

# **Studies in Psychology**

*presented to Cyril Burt*

*edited by Charlotte Banks and P. L. Broadhurst*



BARNES & NOBLE INC • 105 FIFTH AVENUE • N.Y.  
PUBLISHERS • BOOKSELLERS • FOUNDED 1873

First published in the United States, 1966  
by Barnes & Noble, Inc.  
New York, N.Y.

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Printed & bound in Great Britain by HAZELL WATSON & VINEY LTD, Aylesbury, Bucks

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ΣΤΕΦΑΝΟΣ





## Preface

ΣΤΕΦΑΝΟΣ (STEPHANOS) is in honour of Professor Cyril Burt as he enters the ninth decade of his life. It comes as a tribute to a very distinguished psychologist from some of his many friends, colleagues and former students at University College, London, and from University of London Press with which he has long been associated. The happiness of this occasion is tempered by the death, in May of 1964, of Professor Burt's oldest friend and colleague, Professor Valentine, whose own tribute constitutes the first chapter of the book.

It is not an easy task to edit a *Festschrift* to a great scientist and pioneer, especially when he has himself been an editor of international repute, and his contributions to psychology are so immense. These, as a glance at the bibliography will show, extend, up to the present time, over nearly sixty years and cover a very wide range of topics. The variety of the subject matter in this book is itself further witness to the breadth of Professor Burt's interests, which are reflected in the papers written by his former students that make up more than half the book. There are two kinds of contributions, the theoretical and speculative and the empirical and experimental; the theoretical papers have been put first.

In humbly offering him these essays, gathered together in gratitude to him as our first teacher of psychology, and in recognition of his own already formidable contributions to our subject, we look forward to those which are yet to come.

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October 1965

## Acknowledgments

We owe a great debt of gratitude to all those who have helped us; in particular to Miss Gretle Archer, who was largely responsible for compiling the bibliography, to Miss Murphy and Miss Randau who typed many of the manuscripts, and to Mrs. Ella Mollo who took the photograph reproduced facing page 5.

C.B.

P.L.B.



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## Cyril Burt: A Biographical Sketch and Appreciation

I am delighted to share in this tribute to Sir Cyril Burt. He is my oldest friend among psychologists, and I, I think, am his. Our friendship reaches back over nearly sixty years without a shadow: even on psychological points the differences between us have been astonishingly slight. I regard him as one of the half-dozen greatest psychologists this century has produced, worthy to stand with Ward, Stout, McDougall, Myers and Spearman. Indeed the scope of his work, wider than any of theirs, makes his place unique. His versatility is shown by the number of his publications, already over 200 in 1950 when he retired from his Chair of Psychology, and by now, I gather, over 300. As will be seen from the bibliography at the end of this volume, the list starts as early as 1909. It comprises pioneer studies on factor analysis (1909 and 1910), on mental inheritance from the Mendelian standpoint (1912), an early work on *The Distribution of Educational Abilities* (1917, translated almost at once into German), a large volume on *Mental and Scholastic Tests* (1921), followed later by *The Backward Child*, now in its fourth edition, and *The Young Delinquent*, which is more entertaining than many detective stories, yet so soundly based on statistical evidence that its main conclusions still remain unchallenged. His later publications include papers on the psychology of aesthetics, of value, and of religious and mystical experiences, a set of broadcast talks on *How the Mind Works*, lectures on *The Subnormal Mind*, and articles on the neurologi- cal and philosophical aspects of psychology. Finally, there are his highly original contributions to statistical theory, notably his book on *The Factors of the Mind*, and his contributions to *The British Journal of Statistical Psychology*, of which he has been an editor from the start.

Cyril Burt was born on March 3, 1883, in a street in Westminster now called Petty France, where John Milton two centuries before had his 'pretty garden-house', and both Jeremy Bentham and the Mills

made their home. One of his earliest memories, so he relates,\* is of the royal procession in which Queen Victoria drove to celebrate her first Jubilee. His father was a London doctor, and hoped that his son would also take up medicine. His mother thought of teaching or the Church. His ancestors included physicians, preachers and mathematicians, one of them (Sir Isaac Barrow) the tutor of Isaac Newton at Cambridge. Burt's father took an active part in his education, teaching him Latin declensions every morning while he was still in his cot. When his father moved to a country practice in Warwickshire, Cyril (aged 10) would often accompany him on his rounds and go into the bedrooms with him, where he was told to observe the patients' various symptoms. Burt senior would afterwards give brief lectures on the illnesses with illustrative diagrams. Here was the beginning of Burt junior's interest in, and wide knowledge of, both physiology and medicine.

At the age of eleven Burt won a scholarship at Christ's Hospital, which in those days was still in the heart of London, opposite Newgate Gaol. He showed a precocious interest in psychology, for when barely fifteen he made copious notes on Ward's famous article on psychology, which he found in his grandfather's copy of the *Encyclopaedia Britannica*—little imagining that in his seventieth year he would be asked to revise the articles on psychology in the same *Encyclopaedia*. While still at school his versatile ability must already have revealed itself. The mathematics master wanted him to try for a scholarship in that subject at Cambridge; but the influence of the Headmaster, and the enthusiasm of Burt's grandfather for the classics, turned the scale. And so Burt gained a classical scholarship at Jesus College, Oxford: being half a Welshman, he chose a Welsh college. His application, at his father's suggestion, to take a degree in science, preparatory to medicine, was not allowed by the college; and he therefore read 'Greats', specializing in philosophy, with psychology (studied under McDougall) as his special subject.

There followed the John Locke Scholarship in Mental Philosophy, and research on mental tests with McDougall. Burt told me that at McDougall's suggestion he enrolled as a student in training for teaching at Oxford Education Department. This was chiefly with a view to securing facilities for entrance to schools for the purpose of testing the pupils—a fact which few people have known and which helps one to understand Burt's ready apprehension of the practical application of psychology to educational problems. With Keatinge, Reader in Edu-

\* Boring, E. G., et al., (ed.) *A History of Psychology in Autobiography*, IV, pp. 53-74.

cation at Oxford, he shortly afterwards started a Child Guidance Centre in the department; and as part of his training accepted a temporary post as classics master at Clifton College.

Burt's father was physician to the Galton family and himself an ardent admirer of Francis Galton. As a boy Cyril Burt had met Francis on several occasions, and become keenly interested in Galton's statistical studies in individual psychology. So when in 1901 McDougall was appointed secretary of a British Association Committee which planned to carry out a nation-wide survey of physical and mental characteristics along lines proposed by Galton, Burt naturally became one of a small group of research students working under McDougall in the Oxford laboratory on tests of mental capacities—a team which included William Brown, J. C. Flugel, and a little later Charles Spearman. Burt had already bought the first volume of *Biometrika* (a new periodical just founded by Galton) which contained a mathematical article on 'Anthropometry and the Identification of Criminals': here Karl Pearson had published a table of intercorrelations (in the form now so familiar) based on the body measurements of criminals, and had suggested reducing them to terms of a few 'index-characters' or 'factors'. Burt thought the same type of analysis might usefully be applied to mental measurements. For this purpose he simplified Pearson's complex equations and so produced the well-known 'centroid formula' (as it was later called) which has since been so widely used. The results, he maintained, fully confirmed Galton's twofold theory of a 'general factor' of intelligence (largely innate) and a number of 'group factors' for special aptitudes. His tables and conclusions were published by Ward in an article of over 80 pages in the *British Journal of Psychology* (1909, III, pp. 94–177) on 'Experimental Tests of General Intelligence'—the longest paper, I believe, that has appeared in that journal and the longest Burt has ever published. In accepting it Ward praised the author's 'ingenuity and industry', but regretted that his time should have been wasted on a problem like mental testing which has so little future before it.

Having completed his Oxford experiments, Burt went to Germany to work under Külpe. He was accompanied by Flugel; I followed two months later; Pear arrived later still. And it was thus that my three most enduring friendships were formed. Psychology's growing point had moved from Leipzig to Würzburg, where Marbe, Bühler, and Ach were now extending scientific techniques to the study of 'imageless thought' and 'higher mental processes'. The most original member of

the team was H. J. Watt, who showed how the older methods of trained introspection could be combined with the newer experimental procedures—a twofold approach which became one of the most characteristic devices of Burt's later researches. Burt's contribution was to use 'free' and 'controlled' association to demonstrate that 'relations' as well as 'images' could form the conscious content of thinking and reasoning.

It might, I think, be plausibly argued that the fruitfulness of British psychology has been largely due to the multiplicity of interests cherished by British psychologists. 'It is typical of the Englishman', wrote Chesterton in one of his more paradoxical moments, 'to be Jack of all trades, and therefore brilliant in one'. The bespectacled German students, on the other hand, struck us as having singularly one-track minds—so earnest, humourless and concentrated. To them the *verrückten Engländer*, as they called us, seemed always to be wasting time on a variety of irrelevant hobbies. Flugel was a brilliant musician, with a piano in his lodgings. His father was an amateur astronomer with an observatory at the bottom of his garden. So while the Germans spent their nights arguing in the nearby *Biergarten*, Burt and Flugel were gazing at strange constellations in the clear Bavarian skies (you may still catch Burt peering through his telescope at home, mapping sunspots or the craters of the moon). The hobby of Burt's father was botanizing (his unique collection of pressed British wild flowers can be seen in the museum at Kew Gardens). So Burt used to be off on Sundays into the woods hunting for new specimens. It was, so he always declared, his botanical interests that helped him to understand the new Mendelian theories that were being tested out by Cambridge botanists, and to see their application to mental heredity.

Burt's first post was that of Lecturer in Psychology and Assistant Lecturer in Physiology under Sir Charles Sherrington at Liverpool University, a post previously held by R. S. Woodworth and H. J. Watt. Here he turned to the study of inherited traits and individual differences. While at Liverpool he published an article on 'Tests of Higher Mental Processes', containing the first examples of the types of group test now so widely used in the eleven plus examinations—opposites, completions, analogies and reasoning. Nor were his factorial studies limited to the intellectual aspects of individual psychology. As early as 1915 he contributed a paper to the British Association on 'The General and Specific Factors underlying the Primary Emotions', in which he demonstrated the existence of 'general emotionality'



(the characteristic of the 'unstable child') and of factors corresponding to what were afterwards termed 'extraversion' and 'introversion'. Later he produced evidence for both the general and special factors underlying aesthetic appreciation, and investigated their nature. These, with his work on tests of personality and artistic taste, led him to take up a topic still unexplored except by philosophers—the psychology of value. And it was doubtless his interest in the non-intellectual aspects of individual character that led him, in the days when Freud's name was barely known, to take up the study and criticism of psychoanalysis.

In 1913 the London County Council appointed him Educational Psychologist—the first appointment of its kind in the world. He was left to draw up his own plan of work. As he envisaged it, and as it was finally approved by the Council, it was essentially threefold:

1. To carry out periodically, with the assistance of the teachers, psychological surveys of the children in the Council's schools; 2. to examine and report (a) on individual cases of educational subnormality (chiefly in connection with certification and training of mental defectives), (b) on individual cases of gifted children (chiefly in connection with borderline cases for junior county scholarships), (c) on delinquent children (chiefly in connection with juvenile offenders appearing before the courts); 3. to study and report on the psychological aspects of any specific educational problems that might from time to time arise (e.g. the methods adopted in the Council's examinations for entrance to 'secondary' schools, i.e. what are now called 'grammar' schools).

The 'Psychologist's Office' in the old education building on the Thames Embankment thus became the first official child guidance centre in the country.

The appointment was to be half-time. The other half Burt devoted to theoretical research, for which he had as his field of investigation all the schools and institutions under the County Council. He was thus enabled to gather an immense harvest of facts by obtaining data in the form of test-scores, teachers' assessments, reports on home conditions, etc. for thousands of children. The conclusions were published in a series of reports to the L.C.C.; and the more important results were incorporated in his *Mental and Scholastic Tests*, of which a new edition was issued only a year ago. During this period he introduced a number of new statistical and experimental techniques, which later investigators adopted, sometimes with a different terminology, sometimes apparently ignorant of the fact that Burt had already employed them.

With all this development of theoretical studies it was characteristic of him that he sought to gain a first-hand knowledge of the kind of life his various subjects led. Thus he was not content with living at a University Settlement in the slums, but spent many weekends as a guest of a docker in Stepney, and even of a burglar in a back street in the East End. As a further basis for his study of delinquents he got himself accepted as a member of a criminal gang meeting in a disreputable Soho café. Later he discovered he had been known as 'Charlie the parson', though, to his great relief, no one had ever suspected him of being a 'copper's nark'.

Of Burt's work for the schools and for school children there is no need for me to speak at length. It has been admirably reviewed and summarized by Dr. Wall, Director of the Foundation for Educational Research, in a recent issue of *Forward Trends*. Intelligence testing, which is commonly associated with Burt's name, and which he did so much to develop and popularize (though always as an adjunct to other methods), seems at the moment under a cloud; nevertheless, there must be many children from poverty-stricken and illiterate homes who have reason to thank him for rescuing them from the stigma of mental deficiency under which they would otherwise have suffered, and hundreds who, owing to their social handicaps, would never have won their way to a grammar school (and often to a university) without the methods which he introduced.

When the First World War broke out, Burt became secretary of a British Association Committee for war research, and then joined the Ministry of Munitions as a statistician. Shortly after the war was over he was appointed head of the new Vocational Section of the National Institute of Industrial Psychology founded by Dr. C. S. Myers, a post which he held jointly with his L.C.C. appointment. This addition to his field of work opened out opportunities for still newer types of test (for occupational guidance and the like) and new lines of research. In 1926 he left the National Institute of Industrial Psychology to become Professor of Educational Psychology at the London Day Training College (now the Institute of Education) in the University of London. This led to a further range of activities, including the application of tests and other methods to the selection of candidates for the teaching profession: the results were checked by 'follow-up studies' on the students after they had accepted posts as teachers.

At the same time he and his colleagues became pioneers in many new and unpopular branches of psychology. With Flugel and Susan Isaacs

(both of whom later became members of his staff), and of course Ernest Jones, he was one of the founding members of the British Psycho-analytical Society. But here his aim was not so much to accept or to propagate the novel doctrines of Freud and Jung as to subject them to the test of experimental and statistical techniques. From the early years of his work with McDougall, both he and Flugel were actively interested in the problems of parapsychology, and did much to make it scientifically respectable. It was in his laboratory that Soal carried out the earliest of his well-known researches on extrasensory perception, and obtained a doctorate; and thus, in the face of no small opposition, Burt became the first to make it possible for a University degree to be awarded for critical inquiries in the field of psychical research.

In 1931 he was appointed Professor of Psychology at University College, London. There were then only about half a dozen undergraduates taking a degree in psychology, and some dozen research students. Twenty years later Burt had over 200 students; and of these nearly 80 were engaged in research. Later many of his students themselves became professors or continued work along similar lines, notably Professor Cattell, Professor Wrigley, Dr. Ardie Lubin and Dr. William Stephenson in America; Sir Fred Schonell, Professor McElwain and Professor Meadows in Australia; Professor Field in New Zealand; and in this country Sir Frederick Bartlett, Professor Broadhurst, Professor Eysenck, Professor Cohen, Professor Summerfield, Professor Hamilton, Professor Warburton and many others who have obtained official posts in Canada, Egypt, India, Pakistan and Persia.

On his appointment to the Chair at University College, Burt resigned his L.C.C. appointment, and now at last had time to begin sorting and analysing the immense fund of data he and his co-workers had amassed; indeed, he is still occupied in analysing some of it to throw new light on current questions as they arise. Now too he had time to turn from the practical problems of educational and vocational psychology to issues of a more theoretical nature. And it is here we must look for his major contributions to general psychology. He was already the first psychologist to apply Mendelian genetics to the study of mental inheritance, and his adoption of a multifactorial theory of heredity to account for the way mental abilities are distributed (notably in his studies of identical twins reared apart) will, if finally confirmed, have far-reaching practical implications. One of his most suggestive hypotheses is his theory of the hierarchical structure of mental abilities and emotional characteristics—a theory derived in the first

instance from Sherrington's conception of the structure of the nervous system, but now grounded on a firm experimental foundation as a result of his factorial techniques. His detailed knowledge of the physiology of the brain, gained from his work with Sherrington, has led to him not only to recognize the importance of basing psychological theories on firm neurological and biological foundations, but also to realize the limitations of an exclusively physiological interpretation of conscious phenomena. Thus, while making full use of a behaviouristic approach in his more practical applications, he has from the very outset vigorously opposed the dominant tendencies of the behaviouristic school in their efforts to eliminate all reference to consciousness and the use of introspection, and thus to 'elevate a methodological principle into a metaphysical dogma'. This is clearly displayed in his early work on mental imagery, and in his repeated insistence on the need to verify factorial interpretations by introspective reports. Probably none but specialists can follow his disquisitions on quantum theory and the principle of indeterminacy, and their bearing on the nature of neural processes at the synapses in the brain, or his conception of the mind 'not as a substance, but rather as a "field" in the technical sense as that term is used in "field theories" of electricity, magnetism, gravitation and the like'. But all can follow and appreciate his criticisms of the sense-datum theory and the traditional psychology of perception, his illuminating exposition of 'the concept of consciousness', and his emphasis on the need to recognize and study by systematic and scientific techniques the 'psychology of values', especially in the areas of aesthetics and ethics. He takes Sartre's 'moral nihilism' as 'a typical outcome of the crude and narrow postulates of modern psychoanalysis and determinism', and argues on the basis of empirical evidence that the contention of the existentialists and other positivists—that "the universe includes no objective values which we can intuitively perceive"—is bad psychology and false metaphysics'.

During and after his appointment at University College, Burt received many well-deserved honours. He was President of the British Psychological Society in 1942, became an Honorary Fellow of Jesus College, Oxford, received an honorary LL.D. from Aberdeen University and a D.Litt. from Reading, was elected Fellow of the British Academy, and was the first psychologist to be knighted. In 1950 he retired from his Chair. Since then he has been an active member of the Council of the British Psychological Society and many of its Working Parties, and has continued to give advice and assistance to research

students. But most of his time has been spent as Editor of the *British Journal of Statistical Psychology*—the kind of task which, as I can testify from my own editorial experience, is apt to become a full-time job.

And here I should like to add a word about the foundation of the journal and its policy, since I must be one of the few living members of the Society who took an active part in its inception. When I was editing *The British Journal of Educational Psychology*, I used to receive an increasing number of papers with a large amount of statistical material in them. (The publication of some of these led to complaints, especially by our Training College subscribers, because the statistics were beyond their comprehension.) My refusal of a few papers, excellent in themselves but too technical for our readers, led to a protest by Professor Godfrey Thomson, and finally to a proposal that a new journal dealing with mathematical aspects of psychology should be established. With this Burt, Spearman, Brown, Stephenson and others were in full agreement. A fund was started before the war. As soon as the war was over, plans were got under weigh, and as co-editor with Godfrey Thomson, Burt threw himself into the work and undertook the major part of the burden. Incidentally, I may mention, he paid for much of the necessary secretarial help—the retyping of manuscripts, and the heavy correspondence—out of his own pocket.

Burt, however, is no believer in 'statistical psychology' as an independent and self-contained branch. From the start his aim has been not only to provide an outlet for reports of statistical investigation, and to expound in relatively simple language the aims and the value of statistical techniques with a view to popularizing this all-important aspect of psychological research, but also to link statistical work with non-statistical, quantitative studies with qualitative. He has shown himself particularly concerned with the complex methodology of psychology as an empirical science, and with its philosophical bases. Here his eclectic standpoint has been of special service, as is evidenced by his work on the concepts of mind and consciousness, and his criticisms of the narrower approaches adopted by behaviourists and logical positivists. In editing the manuscripts that passed through his hands he aimed always at offering his readers a lucid and topical set of contributions, free from the slipshod grammar and unreadable jargon that nowadays disfigure so many psychological publications.

He himself was, as I can testify, a popular and witty lecturer, attracting large audiences not only in London, but in Birmingham and elsewhere. He was the first psychologist to broadcast talks over the wire-

less and by television, and was from the start an active member of the educational and technical committees of the B.B.C. Yet another instance of his versatility is his extensive knowledge of languages. As an old 'Greats' Scholar at Oxford one takes his Latin and Greek for granted. In addition he has a wide acquaintance with French, German, Italian and Indian literature. I myself once found him discussing the meaning of obscure words in Hebrew with an elderly Jewish scholar, when we were conducting an oral examination on a higher degree thesis in psychology; and he has contributed articles on both Hebrew and Hindu psychology.

Finally, I should like to pay tribute to Burt's generosity in helping others engaged in psychological research. His own advanced students know this well enough already; but it was brought home to me over many years when I submitted to him articles sent to me for the *British Journal of Educational Psychology*. He was not content with brief approval or disapproval; he corrected figures, made recalculations, offered various suggestions and constructive criticisms, often including detailed references for further study. In this way he has put many a young psychologist on his first path to advancement. Here too I must record my personal indebtedness to him for his generous help when I asked him to glance over the typescript of my first major book, *The Psychology of Early Childhood*; he returned each chapter with elaborate discussions, which were most useful, but embarrassed me with the thought of the time he must have spent.

Many others have experienced the same generosity. Nor has he entirely forgotten his earlier fields of work. He continues to take an active interest in the problems of the dull and the backward, the gifted and the supernormal, the delinquent, the neurotic and the criminal. This volume is a tribute to a great psychologist and a great man.

C. A. Mace

## Causal Explanations in Psychology

Behaviourism, it would seem, is on the way out. Psychology is regaining consciousness. The most recent evidence for this includes Cyril Burt's paper in the *British Journal of Psychology* on 'The Concept of Consciousness'<sup>1</sup> and H. D. Lewis's Presidential Address to the Aristotelian Society.<sup>2</sup> But the counter-attack of common sense and the retreat of the behaviourist began some years ago. The retreat began with the weakening of support from the philosophers. The behaviourists had been lucky in being carried on the crest of a wave in philosophy and 'the philosophy of science'. Bertrand Russell had sponsored them. So had the logical positivists and physicalists. So too had Wittgenstein in his own way and Gilbert Ryle in his. I, too, must confess to a long and very enjoyable flirtation with behaviourism<sup>3, 4</sup>, a flirtation which very understandably puzzled my colleague, Richard Peters.<sup>5</sup> Behaviourism has contributed massively to the advance of psychology, but it was based on a methodological dogma, an emotion and an intellectual confusion. The methodological dogma was a pronouncement to the effect that a datum for science must be 'public' and 'objective'. This is not a statement of fact. It is an edict, an arbitrary edict proclaimed no doubt precisely with a view to discrediting the evidence of an introspective witness. We may make words mean what we like only when we invent them. When we inherit them, we must use them in their established senses. 'Science' is a word we have inherited and its established use is for knowledge, systematic knowledge, however it is come by. Before the invention of the mercury thermometer the human body was as good a thermometer as there was. Some poor Roman soldier shivering on Hadrian's Wall might have said in colloquial Latin (freely translated), 'Blimey, it is cold here, colder than in Rome at this time of the year'. His companion might have said, 'How right you are, mate'. Each had read his own subjective thermometer, and their readings were data for a science of meteorology.

The case is not in principle very different when a physicist on Hadrian's Wall today, reading his mercury thermometer, says, 'It looks to me as if the temperature is about 27.5° F.' and his companion replies, 'Yes, that is what it looks like to me'. They are comparing notes on their subjective percepts. As Udney Yule pointed out a long time ago, scale reading is a subjective affair. Scale readers have very definite preferences for some points on the scale.

In the early days of their retreat behaviourists conceded that verbal reports, as a sort of behaviour, can be accepted as scientific evidence. This concession did them no good. When the Roman soldier on Hadrian's Wall reported that it was colder there than in Rome, it was not his verbal behaviour which constituted evidence for meteorology. It was *what he reported*—correctly or incorrectly. So, too, if he had said simply that he 'felt cold'. It is not his verbal behaviour which constitutes the datum for psychology. The datum for psychology is in *what he reports*, correctly or incorrectly, about his *feelings*. The methodological edict must be defied. The emotion that sustained behaviourism was *exasperation*: exasperation with the interminable disputes about, for example, imageless thought and the James-Lange theory of emotion. Exasperation, however, is not conducive to clarity of thought. It is one of the little ironies of history that the behaviourists took over from Titchener, against whom they rebelled, Titchener's major philosophical mistake. Titchener has recorded how in the year 1888 when he was reading James Mill's *Analysis of the Human Mind* the conviction flashed upon him 'You can test all this for yourself', meaning that he could test it by introspection. It was in the early 1950s soon after I had written my own defence of 'Analytic Behaviourism', that I had a similar dramatic revelation. Re-reading Titchener the conviction flashed upon me that this is where he went wrong, and where he misguided the behaviourists. What he had done was to confuse introspection with philosophical analysis. Titchener supposed that what James Mill was doing was simply introspecting and that he, Titchener, could test Mill's introspections by comparing them with his own introspections. This was a blunder of the first order. What Mill was doing was to suggest an 'analysis' of mental facts in the sort of way in which later G. E. Moore and philosophers who contribute to the journal *Analysis* have proposed philosophical analyses of mental facts. It is one thing for a poor shivering Roman soldier on Hadrian's Wall to say 'I feel cold'. It is quite another thing for him to tell a philosopher what 'feeling cold' is—a 'sensation', an affective state or just a bodily shiver.



It is one thing for a psychologist to report that he is 'exasperated' by arguments about imageless thought. It is quite another thing for him to say 'what exasperation is'—an organic sensation, or a 'pure feeling' which has its distinctive place on Wundt's tridimensional scale. Titchener can surely be trusted when he reports that in 1888 a certain conviction flashed upon him. I think I should be trusted when I report that in the early 1950s a conviction flashed on me that Titchener had made a mistake. Where Titchener and I are not to be trusted, in the same way, is when we go on to say, on the basis of 'introspection', that being 'struck by a conviction' is to experience a visual image, a kinaesthetic image or a pure imageless thought. The behaviourists can be trusted when they report, on introspective evidence (which they certainly use on the side), that they are exasperated by discussions of imageless thought. They are not to be trusted in the same way when they offer a philosophical analysis of what exasperation is. Here, like other psychologists, they are getting caught up in philosophical issues.

There is another way in which behaviourists, no less than other psychologists, have become entangled in philosophical discussions. That is through the philosophy of science. The 'philosophy of science' has always been preoccupied with the best established and the most advanced science; and psychologists interesting themselves in the philosophy of science have been unduly influenced by dicta, edicts and proclamations which are encouraged by the study of the established and advanced sciences—'Science establishes general laws', 'Science is prediction', 'Science proceeds in accordance with the hypothetico-deductive method', and so on. These are all important ideas. They are less helpful when they become ideologies. All sciences aim at explaining the facts they describe. But they first describe. Just to describe and systematize is regarded as a very humble activity, but it is important in the biological sciences. In psychology in particular no one has done more than Burt to show that factorial analysis is an important and elegant form of *description*. But having described human abilities in terms of statistical factors there remains the possibility of the need for distinguishing causal from descriptive factors. It is common ground that the psychologist has both to describe how people behave and to explain why they behave as they do.

There are several sorts of explanation. There is the sort of explanation, most characteristic of physics and engineering, in which the effect to be explained is deducible and predictable on the basis of universal laws. It is, for example, *deducible* from general laws, and *pre-*

*dictable*, how a spaceship will behave when it comes under the gravitational influences of the moon or Mars. Psychologists are apt to be depressed by the fact that they cannot give explanations of this kind. In psychology there is the sort of explanation of behaviour which begins by describing the behaviour in terms of the movements of limbs, and proceeds to account for this by reference to stimuli impinging on receptors, to traces and other variables which intervene between stimulus and response. Since Descartes proposed his very ingenious explanation of reflex action much has been done to elaborate and refine this sort of explanation. And such explanations do contribute to enlightenment. Explanations of this kind are refined and elaborated by reference to physiological and biological needs. There has been, however, a marked advance in psychology since interest has moved from 'needs' to 'wants' and 'likings'. At first psychologists took over a little uncritically the concepts of nineteenth-century biology—'self preservation' and 'species perpetuation'. These 'needs' were written about sometimes as if they were necessities; and sometimes as if they were categorical imperatives, as if an organism were under some compulsion to save its skin and perpetuate its kind, or under some sort of moral obligation to behave in this way. The dodo and the pterodactyl were under no such compulsion or obligation. If they could have left records of their views they might well have said that they preferred being the sorts of creatures they were even if their qualities lacked survival value. And this surely is the case with man too. Man is under no compulsion or moral obligation to survive and perpetuate his species, nor are the desires to survive and perpetuate his species important motivational dispositions. A man will gulp down a pint of beer after a long walk on a holiday, or sip a glass of cognac after dinner on a cool evening. It may perhaps be that drinking beer or sipping cognac meets some physiological need and has 'survival value'. But the explanation which the man will offer for his behaviour will not be given in this language. He will say that he drinks beer or cognac, *regardless of consequences*, because he likes the taste of beer or cognac or the effects of beer or cognac on his thoughts and conversation. Explanations of this kind advance beyond common sense and become science when they are amplified in the ways reviewed by Gardner Murphy in his discussion of 'How we come to want what we want'.<sup>6</sup> Explanations of why we want the curious things we do want, and like the curious things we do like, are given in terms of primitive drives or propensities and their modifications, in terms of the Freudian concept of 'cathexis', in

terms of the ethologists' concept of 'imprinting', of Allport's doctrine of 'functional autonomy', of Murphy's own account of 'canalization', and so on, including the traditional doctrine of association and the modern doctrine of conditioning. All these concepts contribute to enlightenment; but *none* of them, *not one* of them, enables us to deduce the effect from the cause or predict what people are going to like next.

