientific consensus. Jensen has accused the environmentalists of holding this position on purely ideological grounds,[47] while Shockley attempts to discredit rival geneticists by charging that the correspondence he has from geneticists and other scientists who insist on not being quoted is evidence enough of lack of candor and objectivity. Failing to win the Academy's support, Shockley has further charged that the Academy position is "the most serious and obvious dereliction of intellectual responsibility in the history of science."[48]

The discrepancy between this political strategy of defeating rivals and the scientific strategy of winning by persuasion is further manifested in the tendency of the hereditarians to present the problem as a question of a clear-cut choice between the hereditarian and the environmental theses. At the level of scientific discourse, theories which are logically or philosophically incompatible can nevertheless be simultaneously acceptable as equally fruitful or applicable. This is particularly the case when neighboring scientific theories are organized around different theoretical goals.[49] From a scientific point of view, a clear-cut choice between the hereditary and environmental theses may be unnecessary. But having to operate in the public domain where the complexity of methodological considerations and research strategies are elusive, where the authority of science cannot be divided and remain effective, and where scientific opinion cannot be easily defensible as a premise of public policy if it is not accepted by the lay public as a fixed and absolute description of reality, Jensen and Shockley were driven to present the issue as that of a clear-cut choice.

Similar factors appear to have also conditioned the position of the spokesmen for the geneticist community and the National Academy of Science who opposed the views presented by Jensen and Shockley. In meeting their challenge the Academy was also forced to engage in the public consideration of politically-charged scientific issues.

The difference between the two sides to the controversy is not that the one has acted politically while the other has been a champion of the scientific conscience. It is rather in the nature of the mix between scientific and political arguments and strategies employed by each side. The Academy as the institutional apex of established science obviously could not afford to ignore the social and political implications of lending the authority of science to a concept of human nature which appears to conflict with a fundamental social belief. Regardless of whether this belief is well grounded or is merely a myth, the point is that in the American context it has been a cornerstone of existing socio-political arrangements. Furthermore, the Academy is certainly not unaware of the fact that the social legitimacy of the scientific enterprise as well as its apolitical and relatively autonomous status could be seriously threatened if it were to get involved in fundamental social or political conflicts. The interest of preserving the institutional base of science in the American polity, or for that matter anywhere, could conflict in some areas with the application of the traditional norms of scientific communication and even with the short-term advancement of knowledge.

To say that the arguments and actions of any of the parties to the debate can be characterized, at least in part, as political does not imply in this context that such arguments or actions are bad or immoral. The term "political" is not used here as an attribute of the deliberate intentions of the involved actors. It signifies rather an objective structural "property" – that their communications and actions had in this particular context a political impact and were largely subject to the logic of political and ideological conflicts. Neither do we mean or need for the purposes of this discussion to pronounce judgment on whether these communications and actions were legitimate. The same communications and actions may be simultaneously evaluated in the social context in terms of diverse and at times conflicting notions of legitimacy, and such differences may exist not only between the scientific and the political communities but also within each of them respectively.

One of the principal differences in the strategies of the two parties to the debate seems to be that whereas the hereditarians have insisted that there exists a scientific basis for a choice in favor of their position, their rivals have insisted that there is insufficient evidence to warrant a decisive change. This difference may stem at least in part from the fact that while the scientific establishment was defending the status quo, the hereditarians needed to mobilize the entire authority of science in order to induce a change. The hereditarians tried to persuade the public that the discrepancy between their attitude and that of the official spokesmen of science stems from the latter's failure to inform the public about the strength of the evidence supporting the heredity thesis. The spokesmen of the Academy, the geneticist community and various groups of the social sciences, on the other hand, insisted that there is no decisive new evidence which would warrant the claim that there is a new mandate for science to revise common public definitions of socio-biological realities.

A group of highly respected members of the faculty of the Stanford University Department of Genetics responded to the Shockley campaign with the statement that "the quantitative importance of hereditary variation for our social problems is ... quite unknown, nor will it be as easy as Professor Shockley implies to find out."[50] With the help of several eminent geneticists, the National Academy of Science prepared a 1,600-word statement on "Human Genetics and Urban Slums." The statement read in part as follows: "There is no scientific basis for a statement that there are or that there are not substantial hereditary differences in intelligence between negro and white populations."[51] The Academy statement attributes the lack of enthusiasm on the part of potential researchers for the study of the relations between genetic factors and complex behavioral traits not only to their preference "to work in noncontroversial areas... where they can work undisturbed [but also to] the conviction that none of the current methods can produce unambiguous results."[52] It is noteworthy that even the first motive, though the Academy implied that it is less valid than the second, is nevertheless perceived as a matter of the objective conditions for productive research and not a matter of sectarian political conviction, as Shockley and Jensen explicitly charge.

The strategy of diminishing the social standing of the "hereditary thesis" by emphasizing the ambiguities and the disagreements concerning its scientific validation was adopted also by the *Harvard Educational Review* itself, following the publication of the Jensen article. Concerned by the mounting public storm instigated by the article, the *HER* editorial board decided to stop circulation of the original issue with the Jensen article, regardless of the tremendous financial loss involved for the journal. Then the *HER* notified all those who requested original copies that a forthcoming special edition of *HER* would include the original article plus a series of dissenting opinions. In a letter of response to requests for reprints, the *HER* wrote: "The Jensen article ... presents a view of intelligence that we feel must be read in the context of expert discussion from other psychologists and geneticists the Spring issue will contain. It is imperative that our readers be given access to the entire debate."[53] Answering a request for permission to reproduce the Jensen article for distribution among the Academy members, in connection with the Shockley proposal on the Academy agenda, the *HER* requested that this not be done and stated that the "entire editorial board wishes that the article be considered as a part of a conversation."[54] In remarks preceding the publication of the promised discussion of the Jensen article, an *HER* editorial referred to the Jensen article as "controversial" and noted that its conclusions "run counter to many of the assumptions on which educational programs of the past few years have been based."[55]

What was common to the attitude of the *HER* editorial board, the position of the Stanford geneticists and the National Academy of Science was the effort to deprive the hereditarians of the means for consolidating the social appearance of the hereditarian thesis as the authorized position of science. In republishing the same article together with several dissenting opinions, the *HER* clearly intended to diminish the article's usefulness in lending scientific sanction to the hereditarian thesis as a premise of social interaction, court decisions, or public policy, while at the same time preserving its integrity as a means for intra-professional research communication. By insisting that the scientific debate is yet unsettled and that no scientific consensus has been achieved on the issue, the *HER* and the Academy and other spokesmen of science wanted to persuade the lay public that no one has a scientific mandate to urge the social establishment of the hereditarian thesis in the name of science.

The spokesmen of official science obviously enjoyed a strategic advantage in the controversy. In addition to the fact that in defending the status quo they could shift the burden of proof to those who demanded a change, they controlled most of the institutional apparatus and the other means through which the scientific community makes science visible to the lay public and applies the authority of science in public affairs.

Nevertheless, as the Shockley-Academy controversy illustrates, dissenting scientists are not without significant resources of influence. Their strategic advantage stems from their ability to capitalize on the discrepancy between the ideological and institutional premises which legitimate the authority of science to the larger society from the outside, and the realities which underlie the discretion and the practices of scientists. They can exploit the conflict between the norm of the right of the citizen to know the facts and the inaccessibility of scientific knowledge to the lay public; between the commitment of the scientific ethos and democratic ideology to the values of public knowledge and the constraints imposed by the professionalization of science. They can exploit the public belief that scientific truths are absolute and conclusive and attribute genuine ambiguities in science and disagreements among scientists to personal or political motives. Finally, they can utilize the reservoir of public sympathy which is often reserved for those who challenge the established cultural and political order with what appears to be great courage and conviction.

Shockley and Jensen indeed tried to utilize these factors and in particular to convince the public that the official carriers of the authority of science had, by misrepresenting scientific evidence to the public, violated the terms of their mandate to be the reliable certifiers of scientific facts and reality. While questioning the integrity of the established authorities of official science, Shockley and Jensen were "courting" the public by insisting on its capacity and right to judge in the matter. Shockley thus expressed his confidence in the "collective public wisdom of an objectively informed electorate,"[56] and Jensen insisted that any "reasonable hypotheses concerning socially and educationally relevant questions should be subject to appropriate investigation and the findings be published and widely discussed by the scientific community and the general public as well."[57]

In the process of blurring the unique logical and social limits of scientific discourse, the hereditarians not only equate scientific with social criteria of evidence and rationality, but also identify the goals of science with those of society. Shockley declares his adherence to the principle that "the truth shall make you free,"[58] and Jensen denounces those "who believe in searching for the truth by scientific means only under certain circumstances and eschew this course in favor of ignorance under other circumstances, or who believe that the results of inquiry on some subjects cannot be entrusted to the public but should be kept the guarded possession of a scientific elite. Such attitudes represent a danger to free inquiry and consequently in the long run work to the disadvantage of society's general welfare."[59] Such affirmations of public rationality and competence are clearly meant in this context to challenge official *scientific* and government opinion. If the public can be trusted, then it can legitimately challenge the institutionalized scientific definitions of truth and reality.