The dictum that 'Science is prediction' has odd consequences when applied to the biological sciences in general and to psychology in particular. It is possible for example, that the senses of man should develop so that *visual* experiences would be produced by infra-red or ultra-violet stimuli. But it is not possible to predict what those visual experiences would be. It would seem that fashions are in principle unpredictable. Market research and experimental aesthetics are both concerned with what people like: what flavours they like in their chocolates, what sorts of pictures they like to hang up in their living rooms. Both kinds of research can be carried out under stringent experimental conditions and with statistical rigour. From both kinds of study it is possible to predict (if 'predict' is the word) that what was liked in the sample population will be liked in a larger population from which this sample was drawn. What it is not possible to predict is what flavour in chocolate will be preferred next year. There is something different in principle between predicting the weather and predicting change in public tastes. Theoretically it could be possible to predict the weather ten, twenty or fifty years ahead. If we say that 'theoretically' or 'in principle' it should be possible to predict aesthetic preferences ten, twenty or fifty years ahead some odd consequences follow. It is implied that in principle, at a time when Landseers were fashionable, it should have been possible to predict the later vogue for Picassos. This surely is odd, because it implies that psychologists should in principle have been able to imagine Picassos before Picasso did.

There is moreover something very implausible in the suggestion that what people like can be deduced from their physiological and biological needs. There are distinctive psychological needs and among these are the need to be conscious and needs for certain forms of consciousness. A *prima facie* case can be made out for the hypothesis that human life could be prolonged by the maximization of sleep. A case might be made out for the hypothesis that so called 'mental operations', such as calculations and the solution of problems, could be more effectively carried out during sleep. Perhaps a case could be made

out for saying that men might perpetuate their species while asleep. (Has that not been argued by the defence in certain cases of rape?) Suppose that all these things could be, that organisms could live longer, work more effectively and perpetuate their kind in complete unconsciousness—that man could be an efficient physiological computer and an automated reproducer of his kind. On this hypothesis he would do all the things he now does but would do them all unconsciously. It hardly requires an opinion survey to establish in fact that few people, if any, would choose this sort of life. Even behaviourists enjoy being conscious. They enjoy tasting food and wine, feeling the impact of the sun and the wind on their bodies, flirting and loving, as conscious experiences and they enjoy the intellectual excitement of defending behaviourism. They may even enjoy their exasperation with the disputes of introspective psychologists. What conscious experiences are enjoyable is not in general predictable until after the event. Psychologists perhaps must be content to be wise after the event, to wait for things to happen and then explain why they have happened.

Of course, it is a good thing for psychologists to study the principles and methods of the advanced sciences; but it is also a good thing for psychologists to have sufficient self-confidence to formulate their own principles and to design their own techniques. Ambition is a good thing, perfectionism is a good thing—but so are humility and single-mindedness and contentedness in doing a useful if unspectacular job. It was, I think, Alec Rodger who once remarked that psychologists would be earning their living even if they did no more than go round collecting the data for the calculations of Chi-squares. Cyril Burt has done more than this. He has gone around collecting data for causal propositions of the form

$$p = f(a+b+c+\dots) \text{ or } f(b+c+d+\dots) \text{ or } f(c+d+e+\dots),$$

where  $p$  denotes the probability that a given child (say) will become delinquent, neurotic or educationally backward;  $a$ ,  $b$ ,  $c$ , etc. stand for conditions or causal factors actually observed; and  $f$  denotes that  $p$  is regarded as a function of the group of factors specified in the equation, with each item given its due weight and the whole appropriately combined. The special implication of the formula is that (in Burt's own words) 'the result to be predicted (as a probable, and not an assured, deduction) springs, not from a single universal cause, but from a multiplicity of converging influences, often differing widely in their nature, their grouping, or their relative importance from one group to

another.' In forecasting a child's future success at school the formula may actually become a numerical equation, the probability depending on the child's marks for a *number* of tests, and not just on a *single* all-embracing test of intelligence. In the case of delinquency or neurosis the formula may not be explicitly stated in algebraic or numerical form, but the underlying logic and the implicit principles remain the same. This is the sort of causal proposition which is most characteristic of psychology. Psychologists do not establish laws of the kind: 'All delinquents come from broken homes', or 'All broken homes produce delinquents'. They do what Burt has done with such elegance: marshal evidence that delinquency, for example, is the consequence of a large number of *alternative* combinations of factors,  $(a+b+c)$  or  $(b+c+d)$  and so on to the end of an almost endless proposition.

There is a place in psychology for many kinds of psychologists. It was an interesting event when Cyril Burt succeeded Charles Spearman at University College. Spearman aspired to be the Newton of psychology. He tried to formulate a few general laws from which all the facts of mental types could be deduced and presumably predicted. Burt has not attempted this. He is a psychologist in the tradition of Francis Galton, not only in advancing the development of statistical methods in psychological enquiries, but in showing Galton's interest in *answering the question* rather than in formulating laws or methodological principles. Burt like Galton would, I am sure, be unperturbed by the suggestion that some things in the mental life of man are in principle unpredictable. There is a place in the world of psychology for all sorts of psychologists, but in this world psychologists like Galton and Burt are indispensable.

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*J. S. Wilkie*

## **Neurology and Free Will**

It is possible that we are on the eve of an advance in neurology much greater than that which followed Descartes' suggestion of a model for the brain of the higher animals. As is well known, Descartes denied the possibility of mechanisms simulating the operations of the human mind, and supposed the mind to possess a purely external relation to the brain.

It is clear, however, that at least some operations properly regarded as mental can be simulated by artefacts, and the existence of these gives us good hopes of producing a much more ambitious model of the active brain than that which Descartes felt it possible to present.

It is natural to ask whether there is any foreseeable limit to the powers of such a model; whether, in its final development, it will enable us to explain all mental operations, to express them, that is, as neural operations; and whether, in particular, it will enable us to dispense with the notion of free will.

At the beginning of what appears to be a stage of rapid advance we inevitably feel ourselves somewhat at a loss and are most unwilling to speculate as to what might or might not be achieved in the future; in our present case, it is only the electrical engineer, if anyone, who is competent to form even a guess as to the direction and extent of possible developments.

There is, however, one very simple way in which the layman, in matters of neurology and of electrical engineering, can without immodesty form for himself some judgement respecting the implications of the new method, and that is to suppose it completely successful. What would count as complete success in the production of a theory of the brain would be the demonstration that all mental operations could be performed by specifiable neural mechanisms, together with proof that such mechanisms actually exist. This would, I think, amount to the assertion that an artefact capable of simulating all overt

human behaviour could be constructed; though, as I hope to show, the cost of making such an artefact might well be incommensurate with any possible utility it could have.

Would the existence of such a theory cause a revolution in psychology, in sociology and in philosophy? In particular, would it enable us to say categorically that even our most important choices are, in spite of appearances to the contrary, free only in some entirely trivial sense? For, of course, many of our actions are necessarily in some sense free: there is a difference between walking downstairs and being thrown downstairs.

Obviously a completed theory of the brain would be in many ways of the greatest service, but in some respects it would leave us very much where we are at present.

In the first place, the classical mind-body problem, so far from being solved, would be in at least one way more puzzling than it is at present. For as long as we can believe that some of our operations could not be performed apart from consciousness, we have at least a teleological explanation of the association of consciousness with a brain; we can assign a function to the mind and say that, without it, the animal organism could not survive, indeed, could not exist.

But the existence of our theory and the possibility of making our artefact would show that there is no reason, that we can understand, why the activity of a brain should be associated with conscious experiences (cf. Leibniz, *Monadology*, 17).

Nor, to take a more practical view of the matter, would our completed theory be at all likely to put psychologists or sociologists out of business. For we must be careful not to suppose that a model of the brain, however perfect, would in itself enable us to deduce or to predict the behaviour of an individual or of a community, in the way we might hope to predict the properties of water from the consideration of a model of a water-molecule (assuming, of course, that this model was not simply an economical way of describing the known properties of water).

Not only in human behaviour, but even in human anatomy there is a huge element of historical causation. A complete theory of the brain would be a physical theory, incorporating in its general principles nothing that is not to be found in the physical world generally apart from brains and even apart from living things. But the theory in its applications would have to accept as given the anatomical structure of actual brains; and, if we are to attach any importance to the theory of



evolution, actual brains are the products of historical processes; but of far greater importance, of course, when we come to consider the application of the theory to human beings, is the factor of historical causation in the production of actual communities and of actual individuals. To explain the present or to predict the future behaviour of a person, an artefact would have to be programmed with the education and past experiences of that person. Or if the explanation or prediction were to be made on the basis of direct knowledge of the present state of the subject's brain, a study of immense complexity would have to be undertaken, involving thousands or millions of readings; a study, moreover, which would be both tedious and alarming for the subject, and could hardly be itself totally without influence upon his conduct.

As for sociological studies conducted on these principles, even a computer accustomed to large-number calculations might well blanch at the calculated cost of such a proceeding.

Thus the present methods of psychology and of sociology would be for most purposes incomparably more economical than those of neurology, and it is most improbable that they would be superseded.

Let us now consider whether our completed theory would enable us to get rid of free will.

A completed theory of neurology would be a statement of the rules by which nervous systems in general and brains in particular operate. The motor output of a brain will, of course, depend not only on these rules of operation and upon the given innate structure of the particular brain, together with 'structuring' due to the experiences of the individual organism, but also upon information delivered by the sense-organs. Hence the brain's activity at any moment is due to the nature and state of the brain itself and to influences coming through the senses from without. But what seems to be claimed by the statement that there are acts of will which influence conduct is that there are at least from time to time, over and above the understandable physiological external influences, quite incomprehensible influences acting upon the system from without.

That there is nothing fundamentally silly about this claim can be seen from the fact (or what we surely all believe to be the fact) that one consequence of cerebral activity is conscious experience. If cerebral activity has inexplicable consequences, there is nothing foolish in supposing that it *might* be subject to inexplicable influences.

Would there be, under the assumptions we have made, any possibility

of showing that such inexplicable influences either do or do not exist?

If we have an isolated system of which we know the rules of operation, and if we possess all relevant information about the state of our system at any moment arbitrarily chosen, we can say what its state should be at all times, past or future, so long as it remains effectively isolated. We can, therefore, decide whether or not it has, between two assigned times, suffered any effective interference from without.

For very simple systems governed by unambiguous rules it is possible to do this in fact, as well as in principle, but the more complex the system the more difficult it becomes to know its state at any time, and hence the more difficult to decide what its state should be at future times. Even with quite complicated systems, however, it may still be possible, by treating the elements (or 'particles') of the system no longer individually but statistically, to know enough of its state at one time to be able to say at a later time that the system has suffered no *major* interference from without during the interval.

Applying these considerations to the brain, we see that, complex as the system is, it might conceivably be possible to know not only its laws of operation, but also approximately its state at a time and the sensory input during an interval sufficiently well to assert that no inexplicable *major* disturbance had been suffered by the system during the interval; or, conversely, to assert that such a disturbance must have occurred. Thus, though no proof would be possible, it might be within our power to show that the supposition of external influences was either extremely plausible or extremely implausible, provided it was known that the physical consequences of an act of will *must* be a *major* disturbance of the determinate functioning of the brain.

I want now to argue that there is absolutely no reason to suppose that the consequence of an act of will must be (though it might be) a major disturbance of the brain.

Let us consider somewhat more in detail what we are attributing to the brain when we suppose it capable of explaining all overt behaviour. It is clear that, if we are to explain human activities as results of neural processes, we must suppose both concepts and rules for operating with concepts to be represented in the brain. I want to make sure that the sense in which I here use the word 'represented' is not open to misunderstandings. Of course concepts cannot as such be present in (in the sense of being ingredients of) either the brain or the artefact; neither will rules be present in these systems exactly as they (the rules) exist in conscious awareness.

Consider, for example, the concept 'one'. There is no reason why this concept should not be somehow *represented* in the systems we are considering, and represented in such a way that it can take part in operations such as addition and multiplication. Experience with even the simplest mechanical computers shows one way in which this can be understood. By analogy with computers it seems not unlikely that this concept would have to be represented a large number of times in the brain; and from its use in contexts other than that of simple arithmetic it is probable that the representation would be as complex as the concept is (at least superficially) simple. But one thing is certain: the description of the cerebral representation, or representative, would necessarily require reference to nerve-cells and their connections and to electrical charges, and very likely to certain large and highly complex molecules which (by analogy with genes) might turn out to be the ultimate repositories of most classes of memories. None of this, it need hardly be said, appears, or could appear, in any possible description of the concept; for if it did, how on earth could we attach any meaning to the expression 'one nerve-cell'?

Similar considerations apply to the representation of rules of all kinds, and, of course, we should have to allow for the representation in the brain, not only of rules for intellectual operations, but also for social conduct. It might seem, to those who take them seriously, that moral laws could hardly find neural representation, but in fact there is no serious objection to supposing that they could, if representation is understood in the way I have explained. Indeed, common experience seems to suggest that they do: for moral conduct, fortunately, tends to become habitual and moral judgements, unfortunately, tend to become stereotyped.

Elementary representations would not, of course, be combined simply and solely according to abstract and general rules, such as those of arithmetic or morality. The brain of an adult capable of living in civilized society must be supposed a most complex system of systems, and among the most important of sub-systems must be those which we may call the representations, or representatives, of major interests.

It is clear that men have clusters of habits, rules and desires and that these clusters are to some extent autonomous, and responsible for behaviour within particular sets of circumstances; for example, in the home, at work and in the exercise of leisure activities. The degree of autonomy of such interests is often astonishing, though of course it cannot be more than relative in any normal subject. Moreover, in

some persons the various interests they entertain seem to be approximately equal in value; whereas others cultivate an interest which seeks to dominate the rest. Thus in some, religion or an abstract system of morality permeates and directs all other spheres of activity. In others, an interest in wealth or in social position is overriding, so that work becomes an instrument for attaining these advantages, and home and leisure activities become occasions for their display. But, of course, of the three first-mentioned interests, the home, work and leisure activities, any one may become immensely more important than the others.

We have now, I think, established an adequate context for the discussion of the degree of physical intervention that it would be necessary to postulate as the immediate consequence of an act of will. We may begin by remarking that it is difficult, though doubtless not impossible, to think of the act of choice as itself quantified. Some acts of choice may be more important, more meritorious or more disgraceful than others, but there appears to be some absurdity in speaking of 'a very strong choice' or of 'a very weak choice'. The expressions 'a strong will' and 'a weak will' may, for all I know, be fittingly applied to the will in its own nature; but it is at least possible that they more properly characterize an habitual condition of the nervous system (using 'habitual' in the widest possible sense); and the sense of effort that accompanies some acts of the will certainly may, as the word 'sense' already suggests, arise from powerful conflict between neural systems, the rival claims of which the will is called upon to decide.

Let us now consider the occasions upon which we should be most inclined to say that an act of will is called for. Obviously, not all choices are free, unless perhaps potentially, for we should be disinclined to think of the will as often concerned with trivialities: indeed, the reason men cling with so much pertinacity to the notion of free will is that freedom and responsibility are considered to be convertible terms: to say that not one of my acts is freely performed is to say that there is *nothing* that I do for which I am in any important sense responsible.

At least in the following sets of circumstances it seems to me that a free and responsible choice is called for.

1. When an indecision concerning trivia becomes troublesome.
2. When the attention must be held upon a tiresome problem.
3. When an instinctual activity must be restrained.
4. When a decision must be made between the claims of major interests.

5. When we accept a law or principle of action that has serious consequences.

A few notes are perhaps necessary in amplification.

1. Choosing from a menu seldom, we may suppose, calls for an act of will; but good manners require that we should not delay too long in choosing. Thus, when two dishes appear equally attractive, a particular act of decision is needed, and this deserves to be considered a responsible choice in so far as it is motivated by good manners, which are a great part of morality; perhaps, properly understood, the whole of morality.

2. Attention may be maintained far beyond the point of fatigue if the subject of attention is interesting; but when it is not, an act of will is called for if there be some good reason why attention should be further sustained.

3. Among instinctual activities requiring restraint it is natural to think of sexual impulses which, even in the most libertarian society, cannot always be unconstrained. However, to think only of sex in this connexion belongs to persons living in unusually easy circumstances; under more exacting conditions restraints upon impulses caused by fear, hunger or the need for sleep may be needed more frequently than restraints upon sex.

4. A conflict between the claims of major interests is, I suspect, among the commonest of the situations giving rise to serious moral problems. A man feels that the circumstances of his home are a hindrance to his success in business, or he finds, like Gauguin, that what began as a pastime has become more important to him than home and work together.

5. It is difficult to see how a principle of action could ever be accounted a moral principle unless it was at some time explicitly and freely accepted by the subject acting upon it, and is still while he acts upon it implicitly freely accepted.

In none of these cases does it seem at all necessary to suppose that the will would have to generate any great amount of energy or any powerful force to produce a decisive physical effect.

In the first case, obviously not. In the second, when we maintain attention against fatigue and boredom we do so for some good reason; the activity is not unmotivated, it is simply no longer motivated by the intrinsic interest of the subject. But, by our hypothesis, our

reason for the continuation of attention has its representation in the brain, and this representation is a physical system having (physical) potential energy at its disposal. All the act of will would have to do in this case would be to ensure that this energy is exploited.

In deciding between the claims of major interests we are deciding between two or more powerful cerebral systems; so that the act of will always has, as it were, already at least one powerful ally in the field. Unruly instinctual impulses are commonly opposed to at least one major interest, apart from being themselves opposed by the cerebral representations of moral convictions.

Finally, the acceptance of a moral principle is the acceptance of something that we have been taught from childhood, or something that is being, here and now, offered to us with powerful arguments. In the first case, we decide in favour of an already powerful system, though doubtless against powerful solicitations or plausible arguments tending to destroy the system. In the second case, arguments can only be cogent for us if they appeal to principles that we already accept, and these must, of course, under our postulate, be supposed to have cerebral representation, and hence their own physical dynamism.

In fact, if we accept the postulate of maximal representation, the account of what may be supposed to occur in the brain when a responsible choice is made is the same for the voluntarist as for the mechanist; and neither has the slightest chance of refuting the other's position by any possible observation or experiment. In cases such as those I have considered, and which seem to me to be sufficiently representative, the voluntarist can always assert that the final decision is due to an act of will; the determinist can always say that, the neural forces being well balanced, the decision is due to some unrecorded minimal additional stimulus, or to molecular noise in the brain itself. The finest instruments conceivable would not enable us to eliminate these hazards.

My conclusion, if unexciting, is at least unalarming: no advance of neurological science has the least chance of deciding one way or the other the dispute between determinists and voluntarists, and the neurologist who believes in free will can pursue his science with the same rigour and with the same hopes of unlimited advance as those with which other scientists pursue their aims. He need not fear that at some point the development of his science will be halted by the manifestations of some unpredictable and disorderly entity called 'the will'; voluntary choices being moral choices must, if they exist, be

eminently orderly; and being choices made by ordinary human beings concerning the ordinary affairs of human life, they are not to be expected to act *totally* from without upon a brain in no way prepared for them. The brain is not an immoral machine requiring constantly to be set upon the rails of morality which it as constantly seeks to leave.





Arthur Koestler

## Motivation: A Biased Review\*

### RETROSPECT

Future historians will probably regard it as both odd and significant that throughout the first half of the twentieth century the major schools in psychology—even schools so far apart as behaviourism and psychoanalysis—recognized only one basic type of motivation, and that a negative one: the reduction of biological needs and drives, the diminution of tension, or simply the escape from anxiety;† and with both American behaviourism and continental psychoanalysis the actions resulting were assumed to be the outcome of strict causal determination. Burt's attitude to the problem was in marked opposition to such a view; and in this he was, as he emphasizes, continuing the British tradition: Ward, Stout and McDougall all regarded behaviour of living, conscious organisms as essentially 'free and active', in contrast to the behaviour of inanimate and unconscious objects which is 'passive and compulsory'. Positive striving, and in plain English free will, was the distinctive feature of mental life.

'The aim of Pavlov, Watson and Freud,' says Burt, 'like that of the older associationists and reflexologists, was to turn the philosophy of mind into a natural science, by copying the methods of the physical sciences and borrowing their basic principles. The causes of human behaviour were to be expressed as quasi-physical laws, modelled on

\* Koestler's 'Motivation: A Biased Review' was written for the present volume and was to be included later on, with the agreement of the editors, in his book, *The Act of Creation*. Owing to unavoidable delays in the publication of the present volume, the order of publications was reversed, and Koestler's book came out first; the editors now wish to extend their thanks to Messrs. Hutchinsons for permission to include in the present book part of Chapter VIII from Part Two of *The Act of Creation*.

† 'At the level of ego-psychology,' wrote Mowrer in his survey on 'Motivations' in the *Annual Review* for 1952, 'there may be said to be only one master-motive: anxiety.'

those of mechanics or thermodynamics, but often thinly disguised by a change in terminology—the law of least action (re-named “least effort”), the restoration of equilibrium (re-named “search for security”), the flow of potential from a higher level to a lower (re-named “reduction of tension” or “need”, or “removal of stimulation”, etc.). The brain, in short, is to be treated as a machine; and mind, consciousness, conation and will are to be ignored or rejected as “concepts not found in the natural sciences”.

‘But even in the simple behaviour of the animal or tiny child, motivation of this type will account for only part of its conduct. The creatures in the zoo live in an almost ideal welfare state: well housed, well fed, relieved of any need to flee from danger or hunt for prey, they should be lolling in a paradise of stable equilibrium. Instead, when the environment provides no stimulus or need, they actively create it. For them Nirvana means not bliss, but boredom verging on neurosis. The impulses to play, to explore and to make involve a positive search for effort and often a joy in insecurity. Water can only run downhill; the child runs up. On a higher plane the behaviour of the intelligent adult often seems to obey a law of greatest action and the multiplication of needs. A space pilot, comfortably ensconced in a chamber where he is deprived of all stimulations, starts thinking up mathematical problems to solve in his head. The most satisfying of all pursuits—research in pure science, for example—are from their very nature progressive and unending; and it is the exertion itself, not the reduction of it, that is relished and enjoyed. “To travel hopefully”, says Stevenson, “is better than to arrive”; or Emerson, “Were I to hold the truth in my hand, I would let it go for the positive joy of seeking”.’<sup>22</sup>

The trend towards a theory of purely negative motivation seems to have had its origin in the climate of the Darwinian revolution independently in Germany and America, with Fechner’s (1873) *Tendenz zur Stabilität* and Thorndike’s (1898) *Law of Effect*. Freud, acknowledging his indebtedness to Fechner, postulated his own Principle of Parsimony, according to which ‘the course of mental events is invariably set in motion by an unpleasurable tension, and it takes a direction such that its final outcome coincides with a lowering of that tension.’ Thus pleasure is derived from ‘the diminution, lowering or extinction of psychic excitation’ and ‘un-pleasure [*Unlust*, dysphoria, as distinct from physical pain] from an increase of it’. The organism tends towards stability—a kind of homeostasis, applied not only to autonomic

regulations but also to voluntary behaviour; it is guided by 'the striving of the mental apparatus to keep the quantity of excitations present in it as low as possible or at least constant. . . . Accordingly, everything that tends to increase the quantity of excitation, must be regarded as adverse to this tendency, that is to say, as unpleasurable.'<sup>6</sup>

Now this is of course true, in a broad sense, insofar as the frustration or satisfaction of primary biological needs is concerned. But it passes in silence a whole class of experiences to which we commonly refer as 'pleasurable excitement'. The preliminaries of love-making cause an increase in sexual tension and should, according to the theory, be unpleasant—which they are decidedly not. It is curious that in the works of Freud there is no answer to be found to this embarrassingly banal objection.\* The sex-drive in the Freudian system is essentially something *to be disposed of*—through the proper channels or by sublimation; pleasure is derived not from its pursuit, but from getting rid of it. One might argue that in Freud's universe there is no place for amorous love-play because Freud, like D. H. Lawrence, was basically a puritan with a horror of frivolity, who treated sex *mit tierischem Ernst*. But arguments *ad hominem* do not explain the general trend in the first half of the century to interpret motivation as something negative. As Hilgard ruefully remarked, 'the *Zeitgeist* favoured our seeing incentives not as providing something sought after for what was inherent in the incentive, but something providing relief. The incentive was seen as an avenue of escape from pain, anxiety, tension.'<sup>10</sup> Just as Freud's libido-theory had no room for dalliance, so learning-theory had no room for curiosity or learning-by-play.

Thorndike's 'Law of Effect' was essentially a stick-and-carrot theory: the reward (and to a lesser degree, punishment) is the factor which stamps in the correct responses in learning, and stamps out the incorrect ones. In the extreme behaviourist systems of Watson and Guthrie, the mechanization of the living organism is complete: contiguity is the basic factor in producing associative S-R bonds, and motivation has virtually disappeared from the picture. Nor is any theory of motivation allowed to enter into Skinner's concept of 'operant behaviour'. His system is by programme confined to the description of

\* Ernest Jones says in his biography: 'Freud partook in much of the prudishness of his time, when allusions to lower limbs were improper.' He then gives several examples—such as Freud 'sternly forbidding' his fiancée to stay with an old friend, recently married, who, as she delicately put it, 'had married before her wedding'. (Vol. I, p. 142).

experimental operations, preferably in quantitative terms. The effects of different rates and sequences of positive and negative reinforcers are counted and plotted; the entity on which they act is the 'operant strength', which in turn is measured by the rate and number of responses during extinction; but the motivation of the animal is represented by a single, crude variable: the number of hours in which the rat has been deprived of food. Optimum learning results from the combination of the appropriate number of hours of deprivation with the appropriate rate of applying positive reinforcers, i.e. stimuli of the type, one might say, which are apt to deprive the organism of its deprivation. Differences in learning ability between various species, or of age groups within a species, are not considered to be relevant to this type of 'functional analysis' of behaviour. By the same method of selective reinforcement, by 'baiting' each step in a series of steps, pigeons can be trained to describe a figure-of-eight with their heads held high, and students can be trained to select the right answer among several alternatives and to punch it into the tape of the learning machine—the reinforcement in this case being that the tape moves on to the next question. Since each reinforcer is a drive-reducer, learning becomes a process of progressive de-motivation.

Hull did not share Skinner's rigidly positivistic, *hypothesis-non-fingo* attitude. He kept elaborating and modifying his theory until his death in 1951; the system has been described as the last and most impressive attempt to build an edifice on S-R foundations. His emphasis gradually shifted from primary to secondary drives and secondary rewards; and from need-reduction to drive-stimulus reduction (eating eliminates the *stimulus* of the hunger-drive but not the biological *need*—which will only be satisfied later by digestion). This made the system more elastic, yet in spite of these refinements, the primitive drives of hunger, sex and avoidance of pain were considered to be the only motivational factor in learning. To quote Hebb's summing up of Hull's theory: 'Its weakest point, and clearest departure from the facts, is in the treatment of motivation as biological need . . . According to the theory, the rat in the maze should learn nothing about it until one of his responses is accompanied by a decrease of hunger or thirst, or escape from electric shock, or some similar reward. In actual fact, when he is allowed to run in the maze without reward or punishment, the rat learns a good deal about it. . . . It is clear of course that the primitive drives of pain, hunger and sex are often of overwhelming importance. We need an approach to motivation that neither minimizes these things nor fails

to provide for the unrewarded learning that also occurs when the animal's belly is full and his sex drive satisfied.'<sup>9</sup>

If we turn to the opposite camp—Tolman and the Gestalt psychologists—the emphasis shifts from the need-reducing to the goal-seeking aspects of behaviour. In classical Gestalt theory, motivation by rewards, usually in the form of bananas, is taken for granted; it does not question the effect of reward on learning, the dispute is about whether this effect is achieved by stamping in or by insight. Similar considerations apply to Tolman's sign-learning theory, although he had progressed a considerable step further by his explicit rejection of reinforcement theories, by his emphasis on 'expectancy' and 'purposiveness', on latent learning and 'creative instability'. Lastly, Kurt Lewin's 'psychological field theory', with its complex and changing motivation, its concepts of 'ego-involvement' and 'levels of aspiration'; above all with its notion of striving after 'success' (which is subjective and relative in contrast to reinforcement by tangible rewards), played an important part in promoting that change of climate which has been increasingly noticeable since about 1950.

#### **DECLINE OF THE REFLEX**

This new orientation seems to be the cumulative effect of independent developments in several fields, such as: (a) disillusionment regarding the utility of the reflex-formula both in neurology and psychology; (b) re-discovery of the fact that organisms are not passive masses of software reacting to environment, but 'open systems', feeding on 'negative entropy', engaged in spontaneous activities on all levels; and (c) that animals are capable of 'latent' learning in the absence of tangible rewards, motivated solely by their exploratory drive.

The *physiological* concept of the reflex arc, which even Sherrington considered as no more than a 'useful fiction', has become an anachronism.\* The Pavlovian conditioned reflex was another useful fiction which exercised at first a stimulating, than a paralysing effect—a phenomenon frequently met in the history of science. In Hebb's words, 'Pavlov has deservedly had a great influence on psychology, and his theory has been rejected not because it is too physiological, but because it does not agree with experiment'. There is no need to recapitulate

\* 'The simple reflex is probably a purely abstract conception, because all parts of the nervous system are connected together and no part of it is probably ever capable of reaction without affecting and being affected by various other parts.' (Sherrington, *The Integrative Action of the Nervous System*, 1906 p. 8.)

the evidence which has led to this rejection. 'Conditioning' is still a useful term when applied to induced changes in glandular and visceral reactions, but leads to confusion when used in a loose, analogical way for other types of learning.