This strategy, applied by the hereditarians against the scientific establishment, ironically recalls the strategy used by the early founders of science in the seventeenth and eighteenth centuries, who appealed to the learned public to support them in challenging the entrenched schoolmen and their political patrons. In the heredity-environment controversy, in the attempt to legitimate a public challenge to the official spokesmen of science, Shockley went so far as to label the press a more reliable medium of information than professional scientific journals:

> In the areas of the problems of my human quality concerns, news reports have proven to be a research tool. I have obtained more relevant pieces of information as a result of news stories based on public lectures than I have from my several publications in scientific journals related to meetings of the National Academy of Science .... The National Academy of Science did not do as well as *The Wall Street Journal* and misquoted me in their 23 April, 1968, Minutes.[60]

In claiming the support of a rational public, the hereditarians were, however, bound to face the difficulties which stem from the fact that in reality public opinion is infinitely divided; that by and large lay perceptions of the claims of science are, in fact, indirect and depend in part on the public posture of the socially recognized spokesmen of science; that the layman cannot be "disciplined" by standards of scientific truth or validity in the same way as the scientist who is bound by his tacit commitments to his professional peers; and that what the layman is willing to accept on the authority of science is influenced by what he perceives as its relation to his interests.

The hereditarians could not fail to notice that the public responses to their pronouncements and actions were not guided by a serious consideration of their truth value but by the assessment of their action value in the social and political context. Despite their insistence on the right of the public to be considered a legitimate co-participant in a genuine scientific discourse, the controversy which they have stirred up furnishes some of the most compelling illustrations of the discontinuities in the meanings and uses of concepts in scientific and social discourses.

After the publication of the Jensen article in the *Harvard Educational Review*, newspapers naturally found as immediately "newsworthy" the relatively few references to the links between racial differences in IQ performance and genetic traits. Typically leaving out the qualifications attached to these assertions within the regimented and precise context of professional discourse (though not entirely without the encouragement of Jensen himself), the newspaper quotations of Jensen conveyed to the lay public a wide range of diverse meanings which they did not have in the original text. Most typical is the use of the two key terms, "intelligence" and "race." In his article Jensen gives intelligence a restricted operational definition. He states that "intelligence, like electricity, is easier to measure than to define. And if the measurements bear some systematic relationships to other data, it means we can make meaningful statements about the phenomenon we are measuring. There is no point in arguing the *question to which there is no answer,* the question of what intelligence really is"[61] (My emphasis.) Similarly, Jensen employs the concept "race" in the technical sense used by geneticists to indicate "breeding populations," "which is to say that matings within the group have a much higher probability than matings outside the group."[62] "Race" is thus a meaningless concept when applied to individuals.

Yet these technical restrictions on meaning failed to limit the use of these terms in public discourse. They were for the most part construed rather in their conventional and highly emotionally-charged sense with all the social and political connotations involved. Professor Joshua Lederberg, Nobel Laureate in genetics, termed this popular version of the Jensen thesis, as expressed for example in an article by Lee Edson in *The New York Times Magazine* of August 31, 1969, "Jensenism." Lederberg called attention to the contrasts between the scientific assertions of Jensen and "Jensenism" and warned against the dangerous social and political repercussions of the latter. Lederberg referred to the pessimism expressed in *The New York Times* article towards the utility of the programs of compensatory education in improving student ability as "a vicious extrapolation of 'Jensenism' whose thrust is contradicted by every finding of modern biological research on how the genes influence development."[63] In an article in *Life* magazine, again it is not the scientific argument of Jensen but "Jensenism" which is conveyed. In his effort to "make sense" to the popular reader, the journalist was led to adopt commonsense language which underlies social, not scientific, definitions of reality. In this article, the restricted use of the concepts "intelligence" and "race" vanished and instead Jensen was described as "busy thinking up... ways to find out whether, as he suspects, American Negroes are on the average really dumber than Whites and whether, as he also suspects, it is the fault of their genes."[64]

In view of such metamorphosis of scientific pronouncements in the public discourse, it is no wonder that the "public" which Shockley and Jensen insisted should be a co-participant in the heredity-environment debate has not, by and large, reacted to the internal logical and conceptual dimension of the contending hypotheses, but rather to their contextual social meanings as terms of reference for social action. Most typical of these reactions was a letter to the editor of *The New York Times* on September 21, 1969, by J. N. Jordan, who wrote, "To my otherworldly black mind, anything that insists upon differences between races and then goes ahead to value one race over another, on the basis of these differences, is racist." A group of students and teaching fellows in a Harvard course on "Biology and Social Issues" (Natural Sci. 26) issued a statement calling attention to the news that the Jensen article was included as required reading for Nixon's Cabinet members and charging that "the net impact of the article becomes racist and performs a political disservice." The Black Student Union of the Harvard Graduate School of Education issued a statement which charged that "in publishing the article by Arthur Jensen, the Editorial Board of the *HER* gave tacit support, whether intended or not, to the argument that black Americans are genetically inferior." In "A Letter from the South," I. Brazziel of Virginia State College asks why, in the light of the social and political consequences of the Jensen article, the *HER* "decided to raise the issue in this way and at this time." Brazziel writes:

> Last week, a scant five days after Arthur Jensen made headlines in Virginia papers regarding inferiority of black people as measured by IQ tests, defense attorneys and their expert witness fought a suit in the Federal District Court to integrate Greensville and Caroline County schools. Their main argument was that "white teachers could not understand the Nigra mind" and that the Nigra children should be admitted to the white school on the basis of standardized tests. Those who failed to make a certain score would be assigned to all black remedial schools where teachers who understand them could work with them. The defense in this case quoted heavily from the theories of white intellectual supremacy as expounded by Arthur Jensen.[65]

These reactions clearly challenge the premise implicit in the Shockley-Jensen appeal for public support against the official spokesmen of the scientific consensus, that an open public debate of that issue could clarify the relative scientific merits of the contending positions and help establish the more valid one. If the controversy around the pronouncements and actions of Shockley and Jensen is examined as if it were, in itself, a kind of social experiment in which the assumptions concerning the capacity of public participation in scientific discourse are put to a test, then these assumptions have been clearly refuted. The incompatibilities between the actions of the "real public" which operates in specific socio-political situations and the actions expected of the public on the basis of the ideological premise that science is within the realm of public knowledge naturally manifested themselves in the strains and contradictions which characterize the position of the hereditarians on this point. A most natural escape from the need to admit that the public domain is the sphere of political, not scientific, discourse is to attribute the irrational reactions of the public to the misleading leadership of the scientific establishment. This, however, was not sufficient to prevent a genuine ambivalence in regard to the rationality of the lay public from creeping into some of the public statements of the hereditarians. The most prominent example of this latent ambivalence can be found in the pronouncements of William Shockley himself, who is caught between his charge that official science distrusts the intelligence and integrity of the public and the actual failure of the "collective public wisdom" (which he entrusts with the role of arbitrator between himself and the Academy) to materialize. Shockley is clearly contemptuous of popular opinion when it is on the side of the environmentalists. He criticized Dr. Seitz, then President of the National Academy of Science, on the ground that his views were "frighteningly subservient to a popular majority opinion, rather than to one tested by adequate study." He questioned "the scientific soundness of the Academy."[66] His ambivalence further reveals itself in his confession that his actions

> ....in raising these questions [about the links between racial differences in behavioral traits and racial differences in genetic endowment] are like those of a visitor to a sick friend who urges a thorough diagnosis, painful though the diagnosis may be, so that remedial steps may be based on objectively established facts and sound methodology. To fail to raise these unpopular questions because of fear of resentment towards me that may ensue, is an irresponsibility I am not willing to have on my conscience.[67]

Ultimately, then, it is not to the lay public but to his private conscience that Shockley is accountable. The dissenting scientist who challenges established scientific authority in the name of the "public right to know" and the competence of "collective public wisdom" as a source of valid judgments ends up in reducing this very public or its parts to the status of a "sick friend" who has to be diagnosed and educated to recognize his sickness and then follow obediently the prescriptions of his patronizing doctor.

This ambivalence reflects the objective difficulty of the social legitimation or establishment of any – let alone of a minority or dissenting – scientific opinion by public opinion. Regardless of the scientific validity of the opinion, the laymen who cannot assess it directly do in fact rely on indirect evidence such as the social manifestations of the presence of a scientific consensus or on other samples of what Lippman calls the "external aspects of the behavior of the insiders."[68] In the final analysis, it is not so much a question of public debate on the relative scientific merit of the "diagnoses" made by rival doctors, but rather of who has a superior scientific mandate and the integrity to be recognized as an established diagnostician. If the public cannot function as an arbitrator between contesting scientific positions, it can at least choose the "doctors" whom it trusts. The grounds of this choice and of the process by which rival doctors win over "patients" from each other can hardly be regarded as scientific. In part these choices are based on the outward signs which make scientists credible to the public; in part on what the public wants to hear. On both these counts, Shockley and the hereditarians were at a disadvantage as compared to their rivals in the scientific establishment. Without the visible support of the majority of their peers and without public preference for the hereditarian point of view on other grounds, they have only their "conscience" to rely on. But even if on strictly intellectual grounds they had a firm scientific basis and despite their rhetorical appeal to democratic ideals, from a political point of view the authority of the scientists who are ultimately responsible only to their private consciences is no more consistent with the principles underlying the legitimacy of democratic authority than the authority of self-appointed religious leaders who claim to be motivated by a compelling religious conscience and hold themselves responsible solely to an equally unpublic and "invisible" divine power. In neither case is the public regarded as the conditioner of established authority.

Such considerations of the comparative legitimacy and acceptability of various types of authority are decisive for the public position in the Shockley-Academy and similar controversies precisely because in the final analysis the public is presented more with a choice between competing claims of scientific authority than between competing scientific propositions.

The comparative status of various types of authority is, of course, a subject of great interest in ethical philosophy and theories of political obligation, but our present discussion is concerned with the social authority of science as a socio-political phenomenon. From this point of view, the controversy between the hereditarians and the Academy, largely because it is a rather extreme case, illustrates the discrepancies between the ideological equation of scientific and social validation of world views or pictures of the universe in the public domain and the reality of their distinctiveness. It is to these discrepancies that we trace the differences between the rhetoric and the logic which govern the integration of science into socio-political processes as well as between the ideological and the analytical views of the foundations of the social authority of science in democracy.

VI. CONCLUSIONS

Our purpose in discussing aspects of the recent controversy on the relative merit of hereditarian and environmental explanations of IQ levels and other behavioral traits has not been to present an exhaustive historical account nor to establish a firm empirical basis for generalizations about the relations between science and politics. It is rather to give some suggestive illustrations of the kinds of problems which are raised when one asks *how the interaction between science and politics is effected* or *in what sense science is a factor in political contexts.*

The Shockley-Academy debate clearly illustrates how in the public domain scientific and technical criteria can be fused, often unrecognizably, with social and political considerations. In this controversy, to be sure, neither the hereditary nor the environmental thesis could marshal the support of unambiguous evidence. Yet it made quite a difference that the environmental thesis was already established as a premise of social interaction and public policy and that the hereditary thesis was trying to usurp its position. The controversy illustrates in what sense the establishment and disestablishment of certified scientific "world views" in the public domain is not strictly a scientific question; that it is, in fact, inseparable from the political positions and the policies that it appears to support. This becomes increasingly so the more scientists whose work is perceived as having social consequences find themselves operating with simultaneous reference to professional and extra-professional audiences. The strategies underlying effective communication with these diverse audiences are often incompatible and the scientists may be compelled to choose between them, effectively if not deliberately.

The interpretation between the scientific and the social universes of discourse is further complicated by the fact that while effective performance with respect to professional peers is essential for the progress of research as well as for obtaining professional recognition for scientific achievements, effective performance with respect to the lay public becomes more and more critical for obtaining the very social legitimation and material support without which the progress of research would be equally damaged. Thus, innocent and intellectually legitimate debate between opposing schools of thought may become in the public domain inseparable from a bitter competition for scarce external sources of support. The heredity-environment controversy provides a striking example. An opponent of Jensen has charged that Jensen's statement that "compensatory education has been tried and it apparently has failed":

> ... can help to boost the forces of reaction which could halt support for research on how to foster psychological development, for the development of technology or early childhood education.... Insofar as it succeeds in boosting these forces of reaction, it will leave the issue open only for debate. Once the support for investigation, development, and deployment has been removed, the differences between our readings of the evidence will no longer be open for "investigation" .... It does no good to plead for keeping all hypotheses open for debate and investigation if the form of the debate removes support for the relevant investigation and for the development and deployment required for a meaningful test of the hypotheses.[69]

At first glance the suggestion of limiting the public communication among scientists seems to run contrary to the core values of the scientific enterprise. Traditionally, social constraints upon free scientific investigation were regarded as a manifestation of ignorance or irrationality to be overcome by more vigorous education and a more massive dissemination of scientific knowledge, not by the self-restraint of the scientists involved or by a deliberate confinement of the audience to which scientific knowledge is accessible. But there are mounting indications that the scientific community is becoming increasingly conscious of the constraints imposed by the socio-political context of science on the social willingness to accept without question its claims to have a superior authority to determine what is truth, evidence or proof. One such indication is the recent emergence of institutional mechanisms whose primary function appears to be to guide and discipline the conduct of science and its communications through the complexities and the divisions of the public domain in order to ensure their effectiveness from the point of view of the over-all interest of science. Thus the National Academy of Science has evolved a special Committee on Science and Public Policy (NAS-COSPUP) and more recently the Report Review Committee (NAS-RRC) with the authority to coordinate the public communications of the various specialized scientific divisions or units within the Academy.[70] Both these mechanisms are clearly charged with weighing the anticipated social, political and economic consequences of public pronouncements, reports or recommendations by various scientific groups, and with the careful economical and selective management of the social and political applications of the authority of science. They attempt to discipline the social meanings and effects of scientific opinions by careful consideration of the forms, the timing, and the contexts of their authorized expressions.

These mechanisms (NAS-COSPUP and NAS-RRC) clearly reflect the growing recognition on the part of the scientific community of the fact that the language of discourse and strategies of persuasion which work within science may not work within the public domain. Such consciousness of the need to develop distinct strategies for handling the business of science in relation to its internal professional and external socio-political environments seems to have increased the role of such institutions as the American Association for the Advancement of Science (AAAS), the National Academy of Science (NAS), the National Science Foundation (NSF) and others which are held accountable both to the scientific and the larger communities. As if having thus a "double constituency," these organizations fulfill a critical role in balancing the needs and expectations of the scientific community and those of its ambient society. They constitute an important manifestation of the adaptation of the scientific community to progressively more intense symbiosis with the socio-political context. As these developments testify, with respect to its dealings with the external society, the scientific community is moving gradually towards a greater consolidationand coordination of the conduct of its individual and group members, whereas from a strictly intellectual point of view the social organization of science has in fact been undergoing a continual process of internal fragmentation and differentiation.