The last blow to the reflex-arc concept came with the discovery that it was impossible to make a precise distinction between 'stimuli' and 'responses'. Not only motor units, but also sensory receptors display constant spontaneous activity in the absence of external stimulation. External events alter the pattern of this spontaneous activity, but this in itself does not yet constitute a stimulus. The receptors are under efferent control from higher levels of the central nervous system; the acceptance, suppression or modification of the input starts on the periphery, and the centre decides what shall constitute a stimulus and what shall not. Even the stretch-sensitive receptors in muscle spindles are controlled by efferent fibres from the centre. In other words, 'stimuli' and 'responses' are not one-way processes, and cannot be isolated: 'because stimulus and response are correlative and contemporaneous, the stimulus processes must be thought of not as preceding the response but rather as guiding it to a successful elimination of the incongruity. That is to say, stimulus and response must be considered as aspects of a feedback loop. . . . These properties are a far cry from the ubiquitous S-R reflex arc diagrams that grace (more appropriately one wants to say "disgrace") today's texts.'<sup>17</sup>

It is historically interesting that an independent but parallel 'softening-up' process of the hard and fast S-R concept took place at the same time in psychology, e.g. in Skinner's and Hull's systems. Skinner was careful to state that he used the word 'reflex' not in an anatomical or neurological sense but as a purely psychological, descriptive term for the 'unit of behaviour'. But his definition of the unit was constantly shifting and changing. 'A reflex is not, of course, a theory. It is a fact. It is an analytical unit, which makes the investigation of behaviour possible. . . . The appearance of smooth curves in dynamic processes marks a unique point in the progressive restriction of a preparation, and it is to this uniquely determined entity that the term "reflex" may be assigned.'<sup>18</sup>

As Miller and his collaborators were to comment, to define the reflex in terms of the smoothness of curves is a 'somewhat odd approach'.<sup>14</sup> Yet even so it did not work: 'Skinner's "unit appropriate for experimental study" turns out, in fact, to have a measure of arbitrariness about it. . . . Sometimes the functional unit is a simple response, sometimes a

complex act, sometimes a rate of responding. The unit no longer has the clean dimensions of a correlation between a class of stimuli and a class of responses as implied in the original concept of a reflex. The atom of behaviour proves to be evasive.<sup>10</sup> In the later versions of Skinner's system the stimulus no longer even precedes the response: in operant behaviour the organism emits responses in search of a stimulus, as it were. The reflex as a unit of behaviour has evaporated like the physicist's hard little lumps of matter.

Skinner's experimental work has some lasting merits. He was among the first to demonstrate that 'intermittent reinforcement'—where only *some* correct responses are rewarded—can be as effective as the consistent rewarding of *all* correct responses. Humphreys then showed that random rewards are actually a superior (more extinction-resistant) form of training—the rat thus trained is less discouraged when the reward is withheld, than the rat trained by the consistent-reward method. From hence there was only one logical step to Tolman's theory of motivation by expectancy—a step which Skinner never took.

In Hull's case the 'softening-up' process took a different course. In his later years, Hull's attention shifted more and more from primary biological drives to secondary drives (from the 'need' to the 'taste' or 'appetite'). These secondary drives he saw manifested in anticipatory events—'fractional antedating goal-responses, and 'fractional antedating goal-stimuli'. 'The fractional antedating reaction ( $R_G$ ) with its proprioceptive stimulus correlate ( $S_G$ ), provides for the "automatic (stimulus) guidance of organismic behaviour to goals".' The great importance Hull attached to this postulate is illustrated by his comment: 'Further study of this major automatic device presumably will lead to the detailed behavioural understanding of thought and reasoning, which constitutes the highest attainment of organic evolution. Indeed the  $R_G \rightarrow S_G$  mechanism leads in a strictly logical manner into what was formerly regarded as the very heart of the psychic: interest, planning, foresight, foreknowledge, expectancy, purpose and so on.'<sup>11</sup>

From a strictly logical point of view, the postulate makes no sense—as Hilgard has pointed out in a careful analysis—because  $S_G$  acts at the same time as a *producer* of the secondary drive  $S_D$  and as a secondary reinforcer which *reduces*  $S_D$ . Hilgard regarded this confusion as a sign of logical weakness in Hull; yet he did not seem to realize that Hull was intuitively on the right track, that the contradiction is merely an apparent one, and vanishes if one stops thinking in terms of need-

reducing motivation. 'That a stimulus associated with reinforcement could become at once *both* a drive and a reinforcing agent' *sounds* like a contradiction, but makes eminent sense if, getting rid of the dreadful terminology, we translate it as follows: 'A rewarding experience can at the same time be both an incentive and a reward;' or even simpler: 'Some pursuits are self-rewarding.' That is the implied conclusion of Hull's eighth and last postulate which he regarded as the crowning achievement of his system. That he himself did not realize this implication only shows that the once useful S-R formula had by that time become a straight-jacket to thought.

### HUNGER, FEAR AND CURIOSITY

It took natural philosophy nearly a thousand years to re-discover that the earth is round; it took experimental psychology nearly fifty years to re-discover, after its Dark Ages of need-reducing S-R theories, that rats and men are pleasure-seeking creatures, that some activities are pleasurable self-rewarding and that exploring the environment, solving a chess problem or learning to play the guitar are among these activities.

An interesting reflection on the spirit of the times was the long, impassioned controversy which followed the earth-shaking discovery that rats who were allowed to familiarize themselves with the maze by running round in it without reward, got quicker to the food-box when this was eventually put in than the control rats who ran the maze for the first time. How could the rat profit from its previous experience in the maze without being rewarded by food or punished by electric shock? As Berlyne put it: 'There are plenty of experiments to show that latent maze-learning can occur in the rat, which is embarrassing for those whose theories are not built to assimilate it. Where does the reinforcement for these responses come from? Several writers have considered the possibility that it comes from the reduction of curiosity.'<sup>2</sup> Other writers suggested that it came from the drive-reducing diminution of boredom. One might as well say that composing a song is a silence-reducing activity.

There have been throughout these Dark Ages 'voices crying in the wilderness', but they were dismissed as old-fashioned. Thus McDougall kept reaffirming that striving towards a goal was often more satisfactory than reaching it. Allport held that activities originally derived from biological needs may become autonomous and self-rewarding: 'the characteristic feature of such striving (he says) is its resistance to



equilibrium; tension is maintained rather than reduced.' Goldstein emphasized the tendency of organisms towards 'self-actualization'. But the revival of a dynamic psychology which reinstated the academic respectability of such terms as 'curiosity', 'exploratory drive', 'purpose', only came about when experimental evidence showed that even in the rat the urge to explore may prevail over hunger and fear.

The experiments of Harlow, Montgomery, Butler, Hudson, etc. on rats and monkeys showed—what naturalists had always known—that animals are inquisitive, that they have an urge to manipulate, explore, to 'see what's inside', which is independent of such biological drives as hunger, sex and fear—or, rather, that the exploratory drive itself stems from a primary biological need. They showed that exploratory behaviour may combine with, or enter the service of, hunger or fear, but that it may also compete with and sometimes assert itself against them; and that novelty, surprisingness or puzzlement are as real incentives to learning as pellets of food dropped into the Skinner box.

As far back as 1930 Nissen had found that rats would cross an electrified grill to reach a maze which contained nothing but some unusual objects; he concluded that an exploratory urge did exist—'a biogenic drive to explore, perceive, to know'.<sup>14</sup> Experiments by Hudson, Berlyne and Walley, in which rats were punished for approaching some novel visual pattern, led them to conclude that 'objects that have become associated with danger are often explored before they are shunned'.<sup>15</sup> Carr and Williams showed that the exploratory drive varies with heredity and environment: hooded rats explore more than black rats, and black rats more than albino rats. Montgomery and Barnett showed that wild rats are more frightened, tame rats more attracted, by novelty; Thompson and Heron that young animals are more curious than old ones, and—as one would expect—that female rats are more inquisitive than males. Confronted with novel situations, hungry rats interrupt their feeding to explore their surroundings; but rats whose cerebral cortex has been removed in part, while still capable of learning to run a maze to get at food, show a diminished tendency to exploration. They 'do not evince the preference for a variable path over a standardized path that is characteristic of a normal rat, except when the variable path is the shorter. Brain-damaged rats likewise show less variability of route in a Dashiell maze.' Yet, as Lashley's rats have shown, even depriving the creature of substantial portions of its brain does not make it conform to the S-R ideal.

On higher levels of the animal kingdom the evidence becomes less monotonous and depressing. What a relief to get out of the Skinner box and to read Lorenz's description of curiosity battling with fear in one of his birds:

'A young raven, confronted with a new object, which may be a camera, an old bottle, a stuffed polecat, or anything else, first reacts with escape responses. He will fly up to an elevated perch and, from this point of vantage, stare at the object literally for hours. After this, he will begin to approach the object very gradually, maintaining all the while a maximum of caution and the expressive attitude of intense fear. He will cover the last distance from the object hopping sideways with half-raised wings, in the utmost readiness to flee. At last, he will deliver a single fearful blow with his powerful beak at the object and forthwith fly back to his safe perch.' In the end 'he will grab [the object] with one foot, peck at it, try to tear off pieces, insert his bill into any existing cleft and then pry apart his mandibles with considerable force. Finally, if the object is not too big the raven will carry it away, push it into a convenient hole and cover it with some inconspicuous material.'<sup>13</sup>

As for primates, we can comfortably fall back on Darwin's *Descent of Man*:

'All animals feel *Wonder*, and many exhibit *Curiosity*. They sometimes suffer from this latter quality, as when the hunter plays antics and thus attracts them; I have witnessed this with deer, and so it is with the wary chamois, and with some kinds of wild-ducks. Brehm gives a curious account of the instinctive dread, which his monkeys exhibited, for snakes; but their curiosity was so great that they could not desist from occasionally satiating their horror in a most human fashion, by lifting up the lid of the box in which the snakes were kept.'

Darwin was 'so much surprised at this account' that he proceeded to the monkey-house at the Zoological Gardens armed with a stuffed snake, a dead fish, a mouse and a live turtle: 'The excitement thus caused was one of the most curious spectacles which I ever beheld.' The greatest success was the turtle. The monkeys 'showed unbounded astonishment, as well as some fear. . . . This was displayed by their remaining motionless, staring intently with widely opened eyes, their eyebrows being often moved up and down. Their faces seemed somewhat lengthened. They occasionally raised themselves on their hind-legs to get a better view. They often retreated a few feet, and then

turning their heads over one shoulder, again stared intently. . . . In the course of a few minutes some of the monkeys ventured to approach and touch the turtle. . . . I then placed a live snake in a paper bag, with the mouth loosely closed, in one of the larger compartments. One of the monkeys immediately approached, cautiously opened the bag a little, peeped in, and instantly dashed away. Then I witnessed what Brehm has described, for monkey after monkey, with head raised high and turned on one side, could not resist taking a momentary peep into the upright bag, at the dreadful object lying quietly at the bottom.’<sup>5</sup>

This was written half a century before Köhler’s *Mentality of Apes* was translated into English—with a delay of eight years after the appearance of the German original.<sup>12</sup> It had the effect of something like a bombshell on American psychology, in which Pavlov and Watson were all the rage. Yet even Köhler, though he attacked Thorndike, remained essentially conservative as far as motivation is concerned; it took another quarter-century for a new crop of experimentalists to discover, in the 1950s, that the exploration of novelty, the manipulation of objects, the dismantling and reassembling of complex manual puzzles and even scribbling and drawing were self-rewarding and self-arousing activities with a positive feedback, as it were. ‘Those who have had opportunities to observe monkeys and apes at close hand for prolonged periods invariably dwell on their addiction to looking, mauling, prodding, licking, and generally squeezing every drop of possible entertainment from whatever crosses their path.’<sup>2</sup> Particularly revealing is the fact that Rhesus monkeys who have learned to dismantle a complex manual puzzle of interlocking pieces performed better when there was *no* food reward put inside the puzzle than when they knew that there was one.<sup>8</sup> In the second case they got impatient and tried shortcuts; in the first case they practised disinterestedly, *l’art pour l’art*.

## THE EXPLORATORY DRIVE

The cumulative evidence of these and similar experiments led Harlow to the conclusion:

‘There are logical reasons why a drive-reduction theory of learning, a theory which emphasises the role of internal, physiological-state motivation is entirely untenable as a motivational theory of learning.

‘The condition of strong drive is inimical to all but very limited aspects of learning—the learning of ways to reduce tension. The hungry child is a most uncurious child, but after he has eaten and become

thoroughly sated, his curiosity and all the learned responses associated with his curiosity take place.<sup>7</sup>

Montgomery came to similar conclusions, which he put into the laconic formula: 'exploratory behaviour is motivated by the exploratory drive.'

These clarion calls of a new generation of experimentalists in fact echoed the earlier 'voices in the wilderness'—such as Woodworth's: 'To see, to hear,—to see clearly, to hear distinctly—moment by moment, such concrete, immediate motives dominate the life of relation with the environment.'<sup>21</sup> In the meantime, however, these 'old-fashioned' views had received added, powerful support from neurophysiology. Lindsley, Hebb and others have shifted their attention from tension-reducing, stabilizing processes in the nervous system to the supposedly arousing, attention-sharpening functions of certain structures in the midbrain—the so-called 'reticular activating system', RAS. Although these theories are still controversial, parallel studies on sensory deprivation have dramatically revealed the deleterious effects of protracted stimulus-starvation, and the organism's need for more or less constant stimulation, or at least a steady inflow of information—a hunger for experience and thirst for excitation probably as basic as hunger and thirst themselves. Instead of responding passively to the environment, 'human beings and higher animals spend most of their time in a state of relatively high arousal and . . . expose themselves to arousing stimulus situations with great eagerness.' Two thousand years ago Juvenal had said much the same: '*Duas tantum res anxius optat, Panem et circenses.*'

Berlyne has made a systematic survey of the manifestations of the exploratory drive on various levels—from orientation reflexes to artistic and scientific curiosity. At the bottom of the ladder we have Pavlov's 'investigatory' or 'what is it?' reflex. 'It is this reflex,' Pavlov wrote in a famous passage, 'which brings about the immediate response in men and animals to the slightest changes in the world around them, so that they immediately orientate their appropriate receptor-organ in accordance with the perceptible quality in the agent bringing about the change, making full investigation of it.'<sup>16</sup> From true reflexes such as dilatation of the pupil and automatic scanning, we ascend to oculo-motor responses, movements of the head or the whole body towards the stimulating phenomenon: animals prick their ears, tense their muscles, sniff the air 'musingly'. Next comes 'locomotor exploration' which 'appears to be universal among higher vertebrates and

present to some degree in other branches of the animal kingdom'; yet, as Berlyne ruefully remarks, 'it has been studied systematically in rather few species. By far the greater part of the relevant literature is concerned with the rat.' According to Darchen, even the cockroach is capable of disinterested latent learning, prompted by sheer curiosity; while kittens, puppies and young chimps seem to spend a major portion of their time in 'locomotive exploration'.<sup>4</sup> Lastly, we come to 'investigatory' or 'inquisitive' behaviour, ranging from Darwin's monkey who cannot refrain from peeping into the snake-infested Pandora's box, to the 'unsatiable curiosity' of the artist and explorer.

Thus neuro-physiological considerations, laboratory work with animals, and the observations of ethologists of the Lorenz-Tinbergen school, all seem to converge in the same direction. Even the embryological studies of Coghill and Weiss, with their emphasis on spontaneous, intrinsic activities on all levels of the organic hierarchy, lend indirect support to the primacy of the exploratory drive. The lesson of fifty years of rats-in-mazes has been summed up by Thacker in the statement that 'motivation for learning is central and neural... organised and proliferated cognitive structure itself is the goal towards which learning moves'.<sup>19</sup> In other words, *the motivation for learning is to learn.*

Thorpe, for all his habitual caution, has gone even further. He starts with a rhetorical question: 'And so it becomes important to consider how far there is evidence of learning motivated by a general drive quite independent of the motivation of particular instincts'; and he concludes that 'there is now substantial and precise evidence for a general drive in a number of animals, and this can be looked upon as an indication of a primary motivation which to some extent, however slight, is superior to the governing centres of any of the instincts or of their combinations, and finds its most characteristic expression in exploratory behaviour in all its various forms.'<sup>20</sup>

In his monograph on 'The Nature of Explanation', which has inspired a great many neurologists and computer-theorists, the Cambridge psychologist, K. J. W. Craik put forward the idea that the function of the organism's nervous system is to set up a symbolic model of the external world: 'The brain imitates or models external processes. The function of such symbolisation is plain. If the organism carries a "small-scale model" of external reality and of its own possible actions within its head, it is able to try out various alternatives, conclude which is the best of them, react to future situations before they arise,

utilise the knowledge of past events in dealing with the present and future, and in every way to react in a much fuller, safer, and more competent manner to the emergencies which face it.<sup>3</sup>

To extract information from the chaotic environment is as vital to the organism as it is essential for it to extract specific forms of energy from sunlight and food. If we assume this to be an inherent tendency of all living organisms, then we must also assume the existence of an inherent primary drive to explore the environment for relevant information.

Thus the organism functions not merely by responding to the environment, but by asking it questions. The main incentives to its exploratory activities are novelty, surprise, conflict, uncertainty.\* The exploratory drive may combine with, or be instrumental to, other drives—sex, nutrition, anxiety. But in its purest form—in play, latent learning, unrewarded problem-solving—‘stimuli’ and ‘responses’ are indistinguishable parts of a positive feedback loop along which excitation is running in a circle like a kitten chasing its tail. ‘The scientist,’ wrote Allport, ‘by the very nature of his commitment, creates more and more questions, never fewer. Indeed the measure of our intellectual maturity, one philosopher suggests, is our capacity to feel less and less satisfied with our answers to better problems.’<sup>1</sup>

We have thus established a broader base for the scientist’s motivation. The exploratory drive may combine with the self-transcending mysticism of a Kepler or with the self-asserting vanity of a Galileo. Each original artist has an element of the explorer in him: the poet does not ‘manipulate words’ as Watson thought, he explores the emotive and descriptive potentialities of language; the painter is engaged, throughout his life, in learning to see.

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\* Uncertainty is more arousing than certainty—as witnessed by the universal passion for gambling which coincided with the consolidation of the British Welfare State. Its rudiments can be found even in the rat and pigeon—as Skinner himself pointed out—when rewards are given rarely and irregularly; this treatment induces the creature to go on trying for an astonishingly long time without a single reward—just as Britons will fill in week after week their football forms.

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Philip E. Vernon

## The Personality System\*

For more than thirty years the writer has been struggling to achieve a usable conception of personality—not so much a comprehensive psychological theory as a viewpoint or philosophy, which might serve the practical needs of the vocational, educational or clinical psychologist, or others who have to assess, understand and make practical decisions about children or adults. The applied psychologist receives little help from his training in general psychology. For though psychology has made some progress in isolating the principles of psychological growth and change, the acquisition of secondary drives, the importance of reinforcement schedules, etc. it does not attempt to say what are the chief enduring motives, the relatively permanent forces or intervening variables that underlie personality differences between one individual and another, nor does it indicate how these are to be diagnosed. More probably our applied psychologist's conceptions of human nature are derived from an uneasy mixture of psychometrics on the one hand and depth psychology on the other, both superimposed upon his naïve or common-sense understanding of people as motivated beings like himself.

### THE PSYCHOMETRIC APPROACH

The psychometric or mental measurement approach has been spectacularly useful in the intellectual sphere through conceiving abilities as a series of fairly stable, normally-distributed variables, which can be sampled by the application of properly constructed tests or examinations, and whose major dimensions can be determined by factorial analysis. Very naturally, therefore, the same conception has been

\* This article gives an outline of the main theoretical conclusions of *Personality Assessment—A Critical Survey*, a book written during the author's tenure of a Fellowship at the Center for Advanced Study in the Behavioral Sciences, Stanford, California.

applied in the wider area of personality by Burt and the present writer, Cattell, Eysenck and hundreds of others. People are thought to be characterized by their possession, in varying amounts, of such attributes as traits, attitudes, interests, drives. Any one variable can be assessed by ratings, or sampled and measured by tests, questionnaires, etc. and the enormous number of possible variables can be reduced to an ordered structure, as before, by factorial procedures.

In actual practice, this works only to a limited extent. Vocational and educational psychologists often obtain trait or interest ratings from teachers, acquaintances, or the client himself, or more rarely apply such questionnaire tests as the MMPI, the Maudsley Personality Inventory, or the 16 PF test; (most of the better validated instruments such as the Strong and Kuder Interest tests are suitable only for American high school and college students). Such scores provide some useful guidance on broad qualities, though they are also apt to be misleading because of the obvious susceptibility of ratings and questionnaires to halo, faking, response sets and other subjective distortions and unreliability. Both Eysenck and Cattell admit these deficiencies and aim to measure the main personality dimensions by more objective tests. But they have clearly failed to persuade practising psychologists in general to place any reliance on the instruments they offer. The behavioural tests which appear to help the psychologist more in building up a picture of his client's personality are performance tests of abilities and social situations of the Leaderless Group Discussion type. Both these yield qualitative observations of manner of reacting to difficulties or to people, not scores on specified traits or factors.

Even if better tests were available—and 40 years of personality studies have failed to produce them—the psychological counsellor or selector would be unlikely to be satisfied with the static, cross-sectional picture they yield. However detailed the profile, it does not amount to a person. True, Cattell has delineated tests and factorial techniques for studying dynamic trends in the individual personality, but they are quite impracticable for everyday assessment purposes.

### **'COMMON-SENSE' UNDERSTANDING**

How does the psychologist arrive at his notions of a person in daily life, or in an interview with a client—by observing his expressions and actions and questioning him on his past history, present attitudes and future goals and obtaining further information from acquaintances. Note that this is not a process of rational classification and inference;

the observed data do not give the personality direct. One interprets, or intuits the data as arising from various inner motives, traits or dispositions, which cannot themselves be observed. As shown elsewhere<sup>18</sup> a trait such as sociability, honesty, initiative, etc. is not a definite category of behaviour, comparable to the person's height or his arithmetical attainment. One's interpretations of others (or of oneself) are guided largely by theories of human nature, of what motives are responsible for what actions, theories which one has acquired from personal experience, from talking with and about people, from books, newspapers, films, etc. To a considerable extent these theories partake of stereotypes or oversimplified generalizations, though they nevertheless work well enough for us to get on with and understand one another for most purposes of everyday life. Normally there is a large amount of feedback. If one misinterprets a person on first meeting, one can often adjust one's notions of him in the light of further observations. However, the inadequacies and subjectivity of these judgements are well illustrated by the halo effect in ratings, the unreliability and lack of validity of interviews, which have repeatedly been demonstrated experimentally.

#### **DEPTH PSYCHOLOGY**

The trained clinical psychologist and psychiatrist employ essentially the same approach to the interpretation of the personalities of their patients, though their theories of motivation, based on Freudian or neo-Freudian doctrines, are considerably more sophisticated than the layman's. Their patients tend to display irrationalities of conduct and feeling which cannot be explained in terms of conscious sentiments and habit systems, but which seem to fall into line if we invoke unconscious complexes and fantasies, defence and compensatory mechanisms, built up in earlier childhood. These deeper forces, in their very nature, are even less observable than the layman's motivational constructs; hence they have to be elicited through such indirect methods as free association, projective techniques, transference analyses, etc. It should be realized, then, that Freudian and similar explanatory concepts are theories or fictions just as much as are the layman's stereotypes. They may be clinically valuable, and many of them have been taken over by modern learning theory with fruitful results. But a large amount of experimental evidence demonstrates that they do not yield greater accuracy of judgement. The clinical psychologist's, psychiatrist's or projective tester's predictions of, for example, vocational suitability, tend to be more unreliable and invalid than those of the 'normal'

psychological counsellor or selector who makes relatively little use of depth concepts, and who relies chiefly on relatively surface interviewing and situational tests.

### **INTERNAL DISPOSITIONS**

All these approaches that we have outlined make the questionable assumption that the person possesses a series of internal dispositions, whether we call them traits, factors, mechanisms, conscious or unconscious motives, which underlie his behaviour. Yet is it obvious that no one behaves in accordance with any trait with complete consistency. He varies greatly from time to time in accordance with *his* judgement of the current situation. Indeed these dispositions are as much categories or constructs which the observer imposes on, or reads into, the person as they are generalized behaviour tendencies of the person himself. As G. A. Kelly<sup>8</sup> suggests, the difficulties of personality study are largely due to our inveterate belief that dispositions are real entities in the person, waiting to be discovered, instead of admitting that we have to invent them. Recently, too, Ebel<sup>9</sup> has argued that the classical conception of validity breaks down in the personality field because the variables we are trying to assess are 'notional'; there are no operational criteria for evaluating our success in measuring a trait, attitude, drive, etc. However, this criticism is less serious in the case of an ability or an interest, which can be reasonably delimited in terms of environmental objects (musical, athletic, scientific, etc.). It applies much more to, say, leadership, honesty, extraversion, ego-strength, need for achievement, authoritarianism, etc., where any evidence for their 'existence' is more indirect and hypothetical. A clearer conceptualization of what is involved in studying such traits has been provided by Cronbach and Meehl's<sup>2</sup> account of construct validity. But this does not get over the difficulty that innumerable alternative constructs can be, and have been, proposed to account for the same observational or experimental data, depending on the theoretical bias of the observer or experimenter.

### **PHENOMENOLOGICAL AND PERSONAL CONSTRUCT APPROACHES**

Several attempts have been made recently to cut this Gordian knot of the nature of internal dispositions, by approaching personality in terms of the way the person perceives himself, other people and his environment. For Snygg and Combs<sup>11</sup> there is only one motive—the need for the person to maintain consistency within his pheno-

menal field, to avoid stresses and incongruities among his 'differentiations', i.e. in his self-concepts and his perceptions of reality. Similarly for G. A. Kelly,<sup>7</sup> the person's psychological processes arise wholly from his personal constructs or interpretations; and Harvey, Hunt and Schroder<sup>8</sup> have developed an elaborate developmental scheme or typology of concepts, which enables them to integrate a large amount of evidence from abnormal, social and developmental psychology, and from personality testing. Likewise the 'client-centred' approach to psychotherapy advocated by C. R. Rogers is opposed to explaining the client's condition in terms of Freudian or any other predetermined set of motivations. In the permissive atmosphere of non-directive counselling, the client brings up his own problems, is helped to clarify his feelings, and effects his own reintegration of his conflicting perceptions and goals.

This approach seems highly relevant to the work of the vocational or other practising psychologist, since he can better understand and advise an individual through knowing his attitudes and personal concepts—how he regards himself, people and events. At the same time it is inadequate, both because what the individual can tell us about himself quite obviously does not always explain his thoughts and actions, and because personality is surely not intrinsically 'private'. People can be observed and compared; common types of behaviour can be categorized even if we have no certainty as to the underlying causal entities.

#### **LEVELS OF SELF- AND OUTER-CONCEPTS**

Kelly and Harvey *et al.* have described a number of dimensions or categories of personal concepts. Much has been written also about the development of the Self-concept during childhood through such processes as identification and the acquisition of social roles. But it does not seem to have been sufficiently realized\* that such concepts may exist at a number of different levels or layers. Some people's images and theories of themselves are relatively superficial, others think in terms of deeper motives; (perhaps this is linked with the familiar extravert-introvert dichotomy). Our explanations of other people's motives and our social attitudes in general, i.e. our outer-directed concepts, may vary in a similar dimension. At least four main levels can usefully be distinguished.

\* A somewhat comparable view may be found in Leary, T., *Interpersonal Diagnosis of Personality*. New York: Ronald Press, 1957.

1. The first level consists of the impersonations and public attitudes which we display to acquaintances and strangers, when enacting our various social roles. We try to impress people with our importance and competence, friendliness and reliability, and to express moral, political or other attitudes which they will approve. Sometimes these Selves are carefully calculated, but often they are so habitual that we really believe in them. This level is well typified in the selection interview, where the candidate does his best to get across an acceptable personality, while the interviewer does his best to penetrate beneath the façade. It is also, of course, the level that is normally tapped by the psychological tester. A person's reactions to questionnaire items or to more objective, and even to projective, tests reflect the self-concept and attitudes that he regards as appropriate to the occasion; and this does not necessarily imply that they are wittingly faked.
2. Secondly there is the private Self that one normally recognizes, together with the privately-held attitudes, which one is willing to reveal, to a greater or lesser extent, to close friends or to a trusted counsellor or psychotherapist. Note that this may contain many conflicting parts—impulses that seem to operate outside one's control, and ideals or aspirations that are set apart from the rest (the Superego), though these are bound together by a sense of identity or Self-hood.
3. Thirdly there is a level which may be called the insightful Self, which one realizes when pressed to analyse one's attitudes more objectively. Sometimes a friend points out inconsistencies between one's feelings and one's behaviour, and we come to realize that we possess dispositions which are difficult to verbalize, or which were previously rationalized, and concealed from our own second level. This too would be the level reached during non-directive therapy; i.e. it is a theory of one's motives which the therapist helps to clarify but influences as little as possible.
4. Fourthly there is the picture of one's personality which is normally repressed, but which is reached as a result of depth therapy, whether Freudian or other, and through projective testing. Most clinical psychologists and psychiatrists would regard this level as involving the greatest degree of self-insight, i.e. as in some sense more 'true' than the other levels. But surely it is only one of many possible conceptualizations, and it will differ according to whether the diagnostician is a follower of Freud, Jung, Klein, Horney, etc. Admittedly the uppermost levels contain the largest proportions of demonstrably false rationalizations, but there is no scientific evidence that this level pos-

sesses great validity. On the contrary, we know that predictions of vocational suitability or recidivism based on depth theories are if anything less accurate than those based on more superficial study<sup>10</sup> and that there is no demonstrable superiority of more over less intensive types of psychotherapy.<sup>4</sup>

## THE PERSONALITY SYSTEM

We can see now why there is so much uncertainty regarding a person's inner dispositions, namely that they are always viewed or interpreted in terms of one of these personal construct systems, either by the person himself or by an outer observer. However, just as in ordinary visual perception, the degree of subjectivity varies. Perception of physical objects is relatively objective; there is a high consensus of opinion about the characteristics of a table, much less so about a landscape. Similarly different observers can agree about a person's physique and skin colour, or his tics. Even his abilities and interests can be assessed

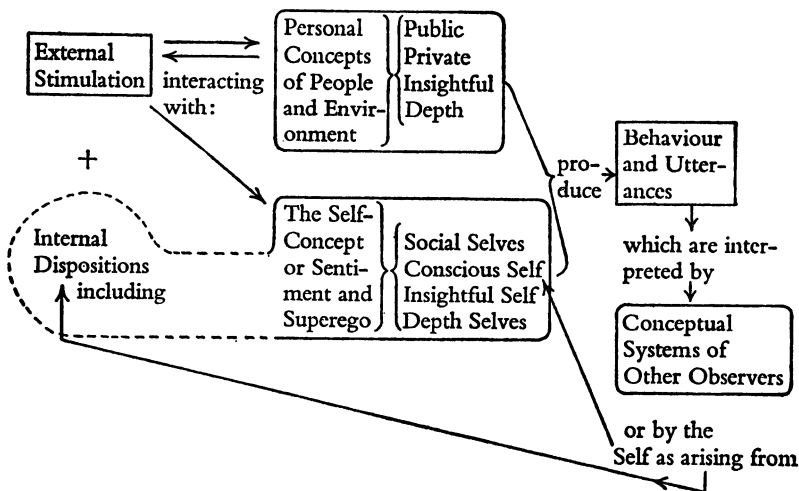


FIG. 1

fairly objectively, but not his underlying motives and personality traits. Thus we are led to conceive of personality according to the model portrayed roughly in Fig. 1, that is as a kind of chain of interactions and interpretations. This figure tries to suggest the more objective links by rectangles, the conceptual ones by rounded areas, and the hypothetical Internal Dispositions by a dotted circle.

The organism is activated both by these Internal Dispositions and by External Stimulation or the objective environment; but the former are 'processed' by the Self-concept, and the latter by the person's Outer Construct system before they issue in Behaviour. The person's Behaviour and Utterances are objective, but these in turn are processed by some level of the Self, or by the Construct systems of other observers, and regarded as emanating from Internal Dispositions.

### **IMPLICATIONS**

It follows that, to know a man's personality, we must study not only his behaviour and the situations that affect him, but also the people he reacts to and who interpret him, and his own conceptual system. Many writers as widely separated as Gordon Allport, R. B. Cattell and most depth psychologists, when they talk of personality, are referring only to the Inner Dispositions which, according to our scheme, cannot ever be diagnosed directly. At the same time the writer would disagree with those who try to dispense altogether with intervening variables—with the Skinnerian behaviourist who hopes to describe and explain personality wholly in terms of External Stimulation, Behaviour and Utterances, and with the phenomenologist who confines his attention to the Self- and Outer-construct systems.

### **THE 'UNITS' OF PERSONALITY**

The system presented here is very incompletely worked out; and it does not dispense with the need for units, elements or descriptive categories of some kind in terms of which a personality can be specified. No doubt traits will always be widely used, especially by the layman, though we would prefer to avoid them both because they take insufficient account of situational variations of behaviour, and because they perpetuate the notion that personalities consist of entities which vary in amount, and which can be rated or measured by suitable tests.

For the same reasons the statistician's factors seem unsatisfactory, although they have many uses in exploratory and experimental studies of personality. For example, they can help in classifying types of stimuli (abilities, interests, etc.), or behaviour clusters, or people's constructs (as in Osgood's and Kelly's work and Q-techniques). Again factor analysis can be applied nomothetically (for studying differences between individuals), or idiographically (for delineating patterns within the individual.)



The disadvantages of the depth psychologist's internal dispositions are obvious—that there is no consensus as to how they are to be defined or diagnosed, since they fall at our fourth level of constructs. It seems preferable, to the writer, to keep to a simpler and more superficial level, and to employ constructs which are as far as possible descriptive rather than interpretative. Thus one of the more successful personality assessment instruments is Stott's *Social Adjustment Guides*,<sup>12</sup> which ask the assessor to check a series of relatively easily observed behaviour tendencies or 'situation-attitudes' among disturbed children.\* These symptoms have been grouped by empirical trial and error, rather than by formal factor analysis, into a number of clusters of deviant reaction tendencies. While this method doubtless involves more halo and subjectivity than Stott believes, it might well be adapted to a variety of other purposes, such as vocational guidance of school-leavers.

### TELEONOMIC TRENDS

Finally we would draw attention to a proposal put forward by F. H. Allport in the 1930s, that people should be described in terms of their 'teleonomic trends', that is, the main purposes they seem to be trying to carry out.<sup>1</sup> These 'trends' resemble Gordon Allport's 'unique traits' or 'personal dispositions' in that they are defined more by consistencies in the individual's behaviour than by similarities between individuals. Nevertheless it seems likely that many trends are sufficiently widespread among members of a cultural group to be compared and assessed nomothetically, while others are more unique to the individual. They differ from traits in that they tend to involve interactions with people or situations, i.e. they are not located so completely *in* the person. Again they can exist at conscious or unconscious levels; thus they are not necessarily accessible to the second level of the Self, but may have to be inferred from observations of behaviour, or by attempting to get down to the third level.

The fullest description is that given by Gregory,<sup>5</sup> who classifies them under seven headings. Some examples are:

- to escape attention from others,
- to be law-abiding,
- to prove oneself superior,
- to secure aesthetic pleasures, etc.

\* Stott's criticisms of the psychometric and clinical approaches to personality, and of 'trait theory', are also worth noting.

However, at the moment there is no satisfactory taxonomy and no clear specification of common trends to look for; also very little guidance is given on how to identify them, other than by interview and observation. Nor do we know how far different psychologists studying the same person would end up with the same list or pattern of trends—though there might well be better agreement than is generally found among clinical diagnoses based on depth-constructs. A combination of the trend approach with Stott's notion of behaviour clusters appears to offer the practising psychologist the kind of framework for viewing personalities that he needs, though a good deal more work will be needed to build it into a usable system.

In conclusion: there is, of course, no simple answer to the problem of how to assess personalities for the purposes of reaching practical decisions. This paper merely attempts to bring out some of the reasons for the failure both of the conventional psychometric and clinical approaches, and to indicate some lines for future development.

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T. H. Pear

## Social Ascent in England Today

Distinctions between the respective fields and methods of sociology and social psychology have been discussed for years. In practice, studies in these disciplines often overlap, yet sometimes they seem completely separate, as if a compulsion for tidy patterns had wrenched them apart. Sociological articles often appear with no reference to any living subject who might comment on the soundness and fairness of the writer's judgements. To such criticism a sociologist might reply that he leaves the study of individuals to the psychologist, perhaps resisting a temptation to add that this task is often shirked. Were the sociologist to remind us that physiologists usually delegate the study of intact bodies to the clinician, the analogy would be unconvincing, since a psychologist may be interested in healthy persons, with no aim of curing or even reforming them: indeed, the view is often expressed that researches promoted by the Medical Research Council ought to be supplemented by investigations encouraged by a Social Sciences Research Council, interested in matters unconcerned with obvious diseases or disabilities.

In many psychological books about personality, idiographic accounts of persons are avoided, presumably from a wish to present a detached, impersonal treatment. Such an account could be given of music by a physicist, but it would disappoint the musician. Novelists and playwrights today are especially interested in *personas*, and novels about the English upper, middle and working classes afford valuable glimpses into the inner life of the authors. It is not, however, widely recognized that quite a number of people who have risen in the social scale are willing to describe and discuss in confidence their experience and behaviour. It is important here to emphasize the word 'experience', less popular with psychologists than it used to be, perhaps because it has embarrassing implications for some workers in the 'behavioural sciences'. The modifications of behaviour due to the conscious exercise of skills, muscular and social (they cannot always be sharply distinguished), are more

important today than ever before, and accounts of their successful use are necessary for social psychology. To describe such experiences to a confidant is often less awkward than to confess to a psychiatrist.

Since the communicator is unlikely to be psychologically trained, some degree of agreement concerning relevant concepts must be achieved between speaker and listener. A few of these will now be described in terms which are, I hope, reasonably simple. I shall not, however, include among them the concept of 'socio-economic status', borrowed by some British investigators from the U.S.A., where it is more useful. Its use, I suggest, may blunt the edges of observations of social ascent in England, where aversion to admitting the existence of social class is not widespread. Perhaps many of those who apply the notion of socio-economic status in England live in towns, regarding the country as a holiday resort. It is true that most English people do live in towns, and that the percentage is increasing, but in the country class distinctions are commoner and more obvious. Yet here matters are far from simple for, owing to increased mass communication and travel, perhaps daily, class distinctions become confused with those due to educational background and knowledge of literature, art and music. The mixing of classes in a lively village (there are many stodgy ones) helps to blur all these distinctions, though in a group acting successfully for a specific purpose, e.g. fund-raising, there are usually class-conscious members.

The three words comprising the term 'socio-economic status' may blunt each other when brought into contact. The idea may be useful in studying a really new town like Scunthorpe in Lincolnshire, where the inhabitants, concerned chiefly with the steel industry, are prone to class each other in terms of known income, possessions, furniture, locality, house, street and the school attended by their children. Income would not be so closely connected with class in Durham, Canterbury, Cheltenham or North Oxford; here ascribed status, intellectual as well as social, may be high in a person who, judged by modern standards, is poor absolutely as well as relatively. Today in many English villages a person may ascend through several social strata without rising far in the economic, intellectual, cultural or sporting scales—'sport' being defined here in the usual ambiguous ways. In H. E. Bates' uproarious novel *The Darling Buds of May*, the Larkins, a family of unashamed 'new rich', are an outrageous caricature, yet a caricature usually has something to bite on, and no Larkin tried to achieve membership of a hunt. If Mr. Larkin remains rich for twenty years, his

children may and his grand-children probably will rise socially and marry 'above their class'. But the parents of some country-school children resist teachers' strongest urgings in the direction of further education: as a social psychologist puts it, keeping down with the Smiths may be more attractive than keeping up with the Jones.

What is the nature of social ascent today? Some observers boldly assert that our middle class is growing so rapidly towards the top as well as the bottom, that before long almost everyone will belong to it: to borrow Professor T. H. Marshall's simile, English society tends to resemble a bungalow rather than a skyscraper. Some political observers suggest that the present policy of the Labour Party aims at this. The facts, as seen at least by the present observer, whose adult life has been spent in the industrial North, London and south-east Kent, do not justify such a simple image.

If the customary concept of a social ladder is used, it should be remembered that today there are several. Between their summits may yawn gaps across which it is difficult, often impossible, to leap, and an aspiring climber would be well advised to choose the right one for his purpose. For financial tycoons and appliers of science there are escalators.

It is not easy to decide upon the meaning of the popular phrase 'Top People', applied to persons who have experienced life at or near a summit. A hereditary Lord, a President of the Royal Society, an O.M., a chairman of a huge industrial combine, an owner of a string of horses or newspapers, a majority holder in an Independent Television venture, a knighted athlete or celebrity in the amusement world, may occupy different top places, but may seldom or never communicate with each other.

The special uses of the interview should be obvious in this connection. Success stories in books, newspapers and on the air seldom use psychological terms accurately, and it is often hard to avoid the suspicion that the account has been 'ghosted'. Radio and television interviews often provide information more useful to a psychologist, though interviewers who 'needle' their victims occasionally find that the victim uses the defensive technique of turning aside a thrust by apparently innocent counter-questions phrased modestly.

If read with empathy and discrimination—and this is not always easy—current plays and novels tell us much about obstacles to social ascent. The title 'Angry Young Men', bestowed by the Press, was often misleading, though some writers took their time in disclaiming it, and

some severe critics of the Welfare State omit to mention that without it they might not have had the university education which trained them to write clearly and attractively.

### CONCEPTS

In discussing personal data with an interviewee the following concepts, illustrated by examples, can be useful:

(a) *Rank*. This is generally recognized, objectively symbolized and unambiguously named, e.g. in books on titles and modes of address.

(b) *Status* is often expressed verbally, e.g. Lord Lieutenant, Mayor, President, Chairman, Police Constable. Interestingly enough, in addressing a person, recognition of his status is more usual in Germany and America—their customs are probably connected historically—than in England. Here, the director of several firms is not addressed as 'Director'. Status, of course, is often temporary, and the degree of respect for its holder may depend on estimates of his adequacy in fulfilling the function which gives him his title. In a business firm, status symbols are often very important: the furnishings in the status-holder's private room, the carpet and its value, his car, its type and chauffeur, if any; the restaurant or canteen he frequents, the lavatory he uses, and perhaps the name by which it is known—all these symbols are respected inside the firm. The type of hat worn may be symbolic: bowlers may be socially graded.

That in America status symbols are more important and numerous than in England is suggested in Vance Packard's *The Status Seekers*. Some of his references to status are to distinctions which in England would be 'class'. L. Reissman writes, in *Class in American Society*, that 'status' sounds less harsh, less final and less materialistic than 'class'. The last of these adjectives may seem to us English to be queerly chosen: some persons whose share of materials is small are class-conscious. Though in conversation the word 'class' is often avoided here, between equals there are periphrases, mutually understood; 'one of us'; 'not quite our sort'; 'good', 'poor' or 'dim' type.

(c) *Stratum*: financial, educational, cultural, 'sports'. Such social layers are often objectively perceptible: a person, and his acquaintances, know that he belongs or does not to a certain layer. Income Tax assessors assign him to a particular economic stratum, though a 'spiv' has been defined as a person who makes money in ways which his Government has not yet made illegal. Here the ambiguities of income arising from the allowance of generous expenses, and the pre-

valance in certain games of 'shamateurism', make objective estimates misleading.

(d) *Elites*: social, political, scientific, military, etc. They may be overt, covert or semi-covert. Sub-élites develop, e.g. 'Pop' at Eton, and power-cliques inside large colleges, which in an old university may be socially stratified. Though the phrase 'The Establishment' is often scorned, it is commonly used, even by American observers of the English scene, and different Establishments are being hinted at.

In discussions with social ascenders of the obstacles they met and the ways in which they overcame or by-passed them, the classifications mentioned below may be useful. They overlap at times, and are not put here in order of estimated importance.

#### PERSONAL APPEARANCE

(a) *Physical*. 'Good looks', and in women a 'good figure', have always conduced to social ascent, yet examination of fashions in the last century shows that criteria of 'looks' and 'figure' have varied considerably. Nowadays commercial interests, through the advertising media, ensure that criteria will vary every year. Cosmetic facial surgery is now popular. Styles of wearing hair, on the face and cranium, are class-implicated: woe betide a man presenting himself with a 'wrong' haircut at an important interview! Whether a woman's hair-style is 'correct' may depend on the whims of highly-placed people. Condition of teeth and hands (cared-for or neglected) is widely perceived as a symbol of financial stratum rather than class. The plethora of women's magazines ensures that everyone who can read can learn details of hand culture. When National Health charges were imposed, dentists soon reported increasing neglect of their teeth by young people.

(b) *Clothes*. The multiple shops make it easy for women to buy fashionable clothes, and for obvious reasons criteria are continually altered, and discussed in the newspapers by specialist writers. A rich male social climber can be guided by a 'good' tailor, who will ensure that correct, and not extreme, trends are followed. Though dress-hire firms are a subject of jokes, some made by the firms themselves, people in very different classes hire suits for special occasions, e.g. a village wedding (there are agents even in small towns), or a Coronation ceremony. Despite periodic attempts to influence the formalization of men's clothes, there is a steady trend towards individual choice, not only in holiday wear. To hear opera at Glyndebourne a dinner jacket must be donned in the early afternoon in London: on

most nights at Covent Garden almost anything goes. It is said that now medal ribbons are permissible on a dinner jacket, evening-dress 'tails' may almost disappear.

#### SPEECH AND MANNERS

These overlap, especially when accompanied by appropriate facial expression and gestures, often slight. In some English people, deliberate suppression of all gesture at certain times is itself a gesture, but the pressure brought by television producers on certain public speakers has had odd effects. In Northern England a bright smile may accompany sentences which to a stranger can sound gruff, even unfriendly. That good manners take longer to acquire than good clothes is underlined when, in *Pygmalion*, Professor Higgins orders his housekeeper to burn Eliza Doolittle's garments and 'ring up Whiteley or somebody' for new ones. Many schools, colleges and some evening classes set out to improve a social aspirant's manners, and several good books can be consulted. Recently speech and manners have become democratized in certain respects, but many 'teenagers' have developed an exclusive brand of both.

'Manners' refer particularly to modes of address: to superiors, inferiors, seniors, juniors, friends, acquaintances, the opposite sex, hosts and guests. To this list some observers of society add 'horses and dogs'. In some parts of England the use of special phrases for these friends of man are class-signs.

Though we sometimes hear, 'We are all middle-class now', this belief would be questioned if one were to consider:

#### AMUSEMENTS AND SPORTS

Social ascent can be facilitated by knowing which are the 'right' ones today, as compared with even a generation ago. Addicts of whist drives and bingo sessions seldom attempt to learn the more complicated varieties of bridge. In at least one provincial city lessons are being given in roulette to members of new 'casino clubs'. The betting shop, legally permissible, is democratizing the methods of backing horses, and may put the low-class street corner runner out of business, but many bets will still be negotiated as before over the telephone, by persons who have an account with their agent. A new game, ten-pin bowling, which owes part of its immediate success to the fading popularity of the cinema, is unlikely to attract squash players. Whippet racers do not play, or watch, polo. Public 'all-in' wrestling is not for the



upper-middles. Professional jockeys compete in flat races, but not in public horse-jumping competitions; in these, money prizes are won by people who may declare publicly that they depend on them for the upkeep of their horses. To give a riding lesson for pay would make them professionals. There have always been grumbles when a professional cricketer has been suggested for the captaincy of a Test team, but the recent abolition of any distinction between cricket amateurs and professionals ('there are no longer any gentlemen in cricket', commented a right-wing weekly) may bring about several related reforms, e.g. certain newspapers will have to decide whether to concede the professional his initials or take away the amateur's.

Tuning in to a football commentary, it is usually possible after hearing a dozen words to decide if the game is 'soccer' or Rugby Union. In England there is a social gap between this code and that presided over by the Rugby League. On the air, soccer players are called 'Tommy', 'Billy', 'Steve' (this applies also to professional boxers) but affectionate Christian names are heard less often in radio estimations of success at Twickenham. (Why do English people in particular laugh at the story of the foreigner who enquired 'Please, am I in the queue for the Sir Thomas Steele concert?') It is socially helpful to be good at cricket, golf, 'rugger', ('soccer' occasionally), squash, riding (English people who say 'horse-riding' incur playful gibes from John Betjeman), shooting (there are obvious financial gradings, though large-scale pheasant shooting run by syndicates is said to pay its way), hunting (stags and foxes only), sailing and ski-running. Proficiency at lawn tennis, swimming, dancing (not too well), all in the right milieus, can help the ascender.

Before the Second World War, ability to skate difficult figures on ice, especially in the so-called 'English' style, was a sign of financial stratum, often of class. In London, Princes' Ice Rink provided, for those with money and leisure, lessons and opportunities to achieve results which could be polished in Switzerland, where resorts were financially and sometimes socially, graded. Today numerous well-equipped ice-rinks in London and the provinces offer expert instruction at reasonable charges for many hours every day. Skates and boots can be hired: cheap refreshments are obtainable. To swim stylishly and strongly was usually possible only to the rich and those who happened to live near a river, but democratization has set in here, even more obviously than in the case of skating.

To enjoy many leisure activities involves tipping, sometimes several

people on one occasion. It is useful to know with certainty whom, when, where, how much, even how to tip. Answers to these questions are now obtainable in easily accessible books, and probably fewer people are too shy to seek such information now that foreign travel is popular with all classes and tipping problems are universal. Protests against all forms of tipping are constantly heard: a thinly-veiled reason for its retention is that it makes it possible to buy extra, perhaps unfair, attention, a fact that was very obvious to travellers in this country in war-time.

Tipping is a class divider which cannot be ignored: a newly-arrived visitor from Australia once remarked that no willing recipient of a tip in public could consider himself of the same class as the tipper. Many people are faced with the question, 'A tip, a present, or nothing more than suitably phrased thanks?' Young people worry less about tipping than their elders did at the same age, since money is more widely spread nowadays: perhaps hotel and restaurant staffs know that at lunch-time the chief contributor of large tips was, until recently, likely to be the 'expense account' customer.

To the social ascender, a hindrance may be the adhesiveness of his present occupation. The relative unpopularity of school teaching in small communities is partly due to the fact that social responsibilities—not always fairly expected—can be declined, out of school hours, only with difficulty and perhaps embarrassment. Yet social expectations even in the same rural district may vary: in one village, parents of pupils rarely consult teachers; in another, visits from parents are frequent, and some, from the teacher's standpoint, unnecessary. A village schoolmistress may travel daily in privacy in her car or on her motor-cycle, to a town where she can live her own life: had she stayed in the village, her social contacts might have been restricted, but high on the list of gossip-priorities. Or she may feel herself socially marginal, never a pleasant experience. Today, certain well-paid occupations are hard to assess on any social scale if their label only is known: examples are 'in advertising', 'journalist', 'photographer', 'model', 'secretary'.

Obvious defects of the interview, if its results are the only ones considered, are that the subject's 'confessions' are filtered to a certain extent as a result of personal censorship, and that the reporter may consider as particularly important a certain class of facts, neglecting a score of others. In this he may be guided, more than he realizes, by his own style of life. Some sociologists, for example, have been criti-

cized as being 'mainly Marx-orientated'; some may look for sexual interpretations whenever a problem presents itself, and find them.

This account may appear unduly influenced by my observations and those of my friends—a one-man view. But studies based on averages of large numbers blur important individual differences and may ignore extreme cases. In our life-time important social changes have been made by social ascenders whose struggles have supplied sources of personal strength—and weakness.



*J. P. Guilford*

## **Implications of Research on Creativity\***

My purpose in this paper is to glance backwards as well as towards the present scene, in order to gain a general view of our present understanding of human creativity. From this beginning I shall attempt to make a number of extrapolations into the future, with special attention to what effects knowledge about human creativeness will have upon our mutual well-being. Such stocktaking is often salutary. Those of us who are closest to research on this subject and to its applications often fail to grasp the larger picture. Nevertheless, as members of society and as citizens, it behoves us to be concerned about the social outcomes of what we are doing, and we have a responsibility, from our own particular standpoint, to express views on this subject.

I shall begin with some general propositions about the nature of creativity and the relations of outstandingly creative individuals to their society. I shall then point out some possible effects on various aspects of everyday life in a number of fields, including education, art, the economy, the government and society in general.

### **GENERAL PROPOSITIONS**

My general propositions have to do with the questions where and how we find creativity, its relation to intelligence, and the general reaction of society to outstandingly creative persons. My first proposition is that all have some degree of potential for creativity, although in the past, it was recognized in only a small fraction of the population and was regarded as a divine gift. Even after Darwin laid the basis for the belief that the gift of creativity is inherited, the creative genius was for

\* Based on a paper presented at the symposium on 'Implications of Creativity Research', sponsored by Los Angeles State College and the Chouinard Art Institute, and held at the Huntington-Sheraton Hotel in Pasadena, California, on March 10th, 1962. Such a topic seems very appropriate in a volume in honour of Professor Cyril Burt, who has made so many creative contributions to psychological and educational measurement.

a long time treated as a kind of genetic sport or rare exception. There was an attitude of inevitability and fatefulness towards creativity, which offered little hope either of understanding it or of being able to do anything about it.

More recently psychological thinking has changed all this. Since Galton, himself a genius, initiated the first study of individual differences eighty years ago, more and more psychologists have accepted the axiom that most human traits exist in varying degrees in the population. The genius simply stands out because he is near the top of the creativity scale. Anyone is prepared to recognize that even among geniuses there are differences of degree; there are greater geniuses and lesser geniuses. And the variation does not stop there; it extends down through the population, most of whom are inconspicuous in this respect. The adoption of this axiom of broad variation in creativity has, more than anything else, made possible present-day scientific investigations into the nature of creativity. No longer does one need to find the genius for study in one's investigation of the subject.

My second proposition is that creativity is not a uniform or unitary commodity. Let us not be deceived by the fact that 'creativity' is a single term. Let us not make the semantic error that one noun necessarily stands for one thing. No two people are creative in exactly the same way. In addition to quantitative differences there are also qualitative differences. Analysis shows that there is no one creative ability; there are, instead, quite a number of different abilities that contribute to creative performance. It is also the rule rather than the exception that each creative person tends to stand high in some abilities and lower in others. The person uniformly high in all, like Leonardo da Vinci, is the rare exception.

A third axiom is that creativity is an aspect of intelligence, not something apart from intelligence. Research findings over the past 25 years indicate that the conventional conception of intelligence is extremely narrow. The question is often asked, 'What is the relation of creativity to intelligence?' The answer is that if 'intelligence' is conceived as broadly as it should be, it logically includes creativity. This does not downgrade either creativity or intelligence; it upgrades them both. Those who ask, 'What is the relation of creativity to intelligence?' usually have an ingrained concept that intelligence is what is measured by conventional intelligence tests. Such tests assess a very limited number of components out of more than a hundred probable components of intelligence, if intelligence is very broadly conceived.

I should at this stage make a distinction between creativity and creative productivity. From my somewhat circumscribed point of view, as a psychologist concerned with a basic science, creativity means potential for creativeness. Whether or not a creative person is creatively productive depends upon a number of other circumstances, including his special motivations and his environmental stimulation and opportunity. The Aptitudes Research Project carried out at the University of Southern California has concentrated on aptitudes or abilities entering into creative thinking, on the assumption that this approach will yield the most intimate information concerning creative potential as well as the nature of creative thinking itself. We also believe that on the basis of such information we are well prepared to do something about creativity in individuals. Other contributors to creative potential and creative productivity are general motivational traits of needs, interests and aptitudes. These qualities can and are being investigated from the standpoint of individual differences, as well as by other approaches.

It is well known, of course, that society tends to damp down creative production. This is the last major proposition in my list. The creatively productive person is generally envied, misunderstood and mistrusted. He is upsetting to comfortable ways of thinking and doing. Sanctions of various kinds are applied to him whenever he becomes too much of a threat. Although our society no longer literally cuts off the heads of those who think too differently, there are still some extremists among us who would apparently do so if they could, and there are modern societies that do, in fact, treat deviants in such a manner.

#### **PRESENT STATUS OF KNOWLEDGE**

Most of what we know about the nature of creativity and creative productivity has been learned during the past dozen years. New interests in the subject sprang up spontaneously from several directions almost exactly in the middle of the century. These interests have been varied, so too, have been the methods of investigation. Industries had been clamouring for more creative scientists and engineers. The cold war generated needs for improved weaponry of defence at an accelerated pace. Entry into the space age stirred imaginations. There was increased leisure time and there was the do-it-yourself movement. Sputnik number one focused attention upon our faltering educational system. In America, not the least of the stimulating conditions has been a new surge of interest in the arts.

## APPROACHES TO INVESTIGATION OF CREATIVITY

In order to appreciate the effects of researches into creativity on our future, it is necessary to give them at least passing attention. The broad areas of investigation include the psychometric approach, which aims to determine the basic qualities—abilities and other traits—that all of us have to some degree and that contribute uniquely to creative performance. This method has been followed almost exclusively by the Aptitudes Project at the University of Southern California. Another approach, also concerned with individual differences, has concentrated on the differences between qualities of recognized, creative groups, such as top architects, writers and mathematicians, and qualities of persons not so recognized. This is the method chosen by the Institute for Personality Assessment and Research at the University of California at Berkeley, under the direction of Donald W. MacKinnon.

Other groups have been concerned with the prediction of creative output—in the form of science, for example, as in the investigations at the University of Utah, under the direction of Calvin W. Taylor. The information from which such predictions are attempted includes a great variety of trait assessments, biographical data and working conditions. Still other groups are concerned with methods of training children and adults in more creative ways and the evaluation of training methods. Two centres of such investigations are to be found—one at the University of Minnesota, under the direction of E. Paul Torrance, the other at the University of Buffalo, with its Institute of Creative Education. This Institute has become a kind of national clearing-house for information on teaching for improved creativity.