This may justify a reconsideration of our common notions about the nature of the "scientific community." In light of the renewed importance of central institutional mechanisms for the coordination of the collective behavior of scientists vis-a-vis the external society and other complementary institutional trends, it may perhaps be analytically fruitful to see the organization of scientists in public affairs as a form of political rather than intellectual association. In the public domain, the institutions and leaders of the scientific community seem to be more engaged in improving the social position of science than in the internal coordination and organization of research and other purely professional functions. There may be numerous intellectual communities organized along specialized scientific lines within the larger political community of science. This does not mean that central institutions of the scientific community like the NAS do not function as agents of purely intra-professional business such as facilitating professional communication or coordinating scientific effort. But when these institutions act as the agents of science in the larger society, their actions appear to manifest a different kind of logic and consistency.

As the controversy between Shockley and the Academy may indicate, another potential source of insight into the relations between science and politics in a given society are the strategies used in order to challenge or defend the social authority of established science and its political applications in the public domain. Here a comparative outlook juxtaposing Shockley's challenge of the National Academy of Science in the United States with Lysenko's challenge to the Russian Academy of Science may be highly suggestive.[71]* Without entering now into the details of such a comparison, it may be interesting to note the differences between democracy and a one-party dictatorship in the options and strategies available for the mobilization of external political support by dissenting scientists, and the degree of success with which such political pressures can bring the scientific community to its knees.

While Lysenko appealed to the hierarchy of the Communist Party, Shockley directed his efforts towards convincing Congressmen, Cabinet secretaries and, through the national press, the lay public. Lysenko challenged the social authority of the spokesmen of science on the ground that they did not heed the sacred principles of dialectical materialism and in particular the imperative of the close interaction between science and praxis. He accused his rivals of being "enemies of the people" and "opponents of Marxism in science." Shockley challenges the social authority of his opponents on the ground that they are but an exclusive and unresponsive elite who are violating the democratic norm of the "public's right to know" and the commitment "to search openly for truth through objective discussion of conflicting ideas."[72] In each case the dissenting scientist was trying to undermine the social authority of his opponents by questioning their adherence to the political and ideological premises of the system in which their authority was legitimated.

As for the respective success of the two challengers, it is highly significant that Lysenko did obtain the power to dominate research in biology and practical agriculture over a long period of time in defiance of the views of the scientifically authorized scientists in the field. His strength would have been inconceivable without the political backing of the Communist Party. Shockley, operating in the American context, has so far failed in his efforts. The decentralization of political power in the United States has made it, of course, more difficult for Shockley to consolidate effective political pressure

*It should be very clear that we do not mean to imply here any analogy between the intellectual positions or the integrity of Lysenko and Shockley. Scientifically, they are on opposing sides. The comparison is between the socio-political aspects of their campaigns to persuade their respective audiences.

on his scientific opponents. That the heredity thesis is not an attractive card in the political game in the United States whereas Lysenko's environmentalism was politically appealing in the U.S.S.R. goes without saying. The crucial point is, however, that in the United States the Academy and other organs of official science, having developed independent relations with the press, the legislature, the bureaucracy and the White House, are in a better position to utilize their resources in self-defense against such assaults. In the Soviet Union or, for that matter, in any dictatorship, the centralization of political power imposes a severe structural limitation on the capacity of the scientific community to organize politically and defend its autonomy against external political pressures.

The significance of such differences for the ways in which the scientific community is organized in relation to the political system and for the modes in which scientific knowledge and authority are brought to bear upon the conduct of the public life is not confined only to questions of the politics of science and the place of science in public policy. Don K. Price, a pioneer in this field of study, has aptly pointed out that the structure of the relations between science and politics depends on "the way men think about political power in relation to truth."[73] We may thus be dealing here with the social institutional articulations of some of the most fundamental layers of prevailing political and ideological systems.*

FOOTNOTES

1 Bertrand de Jouvenel, *The Pure Theory of Politics* (Cambridge: The University Press, 1963), p. 207.

2 See the literature on the applicability of program planning budgeting systems (PPBS) and operations research in public policy, including: Thomas C. Schelling, "PPBS and Foreign Affairs," *The Public Interest,* II (Spring 1968), pp. 26-36; Don K. Price, *The Scientific Estate* (Cambridge: The Belknap Press of Harvard University Press, 1965), pp. 126-127, 131-132, 294, 128-129.