## SOME BASIC FINDINGS

From the psychometric approach we have learned a great deal about the nature of creative thinking and problem solving. Several categories of abilities play direct roles in creative operations of various kinds. There are a number which have to do with fluency of thinking. By fluency we mean the facility with which ideas are called out of memory storage for use in new situations. Psychologists who are psychoanalytically inclined point out that the creative thinker is in close contact with his unconscious. I am not sure what this means, but I think it means much the same as does my definition of fluency of thinking; the ease with which we use stored information when we need it.



Another category of creative-thinking abilities is known as flexibility. Flexible thinkers are relatively free from dominance by fixed ideas and habitual ways of solving problems; in fact, they are free from rigidity in thinking. Originality falls in this category. The flexible person is able to strike out in new and unexpected directions in order to achieve novel results. A third category of creative thinking is called elaboration. This means that, given certain information the thinker can produce one addition after another, even to minute details, and arrive at an elaborate, finished product.

These three kinds of ability—fluency, flexibility, and elaboration—belong logically in a larger category known as ‘divergent production’. The ‘production’ aspect of this term means that the person produces new information from given information. The ‘divergent’ aspect means that in producing new information, the thinker goes off in different directions. The answers are not fully determined; there is no one ‘right’ answer, as in a contrasting category of thinking known as ‘convergent production’.

When I say that there are categories of fluency, flexibility and elaboration, I mean several things. First, each of the three applies to different kinds of information, whether that information is concrete or perceived, whether it is abstract, in the form of symbols and concepts, or whether it is involved in inter-personal relations. Second, different kinds of outcomes are involved. Flexibility may be of the kind that involves jumping from one class of ideas to another, or of the kind that involves habit breaking, change of strategy or attack, or a revision or transformation of the way in which things look to us. Fluency depends upon the kind of idea involved, whether it is simple, as in the case of a single word, or complex, as in the case of a sentence, or whether it involves relations. All these variations demand that different and separate kinds of abilities be fluent and flexible. Such findings provide a basis for the proposition that creativity is not a unitary thing. The implications for education, particularly, are most significant.

Studies of distinguished creative architects, writers and mathematicians, and of the individual differences of the general run of people reveal certain other qualities that go with creativity. The creative individual is a person with an inquiring nature and broad interests. He observes and he remembers. He is likely to have above-average aesthetic and intellectual interests, and little regard for routine types of activity. If a male, he generally has more feminine interests and attitudes than the average man, but he is not necessarily effeminate

in appearance or behaviour. He is an independent thinker, holding to his own set of values. He is confident, self-assured and even self-assertive, yet has below-average social interests. Although traits such as these stand out among mature individuals who have achieved places of recognition in their professions, we cannot say that they distinguish creative people in all professions, nor can we say that all of them are apparent in the young, pre-professional, creative individual.

In studies of research and development personnel at work, either in industry or in the armed services, much has been learned about the effects of management, supervision, psychological climate and working conditions upon creative output. Programmes for training in creative thinking have been developed and applied, and stock procedures or strategies for attacking different kinds of problems have been tried out and taught. There are many inter-personal problems in fitting the independent-thinking scientist or engineer into an organization and in motivating him to achieve maximum productivity. Such investigations are now in progress. Their chief relevance here is to help illustrate the great complexity of the problem of promoting creative productivity, even after we have assembled people with sufficient creative potential.

## **FUTURE IMPLICATIONS**

### **EDUCATION**

The most direct implications and applications of our knowledge of creative dispositions will be found in education, from the pre-school through the graduate level. Since many aspects of creativity belong in the general area of intelligence, the applications are of one piece with those arising from the components of intelligence in general.

The analytical study of intelligence leads to the general conclusion that from the standpoint of scientific psychology, the individual is an agent that deals with information in many ways. The nature of any one component ability depends upon the kind of operation applied, the kind of information involved, and what is made of that information. About 60 components of intelligence are known and at least as many more are predicted from a theory regarding the structure of intellect that I have put forward elsewhere.

A major objective for education suggested by the theory is that of training the intellect. Education as a means of training the intellect is really a very old-fashioned idea, but new information about the

nature of the components of intellect gives this objective a far richer and more definite meaning.

The first important deduction is that there are *generalised* intellectual skills as well as specific skills. The learning theory of most psychologists during the past 30 years, based largely on the knowledge of how lower animals learn, has emphasized specific skills to the neglect of the more general. Each component of intellect may be regarded as a general skill for dealing with a certain kind of information in a certain way. The nature of the skill suggests the kind of exercise needed to promote its growth. This implies the application of sets of exercises, each tailored to develop a unique kind of intellectual skill that has general use and significance.

A catalogue of the different kinds of skills would be a rich source of information on which to base curriculum development. There should be an attempt to see that a child has opportunity to exercise all the general skills. He can even be made aware of the nature of each, and having tried himself out in all of them he can learn in what direction he is likely to develop most rapidly and where his greatest interests lie. He can be expected to develop minimal levels in all skills that have personal and social value, whether they have to do with concrete things, with symbols, ideas or people. And he can be encouraged to achieve much more than minimal status in skills for which he has special aptitude and interest.

While on this subject, I should like to point out that there is much misconception regarding the 'gifted' child. The current definition in California (by state law) is the child with an intelligence quotient of 130 or higher. The intelligence quotient is a fairly good index of whether or not a child will obtain good grades in academic subjects, but so far as general intellectual assessment is concerned, it is seriously limited. There are numerous ways of being gifted. Our present method of selecting, on the basis of intelligence and grades, favours the more docile, compliant, conforming grade-getter and may miss some potentially creative geniuses or near-geniuses.

The components of intelligence that appear to contribute more directly to creative performance may be regarded as generalized skills, subject to improvement with practice. There is plenty of evidence that scores in tests of the components of fluency and flexibility can be raised as a consequence of taking a course on creative thinking. The results are not such as to make us expect miracles, but there is observable improvement: students come away from such courses with more self-confidence and greater skill.

Exercises have already been designed for courses on creative thinking for both children and adults. There would probably be greater improvement and wider applicability of the learned skills if such exercises were developed in connection with ordinary courses.

Teachers of art often point out the advantages of their subject for promoting creativity in children. Certainly the opportunities are there, but they depend upon the attitude of the teacher and the way in which he teaches. In some art courses in American public schools, when Thanksgiving comes, all children draw and paint turkeys—some particular, standard turkey, perhaps. The teacher may tell the children exactly how it is to be done. Woe to the child whose turkey deviates. This is no way to develop initiative and creative habits. Is it any wonder that some children develop the habit of asking, 'Teacher, what do you want me to do next?'

Probably no course or subject is without opportunities to teach creatively. Any course should be more exciting, more meaningful and inspiring, with a teacher who shows imagination. There should also be encouragement of self-initiated activity on the part of the learner, with rewards for unique as well as conforming achievements.

Among the changes that we should expect from a creative approach to teaching and learning would be not only increased development of creative skills, along with intellectual skills in general, but also changed attitudes and a changed climate, in which new values replace old. There should be increased tolerance for creative effort and increased respect for independent thinking. The learners will learn to question answers as well as to answer questions. Learning should become challenging and exciting and the learner should show more intellectual initiative.

The complete creative performance, of course, includes evaluation of the results, hence children should be taught to apply standards and to show good judgement concerning creative products. But they should learn to apply critical thinking after the production rather than before.

The notion that education should develop general intellectual skills, including those contributing more obviously to creative production, implies the need to assess the growth in the various components of intelligence at appropriate intervals. Such information should be separate from that derived from assessment of more specific knowledge and specific skills. The acquisition of special knowledge should still be an important aim of education, but not an end in itself.

The importance to creative production of a good store of informa-

tion was implied earlier in this paper, but much more important is accessibility to that information. And information should be obtained by the student in ways that are favourable to learning. We remember best and with greatest potential usefulness those things that we discover for ourselves and that have greatest meaning and significance. The active young child is thrilled by his discoveries. We should encourage the learner to seek information actively, not to be a passive receiver of information that is fed to him. Information passively obtained is not likely to be functional.

#### THE ARTS

Artists are not all exceptionally creative just because they are artists. They differ very much among themselves in their notions of what constitutes creativity in artists or in art. There are at the one extreme craftsmen and at the other innovators, while others combine both kinds of talent.

Speaking of artists, I have in mind the many kinds of art—painting, writing, music, architecture, drama and their various sub-groups and relatives. Because of certain parallels, and in spite of different skills needed in the different arts, we can speak of the general ways in which artists can be creative. It is agreed that in his productions the artist is expressing himself, which means he has a message or at least a theme. His theme is one point at which he can show exceptional creativity. Other points at which novelty can be shown include his art form and his materials, his technique, his style and the manner in which he succeeds in getting his message across.

What is likely to be the effect upon the arts of the general surge of interest in creativity? Those of us with cultural, and especially with aesthetic interests, will hope and expect that it will be generally beneficial. Compared with members of other cultures, not only older European but some of the more primitive human cultures of today, the average citizen of the United States has shown in general an unusual indifference, even disdain for art. Perhaps this is partly a tradition coming down to us from the Puritans, the Quakers and other groups who forswore beauty in favour of plainness. Perhaps some of it arises from the habits of the pioneers, who were too busy conquering a continent and developing its resources to have time for cultivation of the arts, beyond the minimum simple forms that they enjoyed. Perhaps our efforts to make sure our young boys are masculine and the fact that art has appealed more to the feminine disposition, have had

something to do with our general reserve with respect to art. To a large extent, too, the arts, like sport, have been treated as a spectator's pleasure. Active participation has been left to those with exceptional skills.

However this may be, we do not find a very large proportion of the American population engaging in the production of art objects, such as we find, for example, among American Indians, among Mexicans or in Japan. Anyone who has achieved a creative object, in art or elsewhere, knows of the genuine satisfaction that such an achievement can bring. Too many of our growing citizens have been denied that kind of satisfaction. A higher value placed on creative expression and a greater tolerance for the range of activity attempted by a child, boy or girl, may be all that is needed to yield a new crop of artists, even if many are only of the do-it-yourself variety.

#### THE ECONOMY

We all know how the health of the American economy has been given a great upward thrust by the introduction of some important innovation: the automobile, the motion picture, radio, television and so on. Minor stimulations are also applied by, for example, such diverting objects as the hula hoop or the yoyo. The inventive thinking of a very few individuals was largely responsible in each case. In addition to the imminent expansion of colour television, international television and the introduction of travel in outer space, there are other, as yet unknown, innovations to come, on which our economy will feed, and thereby, we hope, remain strong. These developments will arise from the creative thinking of a few individuals. In order to ensure continued good health, the economy will need many more creative minds at work, with freedom to go where their hunches lead them. Can we facilitate the development of those creative minds? The answer must be that we can.

There is another aspect to this picture, however. I am told that industry often receives from inventive employees many more ideas than it can use. A single idea that might improve materially some device in general use cannot be adopted because the cost would be prohibitive—another creative thinker is momentarily frustrated. If the management is wise, it will give special rewards for such suggestions, in spite of the fact that it cannot use them, lest the outcome contribute to the drying-up of the source. And it should find a way of conserving such ideas for future use in a manner that will provide ready access. A few years

ago, some people were calling for a moratorium on inventions so that the country could catch up. We do not need fewer new ideas; we need more. No one can tell just when they may provide the key solution to important problems.

#### THE GOVERNMENT

All governments of societies such as the United States face perennial problems and unexpected crises which call for imaginative solutions. Facing our own national government there is the race between rising costs and the need for higher incomes. There are demands from noisy minorities. There are failures of local government and cries for increased services from the federal government, as well as cries against losses of freedom. There is rising unemployment in the wake of the increasing installation of automation. Strife connected with racial integration and civil rights still mars the larger family picture, and agricultural surpluses are always with us.

As we all know, problems in foreign relations present the government of the United States with exasperatingly complex difficulties. In the past we have not shown sufficient imagination in dealing with these. There are some signs of improvement, but more creative thinking and bold new solutions will be necessary before we can say that the difficulties have been adequately met. In the problems of everyday life, be they great or small, there is rarely only one right answer. Realization of this principle would help a great deal, for it would open up many avenues of thinking that might otherwise remain closed, and it would rid us of the paralysis that has too often prevailed while waiting for the one 'right' answer to arrive.

Those of you who are concerned with personnel problems with industry and with governmental agencies, will ask whether tests of newly-discovered, basic aptitudes will help materially in selecting inventive scientists, engineers, administrators and other personnel. About all we can say at this time is that wherever the new components of intelligence play roles in the success of inventive personnel, tests of them should add here and there to better selection than is now possible with traditional tests. Their contributions to selection would be unique; they would duplicate present predictions very little.

One should not expect too much of tests of these new aptitudes tomorrow, however, or even the day after tomorrow. Since the intellectual tasks in any occupation at professional levels are very complex, it is likely that no one aptitude plays an exclusive role, or even an

outstanding one. A combination of tests of different basic aptitudes is needed for each selection situation. The combination may vary from one group to another within the same occupation, as well as from one occupation to another. Which combination of tests will serve best in any particular situation will have to be determined on the basis of developmental research. To one who is looking for highly creative personnel, I would say that he should not confine his attention to tests of divergent-production abilities, for many other kinds of abilities contribute to creative output, depending upon the circumstances.

#### PERSONAL LIVING

We all have personal problems, whether financial, emotional or inter-personal in origin. Some reach the attention of daily newspapers, some even the attention of psychologists and psychiatrists, but the great majority are handled by the individual himself, sometimes with the help of family or friends. From the marked increase, recently, of psychologists and psychiatrists, one might conclude that such problems are growing in number and severity, or that individuals are giving up trying to reach their own solutions, or that in larger numbers they are refusing to live with anxiety. Some of my colleagues will not thank me for saying so, but I think it is time to start a do-it-yourself movement in the solution of personal problems.

With some concern for the development of creativity in children, I have had misgivings about the emphasis on personal adjustment in education; not only because so much attention to personal adjustment and the development of 'good' personalities has diverted attention from development of intellectual skills, but because in some places a certain kind of personality has been regarded as ideal. Where such an ideal has been set up as a goal, children have been encouraged to approach that standard pattern. This is one important levelling process to which many children have been subjected, the other serious one being the pressure toward mediocrity in intellectual performance. From such sources of pressure, the end-product from our schools might well tend to be a collection of happy, but mediocre, conforming manikins.

Personal and inter-personal problems involve what I have called, in my general theory of intellect, 'behavioural' information. Behavioural information includes our knowledge of the perceptions, feelings, thoughts and intentions in the minds of others and in our own minds, to the extent that we can become aware of them. It has been further theorized that abilities to cope with such information intelli-



gently are completely parallel, but distinct from, abilities to cope with other kinds of information, concrete and abstract.

Problem solving in connection with behavioural information should therefore be similar to problem solving elsewhere, and we should give the child exercises in solving such problems as we do in connection with other kinds of information. The basic and somewhat general abilities involved should be regarded as a group of intellectual skills, like other intellectual skills. This would be a much more positive approach than waiting for serious problems to arise and then applying externally imposed solutions.

In the process, the child would develop skills in the evaluation of solutions to personal problems—let us hope that the criteria would not be in the form of an ideal pattern of personality to which all should aspire. The essence of democracy is respect for the individual, which includes the right of each person to develop in his own unique way, so long as his conduct is socially tolerable. The emphasis on equality in all respects, where equality is taken to mean uniformity, is a mistaken attribute of democracy and defeats steps toward creative social progress.

We are aware, of course, that much instruction on mental hygiene attempts to prevent personal and behavioural problems. It is also true that those who deal with individuals who have present difficulties, not only aim to cure these but attempt to prepare the patients to meet future problems. There are, in America, adult education programmes under the heading of 'sensitivity training' aimed, apparently, at improving skills in empathy or what I would call 'behavioural cognition'.

In these three instances—mental hygiene, therapy and sensitivity training—there is a recognition of the possibility of acquiring skills through practice; in these instances, skills dealing with personal interactions and personal adjustment. How much better those goals could be realized if we could delineate the kinds of skills that can be developed for dealing with behavioural information. Knowledge of the nature of such skills would suggest the kinds of exercises needed. It will probably be found that readiness for solving personal problems in creative ways depends on divergent production and evaluation, where the information is behavioural.

#### HUMAN SOCIETY

Problems of enormous scope arise both on national and international scales, problems that call for bold, new solutions. The most immediate

international problem is how to keep the peace in a world of political turmoil.

The second international problem is less immediate but more dangerous for the future, and it is related to the first. How can we put restraints upon the exploding population? Recently there appeared in the newspapers the fantastic prediction that if present indications are correct, by the year 2026, only about 65 years hence, there would be standing room only for the human race. Regardless of how fantastic this prediction may be, our children are in for trouble. Do we have the inventive capacity and the political genius to solve this problem?

The fact that America's enthusiasm for increasing creativity is spreading to other parts of the world may be of some help. Alex Osborn's book *Applied Imagination* has already been translated into a number of languages. There are indications that efforts to increase creativity have occurred even in Red China. Perhaps the first reaction to hearing that one of our so-called 'secret weapons' is known behind the iron and bamboo curtains would be one of increased anxiety on the part of some of our most anti-communistic citizens. On second thoughts, however, if the cultivation of creativity leads to independent thinking, and if this independent thinking spreads to other affairs, it should also lead to rejection of thought control and of other restrictive pressures of highly controlled, collective societies, in favour of more democratic methods.

In American society, the emphasis on creativity should do more than anything else to counteract the recurrent waves of anti-intellectualism and the perennial indifference towards intellectual excellence. There should be a growth of tolerant respect for ideas. The constitution of the United States provides the right for anyone to express and to promote his ideas; and it balances this right with another, the right to criticize. It appears to have been the intention of the writers of our constitution to maintain a full interplay of divergent thinking and evaluation. The former permits us to examine all possibly pertinent, logical possibilities and the second guards against mistakes. Thus, in our efforts to make idea-generating citizens, we also have the responsibility to teach them how to evaluate. Evaluation implies well-considered sets of values, which takes us into the realm of philosophy.

I shall end this discussion by presenting a thought involving points at which creativity touches biology and, indirectly, philosophy. It is safe to say that there are at least some parallels between creative thinking and the processes of evolution if, indeed, it cannot be asserted that

creativity is firmly based in the evolutionary processes themselves. The parallels may be stated first in this manner. Darwinian conceptions of evolution lean heavily upon three principles: variation with respect to a trait, survival (or non-survival) and, if survival, then conservation of the trait variation. The parallels in terms of intellectual operations are quite clear. Variation corresponds to the kind of operation called divergent production, including fluency, flexibility and elaboration. Survival *vs.* non-survival is the grand test of any biological innovation. A biological innovation is tested and found good or found wanting. The survival test corresponds to the intellectual operation of evaluation. The conservation of the new trait corresponds to the intellectual operation of memory. A molecular alteration in the form of a gene is a recorded memory of a racial characteristic. Testing is more or less continual, for changed environmental conditions may alter standards of evaluation.

Only one fact prevents us from going all the way in suggesting that the creation of new inherited human traits is a matter of creative problem solving. This is the traditional conception of the nature of mutations, a conception that has persisted for a long time. Mutations have been commonly regarded as being produced by 'accidental' forces from without the organism, for example by cosmic rays. So far as I know, there is little or no popularity for the idea that an organism itself may initiate any of its own mutations. However this may be, it would seem desirable to keep some degree of openness of mind with regard to the theory of the sources of mutations. After all, there may be more than an analogy to the production of new ideas.

## **SUMMARY**

Potential for creativity is not a single ability but rather a collection of different abilities, each of which can be possessed in different degrees by each individual. Persons recognized as creative stand high in a number of these abilities, along with other intellectual abilities.

Creative abilities may be thought of as generalized skills, and like other intellectual skills they can be developed through appropriate practice. Such development has already been demonstrated, giving education in general a new interest in intellectual growth. Promotion of growth in creative qualities is a responsibility of teachers in almost any subject, with some subjects offering more opportunities than others. Even minor improvements in creative skills and an altered climate that appreciates creative effort could yield great changes in our ways of living.

Numerous problems in connection with economy, government and with society in general, call for imaginative, creative solutions. There is usually not just one right solution to a problem in these areas of living, but many possibilities, some of which are better than others. Thus evaluation is another intellectual quality that needs cultivation. But evaluation is often best suspended until ideas are well generated. Blanketing critical attitudes and climates should be avoided. Furthermore, if we want independent thinking, we shall have to reward it and not confine our acclaim to thinking that conforms.

Scientific research during the past decade has brought creativity down from the clouds and out of the mists, giving it dimensions the knowledge of which enables us to do something about it. Although a general increase in creative productivity in our population should naturally introduce some new social problems, these, too, can be solved imaginatively. We can and must learn to live with increased creativity, for without it we shall probably not live at all.

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*Gardner Murphy*

## **The Nature of Man**

One way of approaching the nature of man is to start with the context, the known universe or the 'author of nature' delineated by philosophy or religion, working in the manner of contexts from this larger nature down to the specific nature of mankind. This is indeed close to what is done in theological systems which start with pantheism or with monotheism as the most rational, or the most obvious, or the only tenable creative principle. From this vantage-point man would be the sort of thing which will emerge as the highest creation of a cosmic creative force. God makes man in his image, perhaps as a copy or as a creature cast by a mould, or by some principle of antithesis, like the photographic negative from the positive. Man is thus by nature divine, or 'a little lower than the angels', or he has the gift of reason because it is there in cosmic structure to start with. Thus we may, as in the gnostic system, work downwards from a principle of emanations or a descending series of creative acts, or we may, with a single supreme creative act, express the universe in a single impulse, as some astronomers think the universe was launched perhaps five billion years ago; and from this particular kind of creative act as imagined by the particular astronomer or theologian everything else will follow, at least within the general limits set by one theory of the nature of the creator and the creative act.

But there is a very different principle which is equally open to us, namely, to start with the simplest kinds of realities and proceed by a process of synthesis. The ancient Greeks loved to select some single principle—the universe is made from water, or air, or fire, or strife, or mind, or small hard material particles, or numbers, or pure rationality, or what have you—and by processes of accretion, or complication, or dialectical opposition, or structure, or diversification and integration (in the manner of modern evolutionary theories) arrive at high-level syntheses which still had in them the cast, the atmosphere,

the form, the odour, the meaning, the structural rhythm, of the original stuff, substance or act. Most attempts to find the nature of man have concluded, 'Here it is, right in the primordial stuff'. If you are Democritus or Lucretius, starting with the logically defensible concept of small material particles in motion, you ultimately derive the nature of life and of man. If you start with the Pythagoreans and you see the simplicity and all-embracing, clarifying effect of number theory upon the effort to understand, you conclude that a rational universe is derived from the ordering of numbers. The numbers, in relation to special kinds of quantitative limitations, such as those of space, give you the perfect structure of music, as known to the Greeks, or explain the aesthetic as well as scientific implications of natural numerical order. As Edna St. Vincent Millay has it, 'Euclid alone has looked on beauty bare'.

The first philosophical method, conceiving man as derived from a cosmic context, has therefore swept *down* the skies to the last particle of dust, even to the 'dust to dust' which is man, one with the earth of which he is compounded. The other method, working from simple to complex, has started with the primordial dust of Democritus' system, and in a whirling-upward vortex has created the highest rational being known. And it may go on to extrapolate one vastly higher.

It is, of course, entirely feasible to use both these methods, and to proceed from the two ends at the same time—making man midway between the least and the greatest. Eddington points out that man is midway between the electron and the whole cosmos, in the sense that the number of electrons in the living body is 10,<sup>29</sup> and the mass of man's body in such units raised to the 29th power is the mass of the known universe. Many have thought of man as midway between the infinitely small and the infinitely great, and what is more to the purpose, midway in all sorts of qualities or degrees of reality or fulfilment. As a matter of fact, he has to be. If man is to be, as another school of Greek philosophy had it, 'the measure of all things', he must be able to look up and to look down at the symmetry of the world. As he faces receding horizons in the skies above and in the waters under the earth, he must find himself in the middle of a structure, just as in his tales of creation the earth must of course be the central core of the universe. Man is the greatest when compared with that which lies below, and the least when compared with that which lies above.

Better yet, if we want philosophical symmetry, is to imagine man as a sort of convergence of maximizing and minimizing tendencies,

the place where the forces which belittle and the forces which aggrandize meet. He can be a unique synthesis of the greatest and the least, or if you prefer, he can be a synthesis of all the realities which exist in this universe, and therefore at a midpoint on any quantitative dimension which connects the greatest with the least. He can be a macrocosm and a microcosm at the same time, because the greatest is nothing more than an elaboration of what he already is, and the least is nothing more than a perpetual reduction of him, without essential change, to his lowest or his simplest form.

He remains the measure of all things, but in many different senses of the term. Perhaps the most congenial to modern thought would be the statement that he contains within himself the raw stuff, form and active principle of that which is simple, fundamental and eternal in cosmic structure, and at the same time he is a promise, a forward reference, to all that can be fulfilled. He can therefore be known best by striving first to define what we know of the least, then what we know of the greatest, and finding a sound method of interpolation between the two. Don't make him too divine or too trifling, for that would be eccentric. The one would inflate his ego; the other humiliate him beyond tolerance. Keep proportion. Maintain the Golden Mean. 'Remember you are nothing but a man', as a wretched slave said to a Roman conqueror in the very midst of his triumph. 'A man's a man for a' that,' said Robert Burns in protest against too much of 'vile earth and miserable sinners'.

It would, however, be more in keeping with the modern spirit to say that it is not just the algebraic mean that concerns us, but rather the creative process. Reality for us in the modern thought age is that interaction of opposites which gives the emergent, the new, the light that never was on sea or land. It is not just the dialectic of opposites, but specifically the interaction of those opposites which have to do with complexity, grandeur or spontaneity that produces obeisance in the scientifically-oriented, but still worshipping, mind of today. Find where the cosmos and the infinitesimal meet in a flash, or in an equation where order, beauty, truth coalesce, and there you have the potential nature of man. A more congenial way of stating it would be to ask the very best of man, set your sights as high as you can and at the same time look at all his baseness and capacity for evil; deny nothing; take it all and try to create a picture which contains in true and undistorted totality both what he is and what he may become. Provide Jonathan Swift's world of Lilliput, where Gul-

liver is a giant among the little people; provide also Brobdingnag, where the doll-like little Gulliver is lovingly cared for by the towering 'little girl' Glumdalclitch. And provide the *inferno* and the *paradiso*, the unutterably horrible and the unutterably sublime. Start neither by overconcern with the little nor with overconcern for the great, but with all that can be learned by the observation of both. Our task then will be to proceed from the little, and from the great, and see where the two meet; but likewise to close in upon man from all the other poles in all the dimensions in which his life can be viewed.

Starting then from the very small and proceeding to the very large, we seem to have today a fulfilment of Democritus' conception of the universe made of particles, all of which are in some sense fundamentally alike. Democritus had atoms of different sizes and shapes, and his Epicurean follower, Lucretius, beautifully extended the conception. Particles are not all exactly comparable—cut from the same bolt, if you like—but they are all alike in some ultimate sense, being particles subject to general laws involving attraction and repulsion, organization into groups, and a subsequent scattering back into their elemental form. They all move in time and space; they all structure themselves, in fact, into such complex compounds as permit the treatment of the compounds themselves as special units (essentially as in solid geometry). They also attract and/or repel one another; they constitute dynamic systems. Thus electrons and protons in combination can give atoms, and the atoms in aggregation can give molecules, and the molecules in aggregation can give a matrix from which cells arise. It turns out then that the universe is 'compounded of' particles which in increasing complexity give a hierarchical structure.

From this point of view, the first answer to the question—what is man?—is that he is a compound in which the laws of compounding are just as essential to his nature as is the stuff of which he is compounded. Here the ancient mechanism-materialism or mechanism-vitalism issue resolves itself into a question about what the organization is, or does. There does not seem today to be very much place left for the conception of a 'vital something' which is a substance independent of the particles of which things are compounded. As C. D. Broad has put it, 'substantial vitalism' has been slowly but effectively ushered out, leaving its cousin, 'emergent vitalism', with its emphasis on life as an *emerging organization*, steadily gaining in popularity. We tend to agree that the modes of articulation of non-living and living are different; life exists not as a kind of stuff but as a special mode of organization.



This may seem to some to be the last gasp of vitalism. But in point of fact the concept of organization is of such central importance in modern science and philosophy that nothing of any real importance appears to have been lost from the vitalistic tradition. A pantheist or a theist could, if he liked, speak of the organizing principle as the breath of life; or in analogy with the old figure of speech about fire as creator, one might comfortably talk of the 'spark of life' or of some super-enzyme which is, in fact, the creative *élan vital* of Bergson, or could well be the 'one eternal purpose' of a self-fulfilling cosmic process. Man, then, while being 'resolved' to the almost infinitely little and its compounds, comes phoenix-like to a new life in which the process of compounding him remains central to the drama.

Something is beginning to be learned about these laws of compounding as they are studied by chemists and biologists, by psychologists and by social scientists. Indeed, the attempt is repeatedly made to demonstrate that they may indeed be similar *at all levels* where life is found. Those who occupy themselves with 'general systems theory' appear to be making a reasonable case for the view that at least here and there the basic form of compounding and integration, even in its mathematical expression, may carry over from one formal level of the life-process to another. Thus Miller, Rapaport and Kluckhohn make a vigorous effort to show that the evolution of language and the evolution of systems of value obey basically similar laws, and are capable of being expressed in the same formal terms.

If, however, man is more complex than other animals, and if the structure of human society is in some ways more complex than the structure of the individual organism, it is possible that there are some modes of compounding which appear in man, and especially in society, which never appeared before in the history of the universe. This would mean that no matter how broad the laws of *general* compounding, there are in addition some specific laws which apply only at these *highest* levels. Man would thus be unique in some respects, and indeed each level of his social organization, as his social life develops, might be unique. It may be that the scientific-technological kind of man now coming into existence will represent an emergent, which quite literally is built on laws of compounding or integration which *had no place before* in the world structure.

In a formal sense such new laws could be derived from a sort of pre-existing matrix in the sense that any new number you can think of, no matter how big, exists latently in a presumed cosmic order

of numbers; or that any chemical element which you can produce in the newest electric furnace, though terrestrially entirely new, was inherent in the basic lawful system of the physical sciences; in a new star a ferocious temperature may create a compound which never could exist before. Such new elements are still derivatives, as it were, from fundamental axioms in the structure of the universe. They would not be created by an arbitrary fiat transcending existing law. But they would be new in the simple sense that they had never appeared. From this point of view, man is *really new* as he moves from step to step in organizational structure. He realizes much that never existed before. There is, in a sense, fully and genuinely creative evolution.

But some thinkers want creativeness, or newness, in a much more *radical* sense. If the basic laws of structural organization are merely being carried to a higher level, extrapolated into new complexities, we have failed to utilize Bergson's conception of the truly creative; we are still living in the same old prison-house psychology against which Bergson protested with the question: 'Is there really anything new under the sun?' Something in us, perhaps as primitive as the infant's struggle to unwind himself from the blanket, wants to say 'no'; the act of creation will give man quite literally a *fulfilment*, and not just a fulfilment of what he is but of what lies in utterly unstructured form—that which is only potential in an abstract sense, that towards which there are no arrowheads of today recognizably pointing. Perhaps William James' 'arbitrary spontaneity' is the best phrase to convey this sense that from the life processes and laws that we know, and even from all those life processes and laws that we do not yet know, or never can know, no safe *prediction* can ever be made. Against the resulting conception of true indeterminism (not at all the simple 'uncertainty principle' of Heisenberg, but an absolute nihilism regarding ultimate causality) the scientific temper of today would seem vigorously to protest. For all that, such concepts hammer at the door; and if not the men of today, then the men of tomorrow, will have to let them come in and sit down and say their say.

When we turn from the approach in terms of little-to-big to a contrasting approach in terms of big-to-little, when we look not at microcosmic but at macrocosmic perspectives, we are faced with an historical and a substantive picture of the evolving world order. We ask ourselves, as did the ancient Greeks: Is it possible that more fundamental than the particles are the trends which express themselves in

the perpetually changing order of the world, the law of change as expressed by Heraclitus in fire and in conflict? Fire is the first principle, and its embodiment, strife, is the 'father of all'. The Greeks saw that there was more than a harmony of the spheres in cosmic structure. There was strife, a primordial conflict between component parts, which in a certain sense was the obverse side of the exquisite harmony itself. It is a world of vast celestial spaces, and of whirling bodies which occasionally engender, at immense temperatures, new stars betokening that unity, strife and birth are all aspects of one process. On the vast cosmic fresco, waters and winds, fires and rocks express materiality, while attractions and repulsions, disintegrations and reintegrations according to a law of number and of quantitative science, provide each aeon with new contexts for new events. Thus that which occurs at the highest level of cosmic abstraction, the potentiality for integration, strife and creation, provides for the specific contexts within which specific planets, specific oceans and archipelagos, and in time specific protozoa and the orders of an evolving universe of life may proceed. The cosmic context determined what must happen: determined the thing called life and determined its aims and manifestations. Each living thing had to find a niche for itself among those which already existed. Each, according to the law of 'adaptive radiation', had to find a habitat, a world where it could make its own home, providing a lattice-work of new niches in which still newer tides of life may eddy into their own small coves and inlets.

It is from this point of view that life as we know it is 'demanded' by the special succession of shrinkings, contractures, coolings which represent the physical changes in the earth, and which, through the required salinity and the middle range of sea temperatures, provide for the peculiar processes which we know as marine life. It is from the conditions of the sea and from marine life, the cooling and re-ordering of the earth's surface, that creatures are called into existence, called out from the sea almost as Prospero would call them from a dark eternal sleep into the little dream-world of their momentary terrestrial existence. It is from these early land-dwellers that the conditions gradually developed under which warm blood and the maintenance of a relatively constant internal environment ultimately gave rise to the mammalian code of life, and from the joint and interacting evolution of the earth's face, its caves and trees and the pressure of mammalian stock under the conditions of adaptive radiation, that the simian and ultimately the human type is derived.

If you ask, then, what kind of a thing man is, you get the reply, from this philosophical outlook, that he is the kind of thing called into existence by the set of cosmic and terrestrial and biological forces of which the record is already written in the language of the astronomer's spectroscope and in the language of the paleontologist's multidimensional maps of changing body forms and functions in response to changing cosmic contexts. Obviously we must push as far as we can every insight derived from this concept of cosmic context. The more we know about the history of the cosmos, of our own planetary system, and of our own fossil record and the contemporary dynamic of biological adaptation, the more we shall be able to etch in the contextual problems with which *homo sapiens* and ultimately modern man has had to cope.

Now obviously this approach, from simple things upward and from complicated things downward, involves no basic contradictions. It is already clear from the astronomer's evidence that vast nebulae are made up of particles such as we know here in terrestrial laboratories of physics. And while it may be true, as Bridgman has suggested, that there are laws of physics which apply at some time-space corners of the universe but not elsewhere, we still seem justified in saying that the laws as we know them are as universal as Newton or Laplace could have demanded; and, if not eternal, at least appear applicable to the whole range of astronomical space and time to which we have access. This does not mean that Newtonian laws are the same as Einstein relativity laws or Planck quantum laws; it means that as the laws are revised it is assumed that they apply *everywhere* in time-space.

This has two direct implications for us: firstly, it means that man is basically of *the same stuff* and that his being represents essentially the *same kind of texture* of scientific law as seems to hold for everything else in the universe. He may, as we saw, be developing in a manner expressive of new *emergent relationships*. But except for those who want a truly 'arbitrary spontaneity' (cf. James above), we shall find ourselves encountering an up-building emergent system which leads to protozoa, to man—and on to a dimly-envisioned superman of Nietzschean, Shavian or Huxleyan form. Secondly, working downward from cosmic context to the human individual we shall likewise find the universality of the laws that apply to man. The two extrapolations must, of course, meet, like the radii connecting two points on opposite sides of a circle. We should expect man extrapolated from the simple

to be the same as man extrapolated from the complex—the same in terms of his material constitution through particles and the same in terms of the laws of interaction, the time trends, the progressive and regressive changes in his own personal structure and in those of the social groups and cultural forms to which his life gives expression. He is the middle, the waistline, of a vast hourglass.

The philosopher and the scientist will rightly ask at this point whether the symmetry of the hourglass is an artefact. But we have already admitted this in quoting the dictum that man is the measure of all things, and in noting that with the instruments of knowing which are accessible to him he would have to place himself in a central position of some sort. He would, moreover, for well-known philosophical reasons—to which Kant on the one hand and Mannheim on the other would point—have to see it this way, whether it has any ultimate or absolute objective reality or not. Man can, up to a certain point, know and control the world through the limited methods of science. He is predictable by the method of working up and he is predictable by the method of working down, and like the two tunnels simultaneously made on opposite sides of the mountain, these methods have to converge to let the light through.

There is one thing wrong with this picture. It is too unified, too rational and too comforting. We begin to feel that this convergence of evidence, this placing of the man at the waistline of the hourglass, is a necessary outcome of his ways of knowing and expresses all the limitations which philosophy and science have placed upon it. We have, as it were, 'painted ourselves into a corner', created a system which is too redundant, too overdetermined. There is too much 'feedback'; everything we know reflects back and confirms more of what we have already begun to assume, like the focal point in a super whispering gallery in which all voices come from the same mouth.

When confronted with such a situation we are likely to find fake comfort in two general principles. The first is that the universe must be basically orderly and intelligible, and therefore, the transfer of our laws from one part of the cosmic scheme to another—so that what we find about man from one angle agrees with what we find from another angle—must, since it is intelligible, be essentially correct. This, however, would have to follow from the very nature of the conceptual processes which have been carried out. Since such conceptual processes cut slices or punch holes according to more or less similar keys in

different sub-universes, they have to come back with patterns which can be superposed and 'let the light through'. This is like a programming device which would give as a solution to all problems  $0 = 0$ . This does not mean that the results are all 'wrong', or that all reason is 'circular' or that all rationality is 'self-deceptive'. It means merely that the derivation of comfort from supposed confirmation of a repeatedly assayed method, offers in point of fact no more proof for reality than would be entailed by the Euclidean demonstration of a principle through the use of axioms.

The second source of our comfort and gratification in the 'unity of knowledge' cuts deeper. The abstractions and predictions which have come from the scientific method do actually appear to indicate that the brute stuff 'out there' which we cannot expect to know 'in itself' somehow has at least consistency enough to be able to throw shadows back to us which we *can* perceive. From the colour of a precipitate we can predict what will happen in another modality of experience such as taste and smell, and even from *new* interactions as in the flashes and the sounds of explosions which come the first time that a specific reagent reacts with another. A cross-checking and confirmation through independent sense evidence and through the verification of predictions satisfies us that there is a modicum of real intelligibility about the world order including that portion of it which we call human life. Only a small fragment, perhaps, of human life is knowable today in scientific terms; but more and more has been gained in the scant 300 years of experimental science. Evolutionary biology has only been with us for a century, yet such intimate and revealing information as the neurophysiology and biochemistry of the brain belong almost to the discoveries of yesterday, and we are already learning a good deal by biochemical and electronic stimulation of nerve cells. We are beginning to get corroborative evidence about some of the physical substrates activated when various sensory, intellectual and emotional aspects of our nature are aroused. We do not know how far the scientific method is applicable, but we know that it can be applied more today than yesterday and that the trend is accelerating.

The issue is not whether our reasoning is redundant or even circular, for it surely *is*; nor does it lie in the question whether we can cross-check and predict behaviour, for we certainly *can*. These propositions, which a philosopher might assume to be contradictory—firstly that nature is unknowable, and secondly that it permits more and more precise predictions—are both manifestly true, and it is up to us to

contrive a philosophy in which the truth of both can be accepted and a reconciliation clearly stated. It is in this direction, of course, that most of the philosophy of science is tending. No great prohibitions seem to be experienced as a result of the inherent subject-matter itself. The issue lies at another point altogether; it lies in the questions 'have we created in our own little way a "block universe", a system of reality regarding which we must pursue those methods which give more and more of the *kind of reality* we already have?' and 'Must we create an image of man who is more and more like the thought-forms expressive of our momentary vantage-point?' The question is not whether we shall find 'absolute' truth, whatever that may be. The question is how far we *consciously or unconsciously distort or exclude the many different kinds of pictures of man which are suggested by the fringe evidence of every era.*

It is well worth while to remind ourselves of what the historians of science have so beautifully documented; namely, that each new factual discovery must wait for a long time before a new coherent theory can be constructed in which to lodge it, because men would rather reject the new evidence than forage about to try to find a completely new frame of reference—the old frame of reference is for them 'home'. If the wandering lonely facts do not come like snails carrying their own houses upon their shoulders, and they seldom do, they must wait for years or for decades. The discovery of oxygen by Priestley, as Conant has shown us, did not disrupt or discourage the predominant belief in phlogiston, because the phlogiston theory was systematically buttressed by all sorts of evidence and cross-arguments, and solitary facts about oxidation could always be squeezed, fussed with, or even excluded until the new system was all constructed and ready in which to house the new small facts. At Yale College, founded in 1701, as the early catalogues show, astronomy could be taught either by the Ptolemaic or the Copernican system of assumptions. In Dayton, Tennessee, in 1925, the court held that evolution à la Darwin could be taught as a theory but not as a fact. In such cases the old theory could not be put to rest, because there was too large an investment in it. Since the new about human nature is almost certain to be felt alien, unsound, absurd and irrational, it is not surprising that the derivation of man from simpler animals was adjudged doubly irrational, first because it was contrary to accepted common sense, and second because it made man the descendant of relatively stupid creatures. This was not the image of rational man to which Plato and Aristotle, the ancient philosophy of India, the wisdom

of the Church Fathers, and the rationality of the Enlightenment, had all pointed. The intelligibility of a system in which the earth is not the centre is obviously lower than the intelligibility of one in which the earth is the centre, at least so judged the age of Copernicus. The intelligibility of a picture of man which again makes him eccentric to an evolutionary process, and by no means a safe or eternal resident of this earth, is still harder. The rationality of a world in which light curves round a large mass, as in Einstein's system, is obviously lower than that of the Newtonian cosmology. Worst of all, so far, are the psychological absurdities involved in making man largely blind and unaware, as the psycho-analysts say he is, and in finding even the cosmic orders relating to human conduct derived in considerable measure not so much from a 'categorical imperative' as from an unconscious process by which the images of parents are early incorporated into the striving instinct-ridden mind of the small child. Men feared irrationality partly because it left this safe universe much less safe, and partly because it pointed to possible irrationalities within themselves, which they could hardly bring themselves to recognize. One might even look up from one's reading and wonder whether the rationality of one's analysis of this whole situation is necessarily at a higher level than that of the other steps already considered, and wonder what will happen if still other concepts of human nature arise in the next century, constituting similar assaults upon fundamental assumptions, fundamental rationalities.

There are two additional difficulties confronted by efforts to extend our knowledge of man, both rooted to some degree in this general fear of the unintelligible but both extending considerably beyond it. The first is the belief that the intelligible, being consistent and intellectually respectable, has to be real, has to be true; the second the belief that the particular criteria of intelligibility which have arisen in modern science can be equated with a universal law of intelligibility; that which is unintelligible within the scientific frame of reference of recent centuries is universally and absolutely unintelligible. This needs only to be stated in order to become a fresh source of anxiety. Intelligibility became a hallmark of the geometric-mechanical conception of world order which, though derived from Democritus and Lucretius, was enormously solidified by Newton and by Voltaire and the rationalists of the eighteenth century, so that that which was unintelligible from the specific geometric-mechanical frame of reference became the supreme example of unintelligibility. 'Give me,' said Archimedes,



a place to stand and I will move the earth' (by the action of a lever). 'Give me,' said Clark Maxwell, 'a world of which I can make a model.' That of which you can make a model is intelligible. Modern concepts of the atom have, of course, struggled against the geometric-mechanical models, and with considerable success in the limited domain of nuclear physics; but their deep acceptance and internalization by rational men in the eighteenth and nineteenth centuries leaves them as part of the general baggage of the rational man of the mid-twentieth century. We allow the nuclear physicist to go his strange way into his own special sub-microscopic world, but it is assumed that the world not only of physico-chemical reality but the world of biology-psychology-and-social reality must in some sense still be modelled upon a geometric-mechanical super model.

When this is realized there is again uneasiness, but there is comfort in the protective device of separating two universes of discourse, in one of which we allow the nuclear physicist his own unique task, while for the rest of us—that is, the practical scientists and practical men of reality—the safe orderly world of yesterday's thought is maintained. Evidently the threat to our own rationality is still serious. Aware of such issues as these, a vast literary and philosophical school of anti-geometrism and anti-mechanism has flourished, if not in science, at least in the outposts of science known as existentialism. Anti-intellectualism, too, has begun to raise its head with increasing confidence. Whether, in terms of some ultimate or absolute rationality, if there be such a thing, this anti-intellectualism is itself a 'sound' logical ploy can hardly be debated within our present small compass. It must, however, be noted that we are now at the cross-roads of an imperious new pan-rationalism and an imperious counter-rational movement which make up much of the substance of today's philosophical struggle about man, taking the place of the older mechanism-vitalism argument. At perhaps a more sophisticated, but certainly a very much less comforting level, the simple *ad hominem* arguments of mechanists and vitalists take the form of abstruse definitions of space, time, causality, which will not rob the irrational of its right to exist and will at the same time appeal to those who must be rational at all costs.

Imagine then what will happen to empirical findings, if they should come to light, which indicated realities in human nature that by their very definition have to be accepted as real and incorporated within a somehow intelligible scheme of man, and at the same time patently

involve radical rejection of the geometric-mechanical conception of matter, time, space and energy. Worse yet, imagine at the same time that they appear to use little or nothing of the 'new physics' which was given its right to emerge from the 'old physics'. If there be such empirical findings, let us by all means turn away from them, we may say; let us push their heads under the water; we have trouble enough philosophically with the anti-intellectualism problem, trouble enough in accepting the new complexities without giving up such clarity and order as the old time-space system permitted.

This is exactly the kind of issue which arises when psychical research or para-psychology is confronted. It would be such labour, such unbearable toil and trouble to dig into such events, such observations, no matter how skilfully made. Helmholtz told Barrett that not even the united testimony of all the members of the British Association, and not even the evidence of his own senses, could convince him of telepathy because it was 'manifestly impossible'. That way madness lies, thought Helmholtz, for the scientist who is also a serious philosopher living in an enlightened era. No amount of spontaneously occurring material studied by the methods of anthropology or clinical psychology can suffice when the weight of counter-assumptions is so great. Recourse must therefore be to experiment. When the experiment is concluded and the data published, the normal response is the response of Donald Hebb, 'If it were a question of evidence we would accept ESP; we cannot accept it because it does not make sense.' The irresistible force strikes the immovable object and we have the old question, what will happen? If, of course, there is such a thing as an irresistible force, there is no such thing as an immovable object. If there is such a thing as an immovable object, there is no such thing as an irresistible force. There is, however, in the mind of a modern scientist or scientifically-trained observer of human life, no such thing as extrasensory perception, and the immovable object, which exists in the form of the modern scientific mind, stands firm, showing that there can be no such thing as an irresistible force, i.e. satisfactory proof of the paranormal.

But can we be sure? When stated in this way, is it not possible that with improving experimental control, and more and more successful replication of the experimental techniques of recent years, there may actually be an ocular demonstration repeatable in an individual laboratory or in many laboratories that may in the course of time do what has so often been done before (as with the oxygen theory), namely,

begin to have an erosive effect upon the rich and orderly conceptual system which had pre-empted the field? Stating it all conservatively, it is only necessary to point out that in the empirical temper of modern science, certain levels of replicability of experimental observations can be conceived which will alter even the redundant and solidly-built structure of the modern world system. Whether it happens to come through the gate of para-psychology or through some of the other gates of the walled city, there will be new knowledges about man. They will be hard to come by, and they will entail not only much replication of experimental method, much reconceptualization and a great deal of emotional wear and tear on all involved, but they will gradually recolour the tableau, the mural painting of man, then alter the shadings, then the fainter contours, and finally the large contours themselves, until the whole meaning has been restructured. The result will be a very disturbing, probably frightening, exhilarating, maddening new image. What will this new image be?

Despite obvious shortcomings we find ourselves cast into a prophet's role—or, indeed, we might say into a prophet's pool, to see if we can swim. This means that we must attempt to say something more specific about man's future. And, since I am neither a nuclear physicist, nor a general, nor a statesman, but only a psychologist, my attempt to look into the future will be made only in psychological terms.

I shall attempt to deal with human futures in terms of three concepts. First, the genetic modifiability of man; second, the environmental—including the socio-cultural—modification of man; and third, certain types of interaction or potentiation of the biological by the cultural, or vice versa.

Only a few years ago it was still common to encounter the statement that man's basic physical structure has not changed much in 30,000 or 50,000 years, or some other such enormous span. The evidence for such statements was based partly upon the study of skeletal remains and partly upon the general analogical reasoning which allows genuine genetic changes to occur primarily as a result of occasional mutations. Evidence in recent years seems, however, to have supported the view that each newly-conceived individual contains several mutant genes; that occasionally these produce visible (phenotypic) effects, and that the experimental production of such mutations, already achieved in research with *Drosophila* and with rodents and other mammals, may well give, within a few more years, some rather fundamental information about the modifiability of the human gene pat-

tern. Over and above the experimental change in human nature which should be feasible within a century or two, there is obviously a great deal of biological change which comes from new forms of hybridization going on all over the world at a very rapid pace. Not very much is known about these results, but massive alteration of the characteristics of the human gene pool is certainly likely to increase the relative frequency of certain attributes and reduce the frequency of others. It may well be that there has not been much biological change in the last 30,000 years, but by the same attention to science one may say that it is highly probable that there will be massive changes in the next 3,000 or even 300 years. As Sewall Wright has well said, 'Evolution is still going on.' It will be noted that none of this assumes the inheritance of acquired characters nor has to do with the immediate short-term effects of changes in diet, regimen, protection against disease, etc., which will certainly have complex and important biological effects. We may treat these last-named phenomena as expressions of social forces interacting with biological forces.

If we ask ourselves what kinds of changes in human nature can come about *very rapidly*, we may point to Margaret Mead's observation of people passing from Old Stone Age culture through machine culture to the atomic and space age in one generation, and reply that we should expect two fundamental changes to come in seven-league boots: 1. science-technology; and 2. value systems (in art, religion, words and the feeling for the 'good life').

When we ask what types of social changes, what types of accelerated socio-cultural movement in the direction of science for the establishment of new value systems may be expected, we may say first that it is not true that all thinking, all abstractions are a dual expression of visceral needs. Man in each generation develops a real craving for thought and understanding. The childing's craving to understand can be channelled, cathected, imprinted or otherwise guided and controlled so as to give from early life onwards very specific intellectual cravings, interests, *wants*, around which the meaning of life is organized. In the same way, the early sensory-aesthetic needs are channelled, and a value system is acquired.

The craving to *understand* and the craving for sensory-aesthetic expression tend to coalesce. The predicament in which man is placed in this universe, which he can understand and control largely by the use of his brain rather than his brawn or his vital organs, is forcing upon him cultural developments which will entail more and more interest

in and fascination by the world of knowledge, the world of curiosity, of question-answering and question-setting, a world satisfying to an aesthetic craving as well as to our curiosity. There is apparently no escape from the conclusion that if mankind is to survive at all it will be a type of mankind pre-occupied with *making sense out of this world and of himself in relation to it*, and a world satisfying to his law of order and beauty. He may do an enormous amount of damage to his fellows and to himself in the meantime. In fact, he may cease to exist. If he does exist, however, it will be on the basis of intensive cultivation of a motive to perceive, judge, think in accordance with the inexorable laws of the universe around him. If he cannot make this kind of adaptation, he cannot make any adaptation at all. If, moreover, we are right in arguing that the socio-cultural world is ultimately of one piece with the stuff of which the universe is made, there will be no ultimate incompatibility between his learning how to navigate in the physical world and his learning how to navigate in a somewhat more complex but not essentially dissimilar socio-cultural world. Our answer then to the question of the direction in which social evolution must move is that it will move by virtue of the channelling or cathectic principle, by giving each generation a stronger and stronger value patterning in the directioning of essentially high-level perceptual-cognitive aesthetic functions. This does not mean obliterating any of man's vital drives; rather, it is likely to enhance them, since the organism is not built in terms of basic incompatibilities between cognitive and vital functions. It will mean, however, that only a humanity moving towards greater rationality is conceivable as a denizen of this universe a thousand years hence.

Finally, in this prophet-role to which we have been assigned, we have to note that relatively little arises directly from raw genetic potentialities in the way in which birth-marks or notched wings appear from genetic predispositions, and relatively few things arise simply from a cultural moulding process which regards the child as clay in the hands of the potter. Rather, there is a continuous process of pushing, pulling, buffeting, squeezing, pounding or, if you like, comforting, punishing, hugging, tantalizing and gratifying the young until they take on patterns which are more or less in keeping with the value world of which they are a part. Most of the physical changes in man's life are not due any longer to natural selection but to the fact that social evolution has created medical and industrial techniques for the prevention of disease. Nutrition makes Japanese taller, and Tennessee mountaineers

less subject to pellagra. Human physical types are changing with extraordinary rapidity as a result of nutrition and immunology and it is likely that psychogenic mental retardation and many other types of mental malfunction will in the same way be caught by a kind of back-door process in which social discoveries lead to different physical handling of the human young and the release of their potentialities in new directions. So, likewise, human cultural types are being driven to change; they have no choice. Or, if you prefer, those that go on rejecting the mind-stretching opportunities of today will cease to exist. This is a way of saying that those that can't exist will cease to exist; those who remain will be a humanity that *can* exist in the new world which is coming into being.

*Max Hamilton*

## **Prediction in Practice**

The theoretical solutions of prediction have been known for a long time, yet very little application of predictive methods is made in the social and psychological sciences. The reason is not far to seek: the gap between what is practicable and what is required theoretically is very great, and there are no simple methods for reducing it. An account of some of the difficulties involved in the application of predictive methods cannot but be of some use. This paper is concerned with psychiatric data, which are psychological and medical rather than psychological and social. Although they have their own special problems, they exemplify some of the general difficulties.

In statistics, 'prediction' covers a number of procedures which are given different names in practical life. When a patient goes to a physician for treatment, he submits himself to a procedure which starts with the taking of a medical history, goes on to a physical examination and may continue with various physical and chemical investigations. On this basis the physician endeavours to make a diagnosis, allocates to the patient a course of treatment and finally makes a prognosis, an estimate of the eventual outcome. From the statistical point of view these three are formally identical, although in common speech only the last would be regarded as 'prediction', for statistical prediction is concerned with the relations between a set of variates. Data are recorded for a set of objects or persons, either as qualitative categories or as quantitative variables, and it is required to estimate one of them or a composite of more than one, from the information provided by the others. Once the 'prediction' formula is established it can be used to estimate the predicted (dependent) variable in further examples, e.g. for other patients. Since the three steps of making a diagnosis, allotting a treatment and making a prognosis are formally identical, then theoretically it is not necessary to go through these three states in the correct order: the second and third can be carried out independently of the

preceding ones. This is not done in clinical practice because the actual procedure differs from this theoretical model, though the details of this are irrelevant here. The relations between diagnosis, treatment, prognosis and the data available, having been ascertained in the past, can then be used to allocate new patients to appropriate categories.

Since the data may be qualitative or quantitative, there are four distinct types of statistical prediction. The mixed data obtained by the physician is handled in a semi-intuitive manner, but the statistician works by converting all the data into one type or the other. Quantitative data may be handled as qualitative, for example, by the method of dichotomizing the variable into two categories above or below a given threshold. The reverse method, of allotting a quantity to the qualitative categories, is preferable, since quantitative methods are usually more accurate or sensitive. It will be worth while here to consider the elements of quantitative prediction to develop their implications clearly.

The simplest type of prediction is embodied in the linear regression equation. If  $Y'$  is the estimated dependent (predicted) variable,  $X$  the predictor (independent variable), and  $B$  the determined coefficient, the linear regression is

$$Y' = A + BX,$$

$A$  being an appropriate constant, to take into account the means of the two variables. If the variables be measured as deviates from the means, the equation becomes

$$Y' = BX.$$

If the variables are put into standard form, i.e. if they have a standard deviation of unity, the equation is usually expressed in the form

$$y' = rx,$$

as, under these conditions, the coefficient of  $x$  is identical with the correlation coefficient between the two variables. The last form of the regression equation brings out clearly two important aspects of prediction.

If we consider the case where we wish to estimate  $Y$  for a given individual but have no information about him, then the best estimate, the one which will give least errors in the long run, is to estimate him as having the mean  $\bar{Y}$ . This is not satisfactory as it stands, since it is always better to give a range of scores rather than one figure. The estimate then becomes  $Y$  plus or minus two standard deviations,  $\bar{Y} \pm 2 S.D.$  This is the 95 per cent confidence level, for in a normal



distribution, it covers 95 per cent of the distribution. Obviously, to predict that an unknown individual will generally have a score within the range where most individuals are to be found is nothing more than a long-winded way of describing complete ignorance. If some information is available on the individual, e.g. his score in the variable  $X$ , then it is possible to shift away from the mean, according to the distance of his  $X$  score from its mean. Perfect prediction means that the scores in  $Y$  and  $X$ , in standard units, are identical; and this is shown by a correlation of one. Nil prediction comes from a correlation of zero, and the regression equation shows that whatever the value of  $x$ , the value of  $y'$  is zero, i.e. the mean. Imperfect prediction, a correlation of less than one, shows that the estimated  $y'$  will always be less than the true  $y$ . The variance of  $y$  scores can therefore be divided into that which is predicted,  $r^2$ , and that which is not, the residual variance,  $1-r^2$ . The standard error of prediction is the square root of this variance, i.e.  $\sqrt{1-r^2}$ .

A simple example will make the point clear and indicate the practical limitations of the method. If the correlation between the variables be 0.6, a figure which is regarded as quite high in research into personality, then the error of prediction is  $\sqrt{1-0.6^2}$ , which comes to 0.8, and this reduces the error of prediction from  $\pm 2$  S.D. to  $\pm 1.6$  S.D., a very minor reduction. Fig. 1 is a scattergram illustrating, for two imaginary variables, the relation between predicted score and final score obtained from a correlation of 0.64. A correlation of zero means that whatever the obtained score on  $Y$ , the predicted score of  $Y$  will be the mean, and all predicted points will lie on the horizontal line drawn through the mean of  $Y$  as shown. A perfect prediction means that whatever the predicted  $Y$ , the obtained  $Y$  will be identical, and all points will lie on a line passing through the origin and making an angle of  $45^\circ$  with the two axes. Imperfect prediction will be represented by dots scattered around a line lying between these two.\*

The diagram shows that although there is a gain over complete ignorance, the result cannot be regarded as satisfactory, for there are many large errors. Correlations have to be extremely high before errors of prediction become small enough to be of use in predicting for individual cases.