3 Herbert Butterfield, *The Origins of Modern Science,* revised edition (New York, N.Y.: A Free Press Paperback, 1966), pp. 104-105.

4 *Ibid.,* pp. 180-181.

5 Michel Foucault, *The Order of Things* (London: Tavistock Publications, 1970), p. 132.

6 Quoted in Maurice P. Crosland, *Historical Studies in the Language of Chemistry* (London: Heinemann, 1962), p. 129.

7 *Ibid.,* pp. 31, 48-49.

8 Quoted in Charles Coulston Gillispie, *The Edge of Objectivity* (Princeton, N.J.:

*I am indebted to Joseph Ben David, Harvey Brooks, Robert Merton, Don K. Price and Judith Shklar for their useful comments on earlier drafts of this paper. Their kindness should not necessarily be taken as expression of their agreement with the argument advanced here or any of its parts.

Princeton University Press, 1960), p. 186.
9 Linguistic conventions in chemistry were established, for example, by guidelines for contributors laid down in 1879 by the *Journal of the Chemical Society of London* and by the International Congress of Chemists in Paris in 1889. See M. Crosland, *Historical Studies in the Language of Chemistry,* p. 345.
10 On the unique roles of the professional public in the organization and orientation of the scientific activity, see John Ziman, *Public Knowledge* (Cambridge: The University Press, 1968).
11 M. Foucault, *The Order of Things.*
12 Robert Darnton, *Mesmerism and the End of the Enlightenment in France* (Cambridge, Mass.: Harvard University Press, 1968), pp. 18-22.
13 Francis Bacon, "Thoughts and Conclusions on the Interpretation of Nature as a Science Productive of Works", in Benjamin Farrington, ed., *The Philosophy of Francis Bacon* (Chicago, Ill.: The University of Chicago Press, 1964) p. 93.
14 Thomas Sprat, *History of the Royal Society, 1667,* reprinted in Washington University Studies (St. Louis, Mo. 1958), p. 113.
15 Quoted from private communication on Aug. 11, 1970.
16 Walter Lippman, *The Phantom Public* (New York, N.Y.: Harcourt, Brace and Co., 1925), pp. 144-145.
17 See, for example, the role of the National Academy of Science in the Battery Additive and UFO controversies: Samuel A. Lawrence, "The Battery Additive Controversy" in *Case Studies in American Government,* The Inter-University Case Program edited by E.A. Bock and A.K. Campbell (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1962), pp. 325-368. On the UFO controversy, see *Science,* Jan. 17, 1969, vol. 163, No. 3864.
18 For some suggestive comments on this problem, see Michael Polanyi, "The Growth of Science in Society", in *Criteria for Scientific Development: Public Policy and National Goals,* a selection of articles from Minerva:ed. by Edward Shils (Cambridge, Mass.: The MIT Press, 1968), pp. 187-199.
19 There are some relevant data and observations on this question in Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 2nd edition (Chicago, The University of Chicago Press, 1970), and Warren O. Hagstrom, *The Scientific Community* (New York, N.Y.: Basic Books, Inc., 1965).
20 See, for example, David B. Truman, "Disillusion and Regeneration: The Quest for a Discipline", in *American Political Science Review,* December 1965.
21 For one example, see Joseph Ben David, "Scientific Productivity and Academic Organization in Nineteenth Century Medicine", in *The Sociology of Science,* Bernard Barber and Walter Hirsch, eds. (The Free Press of Glencoe, 1962), particularly pp. 318-322.
22 See a discussion of this problem in "The Political Resources of American Science" by the author in *Science Studies,* vol. I, No. 2, April 1971, pp. 117-133.
23 Alexis de Tocqueville, *Democracy in America,* Vol. II (New York, N.Y.: Vintage Books, 1965), p. 4.
24 *Ibid.,* p. 72.
25 George H. Daniels, *American Science in the Age of Jackson* (New York, N.Y.: Columbia University Press, 1968).