The usual test of the significance of a correlation is designed to test whether the reduction in error variance by the regression can be

\* The regression line of  $r = 0.64$  is not drawn, to avoid making the diagram confusing. Negative correlations have not been considered.

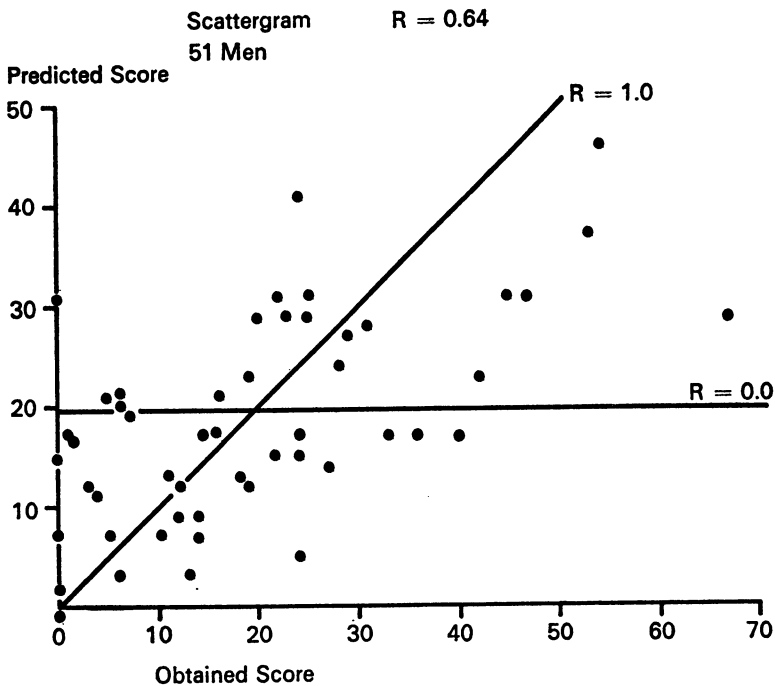


FIG. 1

regarded as significantly different from zero, and even the most trivial reduction will be significant if the sample be large enough. If a correlation is to be used for prediction, what is required is to reduce the error of prediction to as close to zero as possible. The only way to test this is to set up an arbitrary maximum error, and to test whether the obtained error differs significantly from it.

A good deal of research in clinical psychology in recent years has been devoted to devising tests which will distinguish between neurotic patients and normal subjects, or between neurotics and psychotics. Such tests can be combined to give a multiple correlation which is fairly high but usually below 0.7. If these tests are to be used for diagnostic purposes, for 'prediction', then it is clear that they are still far from being of practical value. The actual situation is much worse, however. To show whether the tests do differentiate between the various groups, it is customary to try them out on clearly defined groups of

'normals', neurotics and psychotics, but actually the tests will not be used on obvious psychotics to determine whether they are psychotic. To be of any practical use, they should be applicable to the doubtful cases, for if the errors are large with obvious psychotics, they will be even larger with the doubtful cases. Similar techniques have been used to predict recidivism in persons convicted of offences. Since this sort of prediction is about events literally in the future, even a small gain over complete ignorance is worth having, but it is not likely to make much practical difference.

So far only linear correlation,  $Y' = A_0 + A_1X$ , has been considered, and this includes the extension to multiple correlation  $Y' = A_0 + A_1X_1 + A_2X_2 + A_3X_3 + \dots$ . In psychology, the relation between the variables is usually represented adequately in this way, and where it is not, then some simple transformation, e.g. logarithmic or square root, will give linearity. Obviously, exceptions are bound to occur, and the non-linearity may be so marked that it cannot be ignored. Table I shows the relation between ratings of gastro-intestinal symptoms (running from none = 0 to severe loss of appetite and constipation = 4) and outcome of treatment of depressives by electro-shock. This is measured by rating the patients before and one month after the end of treatment, and expressing the latter as a percentage of the former. The statistical test shows that a simple linear regression is insufficient to show the relation between the variables, since the non-linear component is significant. The parabolic regression equation shows that linearity can be restored by putting in a 'new' variable,  $X^2$ . If it is intended to use this variable with others in a multiple regression formula, then two alternatives are available: either the original variable  $X$  is replaced by the transformed variable  $Y'$ , where  $Y' = 30.1 + 24.6X - 7.1X^2$ , or else both  $X$  and  $X^2$  are included as two variables with the others, in the correlation matrix used for calculating the final regression equation. There is no difficulty here, but if more than one of the predictors show non-linear relations with the dependent variable, the matrix (and the regression equation) rapidly becomes unwieldy with an accumulation of powers and cross-products of powers. From the statistical point of view this signifies nothing more than a laborious task in computing, but from the clinician's standpoint it may signify that disparate sub-classes are being lumped together in an overall approach. In such cases, it behoves the investigator to consider whether it would not be better to examine his material carefully to see if it might not be possible to distinguish such sub-classes and to treat them separately.

TABLE I

*Relation between Gastro-intestinal Symptoms and results of  
Treatment by Electro-shock*

*Male Depressives*

Rating on symptom	X	0	1	2	3	4	Total
Mean rating after treatment	Y	30.5	24.0	56.2	27.0	16.5	34.7
Number of cases		2	1	12	5	11	31

Correlation Coefficient	— 0.35
Linear Regression	$Y^1 = 61.0 - 9.7X$
Parabolic Regression	$Y^1 = 30.1 + 24.6X - 7.1X^2$

*Analysis of Variance*

Source	d.f.	S.S.	M.S.	F.	P.
Linear Regression	1	3,991.46	3,991.46	7.38	.01
Parabolic Component	1	13,566.29	13,566.29	25.10	< .001
Residual	28	15,136.19	540.58		
Total	30	32,693.94			

**SELECTION**

When it is desired to devise a prediction formula which will allocate patients to some form of treatment, e.g. operation in a surgical disease, or electro-shock in psychiatry, it is obviously impossible to use the standard procedure, i.e. to take a random sample of all patients, submit them to the treatment and then analyse the data. In practice, it is possible only to take those cases considered suitable, on clinical grounds, for the treatment. If the data on which the clinical judgement is based are highly efficient, then only those patients who will respond to treatment will be selected for it. In such a case, the correlations between predictors and outcome of treatment will be very small, and in any sample of reasonable size are likely to be non-significant. Thus good selection may give rise to low correlations but, obviously, low correlations may still signify nothing more than that the predictors selected are irrelevant. When correlations are small, it is useful to divide the sample into two halves, by random allocation of the cases, and to calculate the correlations separately. Then, if the correlations are in the same direction (both positive or both negative) it is legitimate to use a one-tailed test of significance. If the group is small, then another sample of cases will have to be obtained for this purpose.

A second sample will be necessary anyway. In the first place, another group of cases is required to see how much the correlation will drop from its first value. Methods have been devised for estimating the extent of the drop, but it needs to be confirmed empirically. This decrease in the correlation furnishes another reason why it is necessary for the correlation to be extremely high in the first place. In any case, if the prediction formula is not to remain a theoretical exercise, it must be used and tested on further cases. Here another difficulty appears, for it is implicit in the whole procedure that the new cases and the old are all sampled from one population. It is known that this is not true in clinical medicine.

It is notorious that after a rare disease is first described, it soon becomes apparent that it is not as rare as was first thought. The reason is that once the particular pattern of symptoms is described and becomes known, it becomes more and more readily recognized. Furthermore, the original description is usually, and in fact must be, based on extreme, severe, outstanding cases, which become the 'classical' type. Once the 'typical' syndrome is known, other cases can be recognized which are not of that ilk. In due course new and improved methods of diagnosis may appear, and therefore the new cases will resemble the classical type even less. Diphtheria and pulmonary tuberculosis are simple examples: once the causative organism had been discovered, it became possible to diagnose these diseases in patients who did not present the classical symptoms. A less obvious example is the finding that diabetic women gave birth to infants that were very much overweight. It was subsequently found that many women who were delivered of overweight infants later developed the symptoms of diabetes; they were in a 'pre-diabetic' condition.

The development of new and simpler methods of treatment also produces a change in the type of case seen. Thus, the discovery of anti-depressive drugs has meant that the general practitioner can treat without difficulty a large number of the patients he sees suffering from this disorder. More and more, the psychiatrist now tends to have referred to him only those cases who have failed to respond to the drugs or who are severely ill and suicidal. The consultant physician sees patients selected according to two different and changing criteria. In the first place, patients 'select' themselves for treatment: the development of Welfare Services relieves patients from the financial burden of treatment and they can seek it before they are desperate. They may learn that their disabilities are now treatable, or that the terrors of treat-

ment are less than the terrors of the disease. In the second place, the general practitioner may change his criteria for sending patients to a consultant: either new methods of treatment enable him to treat his patients himself (which reduces the number referred), or new methods of diagnosis and treatment appear (which increase the number). It may also be added that some diseases appear to undergo changes in the course of time: scarlet fever used to be a dangerous disease, but is now very mild; the classical type of depressive illness is now extremely rare. In predicting recidivism, it is likely that the increasing incidence of crime signifies a change in the 'disorder' or the 'population'.

For these reasons, when the prediction formula is tried out on a new group of cases, it is necessary to test whether the new group may be considered to be comparable with the old.

TABLE II  
*Means of Two Samples  
Male Depressives*

	<i>Age</i>	<i>Rating</i>	<i>Factor Scores</i>	<i>Weight</i>	<i>N.</i>
Sample 1	51.0	44.7	50.2	66.2	77
Sample 2	50.6	46.0	49.4	65.0	42

The problem is to decide which variables to choose for making comparisons. The most obvious choice is to examine those that are known to be related to the criterion which is being predicted. It is also advisable to test those variables (not necessarily correlated with the criterion) which are relevant to the classification of the subjects or the diagnosis. In Table II, comparisons are made between two groups of male depressives for age (for depression tends to occur more frequently with increasing age), on a rating score of severity of illness, on a general factor score derived from the ratings, and for weight, which has been found to be correlated with the outcome after treatment. Statistical analysis, by means of *t*-tests, showed that the differences between the means were non-significant; in fact, the groups are so alike that statistical testing is scarcely necessary. The two groups were then classified according to whether the illness had been precipitated by psychological stress (Reactive, *R*) or had developed without detectable stress (Endogenous, *E*) and into two doubtful groups (*DR* and *DE*). The distribution of cases in the two samples (Table III) is obviously very different:

TABLE III

*Types of Aetiology: Distribution*

	<i>E</i>	<i>DE</i>	<i>DR</i>	<i>R</i>	<i>Total</i>
Sample 1	16	12	15	34	77
Sample 2	23	5	6	8	42
Totals	39	17	21	42	119

$$\chi^2 = 15.1, d.f. = 3, .01 > P > .001.$$

It is notoriously difficult to obtain accurate information about psychological stresses, and it may well be that this is the cause of the different distributions in the two samples.

If the criterion for classifying the patients has changed in the course of time, this might be detected by examining the characteristics of the sub-types of depression which showed differences between them. For if some patients previously classified in one category were now to be found in another, then the characteristics of the types would change from one group to the next. Since endogenous depression tends to occur at a later age than reactive depression, then the mean ages could be compared.

TABLE IV

*Means of Groups: Age*

	<i>D</i>	<i>DE</i>	<i>DR</i>	<i>R</i>	<i>Total</i>
Sample 1	53.6	54.4	48.9	49.6	51.0
Sample 2	51.7	48.0	45.3	53.2	50.6

This has been done in Table IV, and statistical tests confirm what is obvious: that the mean age of the sub-types is about the same in the two samples. The inference from this is that, there being no evidence that the sub-types differ in the two samples, there has probably been a real change in the distribution of cases. This is made plausible by the fact that the first sample was obtained before the arrival of the anti-depressive drugs, and the second sample after these drugs had come to be used extensively. Nevertheless, when comparisons are made of the ratings for severity of illness, the type that has diminished in frequency, (the *DE* type) is now seen to be significantly milder in the second group

than in the first. If it is considered that the change in the distributions is due to the 'selection' arising from the use of drugs, it would imply that the severer cases in the *DE* type respond to drug treatment, but not those in the other types. There is no evidence available to confirm this, and to test this hypothesis would be very difficult indeed (Table V).

TABLE V

*Means of Groups: Rating*

	<i>D</i>	<i>DE</i>	<i>DR</i>	<i>R</i>	<i>Total</i>
Sample 1	51·8	52·6	39·1	41·1	44·7
Sample 2	50·9	34·4*	42·2	41·6	46·0

If sufficient variables are tested, then some will show significant differences, but it is impracticable to test enough variables to make it plausible, or otherwise, that the differences are due to chance. In any case, the tests of significance are not independent. As a matter of interest, the second sample did much better after treatment than the first, which suggests that the two samples are not strictly comparable, i.e. cannot be regarded as having been drawn from the same 'population'.

It must be concluded that there are many pitfalls in the practical application of the theoretical methods of prediction. It is necessary to move slowly and to test each step. It is necessary, too, to be aware of the assumptions underlying the theory of prediction and to make sure that practical procedures are in conformity with them.

**SUMMARY**

A simple outline of the theory of statistical prediction has been given, and some of the practical problems met when applying the theory in a medical field have been described. It has been shown that these problems are very difficult to deal with, and this probably goes far to explain the paucity of work on prediction in the field of clinical research.



*J. A. Fraser Roberts and Elizabeth Sedgley*

## **Intelligence Testing of Full-term and Premature Children by Repeated Assessments**

The plan of this investigation was to make observations on groups of full-term and premature babies during the first weeks of life, and then to administer tests at intervals up to the age of seven years, first the Griffiths scale and then the Terman-Merrill Revision of the Binet Scale. It is hoped to describe the neonatal observations later, and also to examine the premature group in more detail. In this paper attention is confined to the test results, their means, their standard deviations and inter-correlations. Consequently the figures for the premature group are given in total only, prematurity having been assessed by the conventional criterion of a weight at birth of less than  $5\frac{1}{2}$  lb.

It is of particular interest to make comparisons with the findings of Hindley,<sup>1</sup> in a comparable study on the Griffiths scale over a range of 3 to 18 months.

### **SELECTION OF THE SAMPLES**

The children were born at Southmead Hospital, where a high proportion of births in the City and County of Bristol take place. They were chosen during the first days of life from infants judged to be viable, those with gross deformities being excluded. The numbers were limited by the capacity of one worker to make the observations. Where possible those whose homes were in or near the City were chosen. Otherwise there was no selection of cases, and it is hoped that these children represent a random sample of full-term and premature children born at the hospital. A few children were inevitably lost to the investigation owing to removal of the family to distant areas. Finally, those followed up comprised 54 children in each group.

## THE TIMING AND ADMINISTRATION OF THE TESTS

It was, of course, impossible to test all the children at rigidly pre-determined intervals, so that some spread of chronological ages at testing was inevitable. Thus, with the Griffiths scale somewhat broad intervals have had to be used—they are as follows:

- 3 months. (At this age the children were in fact tested quite close to the target chronological age.)
- 6-9 months.
- 12-15 months.
- 18-21 months.

It was intended to test the children with the Terman-Merrill scale as soon as possible after they attained the ages of 2, 3, 4, 5 and 7 years. Again some spread was inevitable; with little variation the average chronological age was about  $2\frac{1}{2}$  months in excess of these ages.

All tests were given by one of us (E.S.) with the exception of three seven-year Terman-Merrill tests which were kindly carried out by Mrs. M. M. L. Dickinson, Educational Psychologist at the Child Guidance Centre, Weston-super-Mare. Up to school age the testing was carried out in the homes, and thereafter at the schools.

In making the comparisons described in this paper a difficulty arises owing to the inevitable overlap of the two scales, bright children reaching the ceiling of the Griffiths scale before the age of two years, and some dull children having to be scored on the Griffiths scale instead of the Terman-Merrill scale at the age of 2+ years. This has meant sacrificing some later Griffiths tests, and also basing the Terman-Merrill tests at 2+ years on a rather smaller number, as is described below.

## THE COMPLETENESS OF THE TESTING

With the full-term children all the Griffiths tests at the ages shown were completed. One child missed the 5+ year test with the Terman-Merrill scale (his intelligence quotient at seven years was 99). Two children could not be scored on that scale at 2+ years. Their Griffiths quotient at that time were 88 and 92, and their final intelligence quotients at age 7+ were 108 and 110.

With the premature group all the Griffiths tests were completed. There were some losses later, however. They are as follows:

- at age 2+, 1 child missed. I.Q. at 7+ : 129.
- at age 3+, 1 child missed. I.Q. at 7+ : 137.

at age 4+, 4 children missed. I.Q.'s at 7+: 107, 112, 129, 137; mean, 121.3.

at age 7+, 7 children missed. I.Q.'s at 5+: 97, 100, 101, 103, 109, 112, 114; mean 105.1.

Seven children were unable to complete a satisfactory Terman-Merrill test at 2+. Their Griffiths quotients at that age were 59, 67, 79, 90, 93, 94, 96, with a mean of 82.6; and their final I.Q.s at age 7+ were 69, 89, 96, 102, 102, 108, 134, with a mean of 100.0.

These omissions, particularly those at 2+, must be borne in mind when considering some of the results, though the effect on the figures shown should not be serious. It follows that the means, standard deviations and inter-correlations in Tables I-V are sometimes based on numbers smaller than the full 54 comprising each group.

#### THE MEANS

The means of the results for both scales are shown in Table I. These

TABLE I

*Means and Standard Errors of Intelligence Quotients*

C.A.	Full-term Group		Premature Group	
	Mean	S.E.	Mean	S.E.
<i>Griffiths</i>				
3 months	103.3	1.32	81.9	1.63
6-9 "	106.1	1.36	87.9	1.46
12-15 "	106.5	1.12	89.2	1.49
18-21 "	102.6	1.00	89.0	1.51
<i>Terman-Merrill</i>				
2+ years	103.8	1.90	92.0	1.48
3+ "	105.7	2.35	94.5	2.14
4+ "	105.2	2.35	96.7	2.37
5+ "	106.0	2.10	100.1	2.07
7 "	105.9	2.06	100.0	2.49

seem satisfactory. It is well known that the mean Terman-Merrill intelligence quotients of random samples of English children exceeds 100, usually by some five points or so, which is just the result yielded by the full-term group. It is also to be noted that the mean Griffiths quotients are very similar to the subsequent mean intelligence

quotients. In fact there are only two significant differences amongst the 36 possible comparisons. These are between the Griffiths quotient at age 18-21 months and that at 6-9 months, which is barely significant at the five per cent level, and between the Griffiths quotients at 18-21 months and at 12-15 months. This is entirely due to the low figure at 18-21 months. Hindley<sup>1</sup> reports exactly the same result at 18 months; apparently the scale is a little too difficult at about this age, compared with other parts of the range.

The Griffiths and Terman intelligence quotients for the premature group show what might be expected, namely, a steady rise until a ceiling is reached. This has been attained by five years. Thus, remembering the nature of the premature group, defined simply by a birth-weight of less than 5½ lb., premature children start, compared with full-term children, at a disadvantage of a little more than 20 points. The gap narrows steadily, however, until it stabilizes at about six points at age five.

The same conclusion about the slightly lower mean Griffiths quotient at 18-21 months emerges clearly. The steady rise is temporarily halted, which reinforces the evidence from the full-term group and from Hindley's results.

Of course, with numbers such as these the size of the final gap between full-term and premature children cannot be determined at all pre-

TABLE II  
*Comparison of Full-term and Premature Groups.  
Distribution of Intelligence Quotients at 5+ Years*

I.Q.	Full-term Group	Premature Group
50-59		1
60-69	1	3
70-79	3	1
80-89	4	3
90-99	2	12
100-109	23*	23
110-119	14	7
120-129	5	4
130-139		
140-149	2	
	—	—
	54	54

\* One child missed at 5 years, 7-year I.Q. substituted.

cisely. In fact the difference is not quite significant at the five per cent level, either at five or at seven years. In a recent paper Miall and Oldham<sup>2</sup> give an interesting discussion of the treatment of measurements repeated after an interval. They were dealing with arterial blood pressures, but the problems are analogous. Following their treatment, we can take the mean of the measurements for each subject at five and at seven years (for those for whom both measurements are available). This reduces the error variance, with the result that the difference is now significant. It becomes 6.38 points, with a standard error of 3.08; thus the difference is 2.07 times its standard error.

Table II shows that the final average difference between full-term children and prematures (as here defined) is not due to an excess of very low values in the latter group. It is clearly almost entirely due to a shifting of the whole curve. In this table the five-year results are shown, as they are more complete than the seven-year results. In fact they are complete, except for one missing child, for whom the seven-year result has been substituted.

#### THE STANDARD DEVIATIONS

The standard deviations are shown in Table III. There does not seem

TABLE III

*Standard Deviations of Intelligence Quotients*

<i>Age</i>	<i>Full-term Group</i>	<i>Premature Group</i>
<i>Griffiths</i>		
3 months	9.72	12.00
6-9 "	9.96	10.76
12-15 "	8.21	10.93
18-21 "	7.37	11.11
<i>Terman-Merrill</i>		
2+ years	13.63	10.06
3+ "	17.26	15.55
4+ "	17.24	16.74
5+ "	15.27	15.21
7+ "	15.15	17.07

to be much to say about this table. In general the dispersion on the Griffiths scale is considerably narrower than on the Terman-Merrill;

perhaps this is inevitable. The Binet standard deviations at 2+ years are also low. The standard deviations for the Griffiths scale seem satisfactorily uniform, bearing in mind the small numbers of subjects. They are very similar to those found by Hindley.

#### THE INTER-CORRELATIONS

The inter-correlations for the full-term and premature groups respectively are shown in Table IV. The full symmetrical table is filled in to

TABLE IV  
*Inter-correlations between Successive Tests*  
*Upper figures Full-term Group;*  
*Lower figures, in italics, Premature Group*  
*Chronological Age*  
*Griffiths (months)*                      *Terman-Merrill (years)*

		<i>Griffiths (months)</i>				<i>Terman-Merrill (years)</i>					
		3	6-9	12-15	18-21	2+	3+	4+	5+	7+	
<i>Griffiths</i>	3		.11	— .06	.03	.16	.10	.04	— .05	.00	
	6-9		.44	.40	.22	.15	.11	.05	.11	.16	
	12-15		.11	.48	.54	.54	.30	.36	.27	.22	
	18-21		.44	.71	.62	.47	.34	.26	.32	.26	
			.06	.48	.75	.62	.61	.55	.47	.39	
<i>Terman-Merrill</i>	2+		.03	.54	.75	.67	.63	.60	.50	.49	
	3+		.22	.62	.81	.75	.64	.43	.41	.29	
	4+		.16	.54	.62	.67		.68	.63	.54	.47
	5+		.15	.47	.62	.75		.77	.68	.62	.51
	7+		.10	.30	.61	.63	.68		.81	.72	.66
			.11	.34	.58	.64	.77		.82	.75	.65
			.04	.36	.55	.60	.63	.81		.84	.84
			.05	.26	.43	.43	.68	.82		.84	.73
			.05	.27	.47	.50	.54	.72	.84		.86
			.11	.32	.36	.41	.62	.75	.84		.83
			.00	.22	.39	.49	.47	.66	.84	.86	
		.16	.26	.24	.29	.51	.65	.73	.83		

As explained in the text, totals are sometimes less than the full number of 54.

Correlations exceeding about .27 are significant at the 5 per cent level, and those exceeding about .35 at the 1 per cent level.

facilitate examination. It is probably unsafe to pool the individual results to obtain a combined single table. We have, therefore, averaged the correlations using Fisher's  $z$ -transformation. The results are shown in Table V.

The lessons of these tables are plain. There is a remarkably uniform progression. The magnitude of the correlations is a function first of age, and secondly of the interval between the tests. In other words, the accuracy of prediction increases steadily with age and decreases steadily as the interval between tests increases.

The inter-correlations for the Griffiths scale are broadly similar to those found by Hindley.<sup>1</sup> His inter-correlations of the three-month tests with those at 6, 12 and 18 months are somewhat higher than ours, and it may well be that Dr. Hindley and Mr. Moore (who carried out the testing) were more successful in applying the scale at that early age. Our inter-correlations at later ages, however, are appreciably, though probably not significantly, higher.

Hindley found lower correlations with the 12-month tests. This does not appear in our figures.

TABLE V  
Combined Inter-correlations, both Groups

		Griffiths (months)				Terman-Merrill (years)				
		3	6-9	12-15	18-21	2 +	3 +	4 +	5 +	7 +
Griffiths (months)	3		.280	.179	.127	.154	.104	.044	.028	.075-
	6-9	.280		.606	.583	.509	.323	.315 +	.297	.239
	12-15	.179	.606		.779	.618	.593	.493	.416	.322
	18-21	.127	.583	.779		.707	.635 +	.524	.459	.404
T.M. (year)	2 +						.724	.651	.580	.489
	3 +					.724		.815 +	.735 -	.659
	4 +					.651	.815 +		.841	.801
	5 +					.580	.735 -	.841		.848
	7 +					.489	.659	.801	.848	

As explained in the text totals are sometimes less than the full number of 108. Correlations exceeding about .19 are significant at the 5 per cent level and those exceeding about .25 at the 1 per cent level.

## DISCUSSION

What impresses us most about the results is the stability of the means. This is shown by the remarkable constancy of the full-term group, even from three months. It is also shown by the smooth progression of the premature group, which steadily narrows the gap between the premature and the full-term children. Even if the ultimate predictive

Mean G.Q. and then I.Q. for full-term and premature groups of children tested at intervals from 3 months to 7+ years

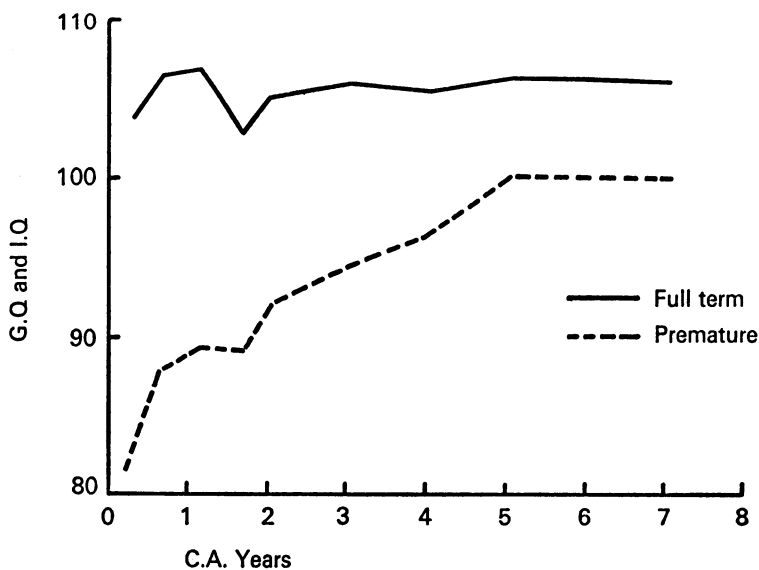


FIG. 1

value of tests for very young children tends to be low, there would seem to be good reasons for confidence in applying the tests when what is wanted is the comparison of large groups. This is well illustrated by the preliminary comparison of the full-term and premature groups. When it is important to compare groups at very young ages, and it often must be, the Griffiths scale seems to be an excellent instrument for the purpose. For example, it could be used to extrapolate backwards in studies on twins.

Hindley (whom we have quoted repeatedly) gives a brief but valuable review of the reasons why infant tests should show almost universally such relatively low predictive values. It seems to us that the most likely



explanation is that it is an inevitable concomitant of the growth process itself. Thus, it has been shown that with physical measurements, where questions of reliability and validity hardly obtain, the same phenomenon occurs. For example, Tanner *et al.*<sup>3</sup> found very poor predictive values for physical measurements made before the age of 12 months.

A rather striking feature of the tables showing inter-correlations is the smooth transition from the Griffiths to the Terman-Merrill scale. For example, there is no sign of any sudden improvement on passing to the Terman-Merrill test at 2+ years. The Griffiths scale appears to be working just as efficiently, allowing for the inevitable improvement in predictive value as age increases. In other words, allowing for this factor, the Griffiths scale correlates as well with the Terman-Merrill as the latter does with itself. It seems to us impressive that the correlation between the Griffiths quotients at 12-15 months should be as high as .62 with the Terman-Merrill 2+ year tests, .49 with the Terman-Merrill scale at 4+ years and .32 at 7+ years. Similarly, the Griffiths quotients for the 18-21 month children give correlations with Terman-Merrill of .64 at 3+ years and .40 at 7+ years.

#### SUMMARY

1. 54 full-term children and 54 premature children (birth weight less than  $5\frac{1}{2}$  lb.) were tested at intervals, first with the Griffiths scale up to the age of two years, then with the Terman-Merrill scale up to 7+ years.
2. The mean Griffiths and Terman-Merrill quotients of the full-term group were remarkably constant throughout, being about 106. The mean given by the Griffiths scale for the premature group was 82 at three months, then it increased steadily to an I.Q. of 100 at 5+ years. There was no further rise at 7+ years.
3. The final difference between the full-term and premature groups was not due to an excess of low values in the former group; it was due to a shift of the whole curve.
4. The stability of the full-term means, even from three months, and the steady progression of the premature means, show that even if the reliability of the test results is rather low at the younger ages the tests can still be very useful for the comparison of groups of subjects.
5. The inter-correlations improve steadily with increasing age and decrease steadily with increasing intervals between tests. There is no break between the Griffiths and Terman-Merrill inter-correlations.