26 Louis Hartz, *The Liberal Tradition in America* (New York, N.Y.: a Harvest book; Harcourt, Brace and World Inc., 1955).
27 *Ibid.,* pp. 10, 46.
28 This idea, of course, has its intellectual roots in classical economic theory. I am indebted for this comment to Harvey Brooks.
29 *The New York Times,* June 25, 1970, p. 9.
30 Harvey Brooks, "Scientific Concepts and Cultural Change", *Science and Culture,* Gerald Holton, ed. (Boston, Mass.: Beacon Press, 1967), p. 71.
31 Robert E. Lane, "The Decline of Politics and Ideology in a Knowledgeable Society," *American Sociological Review,* October 31, 1966, pp. 644-662.
32 *Ibid.,* pp. 655-656, 660, 662.
33 Robert Merton, *Social Theory and Social Structure,* enlarged edition (New York, N.Y.: The Free Press), p. 495.
34 A. de Tocqueville, *Democracy in America,* Vol. II, pp. 4, 72.
35 Erving Goffman, *Encounters* (Indianapolis, Ill.: The Bobbs-Merrill Company, 1961), p. 26.
36 Peter L. Berger and Thomas Luckmann, *The Social Construction of Reality* (Garden City, N.Y.: Anchor Books, Doubleday & Co., 1967).
37 See, for example, Stanley Joel Reiser, "Smoking and Health: The Congress and Causality" in *Knowledge and Power,* S.A. Lakoff, ed. (New York, N.Y.: The Free Press, 1966), pp. 293-314; Kathryn Arnow, "The Attack on the Cost of Living Index," in *Public Administration and Policy Development, A Case Book,*, Harold Stein, ed. (New York, N.Y.: Harcourt, Brace and World, Inc., 1952), pp. 775-853; and S.A. Lawrence, "The Battery Additive Controversy".
38 Thomas Hobbes, *Leviathan,* Michael Oakeshott, ed., with an introduction by Richard S. Peters (New York, N.Y.: Collier Books, 1962), p. 120.
39 Robert C. Wood, "Scientists and Politics: The Rise of an Apolitical Elite", in *Scientists and National Policy-Making,* Robert Gilpin and Christopher Wright, eds. (New York, N.Y.: Columbia University Press, 1964), pp. 41-72.
40 Such a process may be analogous to the role of the segmentation of Christianity into a multiplicity of religious traditions in diminishing the social authority of religion. See Peter L. Berger, *A Rumor of Angels* (1970), p. 44.
41 William Shockley, "Human-Quality Problems and Research Taboos", reprint from *New Concepts and Directions in Education* (Educational Records Bureau, 1969), p. 85.
42 *Congressional Record,* U.S. Congress, August 12, 1969, p. E6849.
43 Arthur Jensen, "How much can we boost IQ and scholastic achievement?" "Environment, Heredity and Intelligence", *Harvard Educational Review,* Reprint series, no. 2 (June 1969), p. 2.
44 *Ibid.,* p. 215.
45 *Ibid.,* p. 2.
46 See Shockley's "Human-Quality Problems and Research Taboos," and his letter to the editor of *Stanford MD,* October, 1966.
47 *HER,* Reprint series no. 2, p. 79. *M.D.* October, 1966.
48 *Congressional Record,* December 20, 1969, p. E10410, and E10910.
49 See on this problem J. Robert Oppenheimer, *Science and the Common Understanding* (London: Oxford University Press, 1954), pp. 74-91. For a brilliant philosophical discussion, see Stephen Toulmin, *The Philosophy of Science* (London: Hutchinson University Library), pp. 105-139.

50 *Stanford M.D.,* October, 1966.
51 Quoted in *Congressional Record,* November 5, 1969, p. E9398.
52 *Ibid.*
53 The content of this letter was provided to me in a private communication.
54 *Ibid.*
55 "Environment, Heredity and Intelligence", *HER* Reprint series no. 2, p. 125.
56 W. Shockley, "Human-Quality Problems and Research Taboos", p. 76.
57 Jensen, "Environment, Heredity, and Intelligence," *HER,* Reprint series no. 2, p. 221.
58 W. Shockley, "Human-Quality Problems and Research Taboos", p. 83.
59 Jensen, "How Much Can We Boost I.Q.," *HER,* Reprint series no. 2, p. 79.
60 "Human-Quality Problems and Research Taboos," p. 75.
61 Jensen, "How Much Can We Boost I.Q.," *HER,* Reprint series no. 2, pp. 5-6.
62 *Ibid.,* p. 80.
63 Quoted in *Congressional Record,* November 5, 1969, p. E9398.
64 John Neary, "A Scientist's Variations on a Disturbing Racial Theme", *Life Magazine,* June 12, 1970, p. 61.
65 Brazziel, in "Environment, Heredity, and Intelligence," *HER*, Reprint series no. 2, pp. 200-201.
66 Quoted in *Congressional Record,* December 20, 1969, p. E10910.
67 "Human-Quality Problems and Research Taboos", p. 74.
68 Walter Lippman, *The Phantom Public,* pp. 144-145.
69 "Environment, Heredity and Intelligence", *HER*, Reprint series no. 2, pp. 148-149.
70 I am indebted to Dean Harvey Brooks, Chairman of NAS-COSPUP, and Robert Green, its Executive Secretary, for useful information and insights concerning this aspect of the Academy's activities.
71 See Zhores A. Medvedev, *The Rise and Fall of T.D. Lysenko* (New York, N.Y.: Columbia University Press, 1969).
72 R.W. Shockley, "Human-Quality Problems and Research Taboos,", p. 96.
73 Don K. Price, *The Scientific Estate,* p. 38.