Bearing in mind the increasing reliability as chronological age advances, the Griffiths scale correlates as well with the Terman-Merrill as that scale does with itself.

6. It seems likely that the relatively low predictive value of mental tests at very young ages is a consequence of the growth process itself, for it is also characteristic of physical measurements.

### *Acknowledgements*

We are greatly indebted to the Bristol Education Committee for permission to visit all schools within its authority; to the School Medical Department for the most efficient and willing help over a number of years; and to the Heads of Schools for interest and unfailing courtesy.

To Dr. Beryl Corner and the Staff of Southmead Hospital we express our thanks for co-operation in the work done in the wards; and to Professor A. V. Neale for his support and help in many directions.

We also wish especially to record our appreciation of the generosity of Dr. Ruth Griffiths who, in allowing the use of her Developmental Scale for Children at the early stages, provided a basic tool for the work that has been done.

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*Gertrude Keir*

## **The Psychological Assessment of the Children from the Island of Tristan da Cunha**

### *Introduction*

In the autumn of 1961 the volcano forming the central core of the Island of Tristan da Cunha erupted. After several days of anxiety the two hundred and sixty-four Islanders were forced to abandon their homes, their animals and their fields. By way of South Africa they finally came to England and were taken first to Pendle Camp in Surrey where they remained in conditions of some discomfort for about three months. They were eventually found homes in an R.A.F. camp near Southampton.

The contrast between the two environments—England and Tristan da Cunha—could hardly have been greater. An article by Mr. Wheeler, the Island Administrator, in the *Geographical Magazine*,<sup>1</sup> gives a vivid account of life on the Island.

‘When I arrived on Tristan, I found a land bypassed by the twentieth century. Huddled in a single village on a plateau overlooking the sea, the thatched houses resembled Scottish crofters’ cottages of the Western Isles. The way of life, simple and severe, had changed little in 150 years. The Islanders knew of motor-cars and television only from pictures. Their language is old-fashioned English with local quirks. “How you is?” queries the Islander. “I’s fine,” you answer.’

There are various sources of information about the way of life, the characteristics of the Islanders and the island itself. Over a fairly long period outsiders have gone there for longer or shorter stays—the doctor, the clergyman, the teacher, the agricultural officer, the radio officer, the Administrator—and reports in various forms have been available, not infrequently in published form. Information on recent conditions in the Island will be found in the short but informative article by Samuels,<sup>2</sup> medical officer on Tristan da Cunha for six months prior to the

evacuation. The only large-scale scientific research, however, has been that of the Norwegian Expedition of 1937-38<sup>3</sup> which reported its findings in detail.

Noteworthy features of this particular environment appear to be:

1. Its extreme geographical isolation. Tristan da Cunha is the largest and the only inhabited one of five small islands of volcanic origin, lying half-way between South America and Africa. Its nearest neighbour is St. Helena, about 1,700 miles away. Cape Town lies some 2,000 miles to the west, while the nearest shipping route is about 200 miles to the north. In the old days of sailing ships the Island used to be visited regularly, as it lay in the path of the Trade Winds. For many years past, however, only three or four ships a year had visited it, to bring supplies. Recently, with the setting up of a canning factory on the island, regular communication with South Africa has been established.

2. The smallness of the Island. It consists of a huge volcanic cone rising steeply out of the ocean to a height of some 6,000-7,000 feet. There is some littoral round it and a flat plain at one side, where the township was built, but the island measures only some 7-10 miles across.

It provides therefore an isolated and restricted environment which, as might be imagined, appears to have fostered a narrow and restricted outlook on the part of its inhabitants. They apparently have little interest in the distant world, a noticeable feature of their general outlook when they arrived in England.

3. The way of life on the Island. This consists mainly of small crofting activities, a few animals, some crops, fishing. Just after the war a canning factory was set up by the newly-formed Island Development Company, but it is, of course, a small one. This was the Islanders' only glimpse of modern industrial conditions before they came to England. Such a way of life, resembling very strongly that, for example, of the Western Isles of Scotland, tends to produce certain habits and attitudes of which only one or two can be mentioned here. Though the members of the community may be interdependent, the independent activities of the individual families are maintained. While fishing may be a corporate enterprise the families themselves work their crofts, the children helping them when practicable. This individual work and responsibility was contrasted by the Islanders with conditions in England, summarized in the statement given many times that 'here, you're not your own boss'.

The sense of time, too, becomes geared to natural events and not (as in England) to the hours of a working day laid down by others, and extended for most by the journey to work, which often resulted in such long hours away from home as 6.15 a.m. to 7.30 p.m. Many of them found it difficult to modify their concepts of time, though in practice they did adapt to the changed working hours. They found them a strain, however, and disliked them.

On Tristan da Cunha, where natural and often unmodifiable conditions determined so much of their working activity, feelings of initiative were unlikely to be stimulated. The actual stresses of living tended to lower energy, in particular the stresses resulting from the incessant struggle to maintain life where the soil is poor, the areas for cultivation small, and the climate at times hostile. The impetus of competition, while not unknown, seems to have been largely absent; and thus the majority of Islanders were unprepared for the translation to a highly-organized competitive society.

This general way of living affected their outlook towards money and the goods it could buy. During their period in Britain both children and adults spoke frequently of the fact that money was so necessary here, contrasting this with conditions on Tristan where so much could be obtained without money. In fact, they had over a period of many years received a considerable amount of assistance in their endeavours to maintain themselves, but they had been bred up to regard this as a natural feature of their way of life. Their thinking on this point remained unrealistic all the time they were in England. The fact that they enjoyed very different shopping facilities here and realized the substantial rewards which money could bring them, co-existed side by side with two beliefs: that things were all round much better on Tristan and that goods could somehow be obtained there without money, even those which they agreed in discussion could only be procured with it.

The lack of competition and their acceptance of themselves as they were resulted, on the positive side, in a relative freedom from anxiety, noticeable in the school children, and in the strikingly tranquil atmosphere in the homes when the pre-school children were being tested. They were usually surrounded by a group of interested adults whose presence however did not seem to produce any tension in the children—an atmosphere very different from that in most British homes under the same circumstances.

#### 4. The close blood relationship of the Island people.

The present inhabitants are descended from eight men and seven women who settled there at different times during the nineteenth and early twentieth centuries. A community was first established (in continuity) in 1816, though visits had been paid to Tristan da Cunha and whaling and other parties had stayed there from time to time before that. During the Napoleonic war it had been a British naval garrison and when this left a Corporal William Glass asked permission to stay behind, together with his wife, a Cape coloured girl, and their baby son. A very full history of the comings and goings of the early settlers will be found in the report of the Norwegian Expedition.<sup>3</sup> Suffice it to say that Corporal Glass is regarded as the founder of the community, and that the family names are those of the seven men who joined it at varying times from 1821 to 1908. There are thus only eight names—Glass, Cotton (of whom there are now no named descendants), Swain, Green, Rogers, Hagan, Repetto and Lavarello. Their countries of origin were somewhat diverse—Scotland (Glass), England (Cotton and Swain), Holland (Green, Groen), America (Rogers and Hagan), Italy (Repetto and Lavarello). The men intermarried with coloured women from St. Helena, a Cape woman and two Irish women, one of them Cape born. These marriages and the intermarriages among the small population have led to several different kinds of physique. Three types can broadly be distinguished by their colouring: a fair type with blue eyes and a white skin; an Italian type with dark eyes, dark hair and olive skin; and a coloured type with dark eyes, frizzy hair and dark skin. Although the Islanders differ in this way, they are, however, highly conscious of their own blood kinship and to the outside world they speak of themselves as one family, whatever their individual differences and disagreements may be. Thus, though there may be as many as 40 people from the outside world resident at any one time on the Island, the Islanders tend to regard them as outsiders in contrast to their own community. As can be imagined, many consequences follow from this closeness of kinship. In all likelihood it operates in conjunction with isolation, restricted opportunity and climatic conditions in heightening inner resistance to suggestions and advice from outside the community. It is true that war-time conditions brought enforced changes to which they had to adapt, and which on the whole they seemed to have liked; but the fresh adaptations required by life in England were demanded of them at a time when the com-

munity was threatened with dissolution. Nevertheless, many attitudes, ways of thinking and practices had to be altered if they were to become integrated into their new environment. What was their new environment like?

1. The site, an R.A.F. camp at Fawley, near Southampton—was chosen primarily because it contained many vacant dwellings. All the Tristans could therefore be accommodated in the same place. This was an advantage, and probably one of the few. Although the houses themselves were for the most part reasonably good, and were furnished for the Tristans by the kindness of many interested people, the camp itself was far from homelike and the type of community it provided was an uncomfortable sort of one. There was no real community into which the Tristans could be integrated, nor, on the other hand, were they sufficiently on their own to function as a self-contained group.

The general neighbourhood is one of fast expanding technology centred round Esso and its subsidiary companies. Few of the original New Forest people remain in that particular area: new estates housing people from all parts of Britain proliferate for miles and the whole aspect is straggling and lacks structure. The schools contained many children whose parents had moved into the area recently, while the camp itself for the first year or so contained that special type of community known as 'headless families'—wives and children of R.A.F. personnel waiting to join them elsewhere. This general family mobility produced a rather restless attitude, particularly among the R.A.F. children.

The society was also socially mobile, the families being attracted south by the opening up of technological jobs, and there was a certain amount of emphasis on material rewards and social status. Much kindness was shown to the Islanders and after a while some made local friends, but differences in dress, appearance and way of life were to some extent highlighted by the social climate around them. To give a simple instance, very few Tristan adults and not many of the children ever went near the beach, although this was not far from the camp—and the long boat brought from Tristan was never launched. The real explanation for this seemed to be that the local inhabitants who hired the beach huts, a sign of social prestige, did not welcome their presence.

2. The British climate proved trying for nearly all the Islanders. The two winters spent in this country were both severe. The camp

itself is very exposed, while conditions at Pendle had also been difficult. Coughs and colds were prevalent all the time, even in summer. Some Islanders died from pneumonia at the outset of their stay, and their loss was keenly felt. Details of health and illness, both on and off the Island, will be found in Samuels<sup>2</sup> and in a recent report on medical work carried out as part of the current large-scale investigation of the Islanders.

3. Space forbids more than a very brief mention of the considerable adjustment to work required of the adults, to which reference has already been made. The employment officers and the local employers showed interest, understanding and kindness, and were rewarded for their efforts in a very good work record on the part of the Islanders. A number of men found employment similar to what they had known on the Island, for example, some of the younger ones obtained work with the shipping lines, usually as deck hands, while one or two found jobs in local boat-building firms. Most, however, had to go into relatively unfamiliar kinds of jobs, usually not very highly skilled. The men contrasted employment on the Island with that in Britain to the disadvantage of the latter, but in spite of this they were reckoned to be good workers, willing and conscientious, and they were proud of this. At the same time they found the hours long; and they were very conscious of not knowing how to deal with the various ramifications of a modern industrial society—Union Cards, Unemployment Benefit, National Insurance, tax affairs and the like.

4. The children had their own problems. These may be summarized briefly as follows:

- a. The journey to school and separation from home and from their brothers and sisters, where differences in age made this inevitable.
- b. The change to much larger classes and schools.
- c. The adaptation to different teachers and to strange children.
- d. The differences in curricula and teaching methods.

a. The journey to school meant, for the infants especially, their first separation from home, doubly trying in a strange country. It was unfortunate that the first two classes of the infant school (attended in 1962 by all those aged 5-7½) were housed in an annexe about a mile away from the camp and two miles distant from the rest of the primary school. This isolated the youngest children from their



elder brothers and sisters, as well as from their mothers, and the double separation was reflected in consistent shyness, withdrawal and extreme dependence upon the eldest child in their age group, attitudes still in evidence months after their first attendance at the end of January 1962.

This contrast with life in the single village school on the Island was heightened for all children by the fact that while the juniors went on to the primary school some two miles away, those over eleven plus were taken a further distance of two miles to the secondary modern school. All the age groups, infants, juniors and seniors, were thus separated.

b. The schools and the numbers of classes were much larger than on the Island. For example there were 75 children in two classes in the infant annexe (39 infants were in the top infant class housed in the junior school), 214 children in six classes in the junior school and 576 children in 21 classes in the secondary school. The school on Tristan consisted only of some 40 children taught in small groups.

How did the children fare in these changed conditions? The junior school was able to create for them the best possible conditions by assigning them to two backward classes, the lower one catering for the backward children in the first two years, the second backward class catering for the third- and fourth-year children. From the first the majority of the Island children were placed in one or other of these classes, and eventually only one girl remained in the normal class. Conditions were somewhat less favourable in the secondary school. The 18 children were distributed among eight different classes, being placed, with one exception, in the third and fourth streams of their years. Several found themselves alone, facing classes of some thirty English children.

c. Unlike those in the primary school, who remained with the same teacher for all subjects except needlework and handicraft, the older children had to cope with the organization of the secondary modern school, where specialist teaching resulted in frequent changes during the day from teacher to teacher. Thus efforts of adjustment to many different people were required of them from the start. This contrasted with conditions in the Island school, where the single teacher in charge was assisted by two Island girls and from time to time by the chaplain. The elder girls particularly had a difficult time meeting *en bloc* the English girls of their own ages. They had to be got out

of their own clothes and into school uniform, which at first they resisted. For some time, too, they found it hard to change into the clothes for Physical Education (especially as the conditions for changing were poor), the Island habits of personal modesty providing a source of conflict. Their hair styles were different from those of the British girls, their pronunciation and idioms of speech were different and all in all they felt very much out of things. They were, in some respects, fortunate in having with them, attached to the school for the purpose, a teacher who had formerly been with them on the Island. She remained in the school until the end of July, staying in the camp, travelling on the bus with them and available for advice and guidance at all times. Nevertheless, the day-to-day stresses in school had to be met by the girls themselves.

The attitudes of the other children to the Islanders were at the start very favourable, especially in the junior school. The same is probably true of those in the secondary school, though it is more difficult to judge, since the large numbers of children and of classes precluded the close study and knowledge which the junior school afforded.

d. The curriculum also presented special problems, especially for the older children. These will be referred to later in the discussion of general educational progress. Among these problems was that of learning many subjects which were quite unfamiliar to the Tristan children, and taught, moreover, by different specialist teachers. Even those in the junior school felt somewhat at sea and relied a good deal upon each other for surreptitious help, which might have been called cheating if it had not been so open and naïve.

This brief summary gives a very general outline of some of the conditions which the Islanders met and which they were to find particularly difficult during the first six months of their stay.

### *The Present Study*

Just before the Islanders moved to Pendle Camp the Department of Psychology at University College, London, was asked to interest itself in the psychological assessment of the children of school age. The study, as originally planned, had two main aims:

1. To study selected characteristics of the children as soon as possible before adaptation took place, in order to evaluate the

effects of a small closed and isolated community upon the psychological development of its children.

2. By means of repeated assessments during the first two years of their life in England to observe modifications of behaviour that might be due primarily to the changed environment.

The characteristics to be investigated were as follows:

- a. General intelligence, specific abilities and disabilities.

The literature and personal opinions of those who know the Islanders present somewhat conflicting evidence as to their level of intelligence and specific aptitudes. However, previous psychological studies of intelligence in isolated communities provide evidence suggesting that social and geographical isolation and reduced opportunities such as were found on Tristan da Cunha are likely to result in lowered intellectual functioning.

- b. Personality, family and social attitudes and social interactions. These were likely to be of interest and to form suitable subjects for investigation.

### *Methods of Research*

The whole investigation has had to be carried out *ad hoc*. It was essential to study the children as soon as possible after they landed in this country, yet at the time when the project was first mooted nearly three months had already elapsed. Fortunately the children had remained fairly well segregated from British children and had not attended school. They were first seen for a brief visit only two days after they went to their schools at Fawley. But the project at that time had not yet been initiated officially, and two months were to elapse before it was approved and testing could start. When it was possible to begin in earnest, time was short and only techniques of which something was already known could be employed. Any planning and trying out of tests in an experimental fashion was not feasible. We had to make do with what was available: a state of affairs that is always somewhat unsatisfactory, particularly so when applied to a community such as this, of which we knew so little. In some respects it was a particularly difficult population to attempt to assess. The Island community had its beginnings in nineteenth-century stock and the development of the community was strongly influenced by the British and American settlers. The language is English, but with its own variations of pronunciation and idiom, sometimes recognizable as early nineteenth-

century vintage. Links with Britain have been stronger than with any other country: the various people in special posts on the Island have for the most part come from this country; the education of the children was carried out until 1958 by the chaplain, and subsequently by British teachers sent by the Society for the Propagation of the Gospel. During the Second World War a British naval garrison was stationed on Tristan—hence the Island people have had the opportunity of contact with British men, women and children.\* On the other hand they have been brought up in a way of life of which only a few examples survive in this country today in very isolated rural communities. Tests suitable for British children were likely to be inadequate, and there was no time to prepare any which might be more appropriate.

The main techniques used were as follows:

1. Objective, standardized tests of intelligence and educational attainment, and of attitudes and personality.
2. Observation of behaviour.
3. Ratings and comments from class teachers.
4. Interviews.

### *Sample*

In March 1962 when the field work began there were 40 children of school age in the Island community. These were distributed as follows:

	<i>Total</i>	<i>Boys</i>	<i>Girls</i>
<i>Attending Infant School</i>			
Ages 5 years 8 months–7 years 3 months	7	3	4†
<i>Attending Junior School</i>			
Ages 7 years 7 months–11 years 4 months	15	5	10‡
<i>Attending Secondary Modern School</i>			
Ages 11 years 9 months–15 years	18	11	7
Total	40	19	21

These 40 children were tested individually in March; the four who came to the school later were tested in July, when further assessments

\* A regular school was established on the Island as a result.

† Three more girls joined in May and were present for the July testing.

‡ A girl of 10 joined in May, coming from the Falkland Islands—present in July.

were made for all the group. However, the total number of children tested was very much larger. In order to see how the Tristan children settled down in their new schools and adjusted themselves both educationally and socially, it was necessary to make a study of the school children among whom they found themselves. It would not, for example, be very productive to find that the scores of the Tristan children in certain tests of, say, educational attainment, altered from one year to another, unless one were also able to estimate the amount of change in the performance of the other children in their classes. Hence, what might at first sight appear a considerable but nevertheless not overwhelming task, namely, the assessment of less than 50 children, became a task of much greater magnitude when to this was added the testing of intelligence, educational attainments and attitudes of the non-Tristan children in the two schools, one of some 250 children and the other of over 550. It was obvious that group tests would have to be employed for this, with all their attendant drawbacks. Their use, however, had one distinct advantage. During the discussions which preceded the initiation of the research, it had been feared that testing the children from Tristan da Cunha would serve to attract the attention of their classmates to them and thus make them feel singled out for special and possibly unwelcome attention. To test the whole school first would not only facilitate the comparisons that we wished to make, but would have the useful effect of making the Islanders feel that the others were being tested too.

### TESTS USED

	<i>Intelligence</i>	<i>Attainment</i>	<i>Personality</i>
<i>Infants</i>	Kelvin	Southgate Reading	
<i>Group</i>	Draw a man: house: tree	Test	
<i>Individual</i>	S-B, Form L Porteus Maze		Rorschach 3 wishes
TRISTANS ONLY	Rigney Classification Test (Bruner)* Piaget (conservation, etc.) Harris laterality test		

\* We are indebted to the kindness of Professor Bruner and Miss Joan C. Rigney, Center for Cognitive Studies, Harvard, for the loan of the pictures for the classification test which we modified to suit British conditions, and for the loan of the thesis in which results of its use with American children are contained.

<i>Junior</i>			
<i>Group</i>	Moray House P.T.2 (7-8 years)	Vocabulary English Progress B2	Sentence Completion
All Children (depending on age)	Primary Verbal 2 (N.F.E.R.)	<i>Reading:</i> Schonell A or Gates Prim.	3 wishes Sociometric choice
	Non-verbal 5 (N.F.E.R.)	<i>Arithmetic:</i> Staffordshire	
	Draw a man: house: tree	<i>Spelling:</i> Schonell 1A Schonell 1B	
<i>Individual</i>	Piaget Causal Reasoning		
	S-B, Form L	<i>Reading:</i> Burt-Vernon	Rorschach Jackson Family
	WISC Performance	Graded	Attitudes (1962 only)
TRISTANS ONLY	Porteus Maze Classification	Vocabulary or Schonell Graded Word Reading Test	Raven's Controlled Projection Gibson Spiral Maze (1963)
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<i>Secondary</i>			
<i>Group</i>	Morrisby GATV (Tristan classes)	Mill Hill Voc. <i>Reading:</i> Schonell B	Sentence Completion Anderson Story
All Children	Progressive Mat 1960	<i>Spelling:</i> Schonell 1A Schonell 1B	Completion (Tristans and controls)*
	Draw a man: house: tree	<i>Arithmetic:</i> Staffordshire	3 wishes Sociometric choice
<i>Individual</i>	S-B, Form L	Burt-Vernon	Rorschach
	Kohs' Blocks (Alex.)	Holborn Reading Scale	Raven's Controlled Projection
TRISTANS ONLY	Porteus Maze Cube Imitation & Cube Construction		Gibson Spiral Maze (1963)
	Woodworth Wells (1962 only) Classification		
	Bender Gestalt Harris Laterality Test		

\* We are very grateful to Professor H. H. Anderson of Michigan State University, U.S.A., for the interest he has shown in the project.

## GENERAL PROCEDURE

a. The testing periods in March 1962 and March 1963 consisted of some three weeks, during which group tests were administered to each class, followed by individual testing of the Tristan children. It was fortunate that the schools in Hampshire remained open for this period after the college had closed for the Easter vacation, for this enabled the test team to move down from London to Fawley. During the first year some assistance with the group testing was given by the county psychologists, who made themselves responsible for giving the Arithmetic tests in the secondary modern school, and who also carried out some other group testing of the non-Tristan classes.\* During the second year all the group testing in the secondary modern school (with the exception of one test) was carried out by the writer, who also tested most of the classes in the junior school.

As far as possible, conditions were kept constant for the two years. The same members of the team tested the same children both times (with one or two exceptions). The March-April periods were the main ones for both group and individual tests. Further assessments were carried out in June and July of both years. These comprised the individual classification tests, the Porteus Maze and in 1963 the Spiral Maze Test.† The Performance Tests given in the secondary school in the spring of 1962 could not be readministered until July 1963. The Rorschach test was given in the third term of each year, as were the sociometric assessments.

b. Observations of behaviour were carried out during these testing periods, and at one or two other times when the writer was paying short visits to the school.

c. Ratings from class teachers were obtained in both summers, while comments were recorded during all visits.

Towards the end of July and in September and October 1963, visits were made to the homes to test pre-school children, the Merrill-Palmer being used for this purpose. At this time the homes of all the school children were visited and many of the older Islanders were seen.

\* We are much indebted to the Senior Psychologist, Mr. A. W. M. Harborth, and members of the School Psychological Services in Hampshire for testing assistance in 1962 and for help in marking certain group tests; also to the Statistical Branch of the Service for assistance in 1962.

† Copies of this were kindly made available by Dr. H. B. Gibson of the Family Development Study, Cambridge House, London, who scored the results.

It was possible to take both still and moving pictures of the children's behaviour in the playground. Unfortunately the films of day-to-day behaviour could not be made until the spring and summer of 1963.

It was unfortunate, too, that no camera was available the very first day the schools were visited, two days after the children's first attendance. The scene in the playground of the junior school was particularly striking, both during the morning and the dinner breaks: the Tristan children, as a whole, tended to be physically small and thin, and the coldness of the day—there was a powdering of snow on the ground and a keen icy wind—made them appear even smaller and thinner than they were. They shivered in their thin, inadequate clothes, still with the Tristan molly caps and white knitted socks with coloured bands round the top (probably both responsible for whatever warmth the children might feel). They huddled together in mute, wondering and rather frightened groups, eyeing their new companions apprehensively. The Tristan girls in the secondary school group were easily picked out too, partly because of their style of hair dressing, partly on account of their long mid-calf length dresses which stood out from the school uniforms of short skirts and blouses worn by the other girls. It was to be some time and involve considerable efforts on the part of others before the Tristan children adopted the clothes and took on the general appearance of the other school children.

### *Results*

The results are presented here in summary form only and for a limited number of the assessments made. The following measures have been selected:

1. Level of intelligence.
2. Educational progress.
3. Some features of personality and the children's social interaction within the school.

#### **LEVEL OF INTELLIGENCE**

As stated, group tests of intelligence were given (*a*) to obtain an indication of the level of intelligence of the children among whom the Islanders were placed, (*b*) to yield some information on what the Tristan children could and could not do, and (*c*) to enable comparisons to be made between successive testings which might throw light on possible adaptation and learning. As originally planned, the investigation was



to have included a two-years' follow up: but this proved impossible as the children returned to Tristan in October 1963. More intensive study was carried out of the Island children with individual intelligence tests.

## CHOICE OF TESTS

### 1. *Group Tests*

An important drawback of the majority of group tests lies in their restricted age range. This had two consequences for the present study:

a. children in the infant and junior school could not all be tested with the same tests in any one year, hence valid comparisons could not be made within the Tristan group as a whole;

b. some of the children moved out of the appropriate age for the test between the period March 1962 and March 1963. This did not affect consideration of their results relative to those of the rest of their class, since all were given both tests, but it made estimation of individual children's adaptation more difficult. It would have been neater if we could have obtained successive scores on the same test. We were able to do this for a fair number of the junior school children but not for all. A further complication lay in the fact that three children moved up into the secondary modern school in September 1962. They had to be tested along with the other children, using the tests selected for that school. Time precluded special re-testing on the old test: in any case they had all moved out of the age range.

Gaps occur in the group test results through absence. Although the attendance record of the Island children was on the whole good, there were a number of children absent either in 1962 or 1963 when the group tests were given. Colds, asthma and odd days off for other reasons meant that our group test records are not complete.

### 2. *Individual Tests*

We were well aware of the problems inherent in giving the Stanford-Binet to children such as these. The decision to do so was taken on several grounds: (a) the test itself, like any other intelligence test, is affected by cultural conditions, but it samples a wide variety of cognitive functions and often yields qualitative data of value; (b) we were interested to discover any changes occurring over the period of study, and though there is some difference of items in the different

TABLE I

## TRISTAN CHILDREN

Results of Intelligence Tests: Intelligence Quotients  
(Chronological ages in years and months)

<i>Infants</i>		<i>Group Tests</i>		<i>Individual Tests</i>	
<i>Boys:</i>	<i>Age</i>	<i>Kelvin</i>	<i>Moray</i>	<i>Stanford-Binet</i>	
	1962	1962	House 1963	1962 <i>corrected</i>	1963 <i>corrected</i>
<i>Girls:</i>	5·9	96	absent	90	92
	6·3	110	87	95	115
	6·6	108	82	94	92
	6·3	100	86	99	105
	6·11	104	absent	99	102
	7·0	96	absent	87	81
	7·3	92	NV <sub>5</sub> 96	84	92
			103		
			NV <sub>5</sub> 107		
					*Boys:
				<i>Age</i>	
				6·0	108
				*Girls:	
				<i>Age</i>	
				6·1	101
				6·3	92
				6·4	106
				6·4	100

\* These children were not in school in 1962.

<i>Juniors</i>		<i>Group Tests</i>		<i>Individual Tests</i>			
<i>Boys:</i>	<i>Age</i>	<i>Non-verbal 5</i>		<i>Stanford-Binet</i>		<i>WISC</i>	
	1962	1962	1963	1962 <i>corrected</i>	1963 <i>corrected</i>	1962	1963
	8·7	86	90	90	93	93	90
	9·7	85†	<70	63	61	64	71
	10·6	79	74	80	87	76	83
	10·8	absent	secondary school	93	79	74	74
	11·4	absent	secondary school	88	88	76	79
<i>Girls:</i>	7·7	MH 79	NV 92	90	84	85	90
	7·11	absent	left	80	79	86	left
	8·6	MH 109	NV 112	100	115	120	113
	8·11	NV 79	75	75	78	69	76
	9·2	108	114	102	100	86	113
	9·8	102	103	85	104	94	92
	9·8	93	86	85	87	79	92
	9·11	81	80	71	84	90	92
	10·3	—	110	102	109	104	110
	10·5	76	<75	71	78	65	64
	10·9	96	secondary school	83	91	92	86

† This boy received some help with the test in 1962, but none in 1963.

TABLE I (continued)  
Group Test Results

Seniors	Raven's Progressive Matrices						GATV				Stanford-Binet		
	1962		1963		1962		1963		1962	1963	1962	1963	
	Score	Percentile rank	Score	Percentile rank	Scale score	Percentile rank	Scale score	Percentile rank	Corrected	Corrected	Corrected	Corrected	
Age													
1962													
Boys:													
11·9	14	< 5	18	5	7	16-23	6	11-16	83			89	
12·2	17	<10	34	<25	1	< 1		not given†	79			77	
12·3	16	< 5	47	<75	6	11-16	7	16-23	105			111	
12·6	41	50	46	>50	5	7-11	7	16-23	92			86	
12·8	15	< 5	12	< 5	1	< 1	1	< 1	75			76	
13·7	47	>50	53*	<95†	6	11-16	6	11-16	95			93	
13·10	9	< 5	35*	<25	1	< 1	3	2-4	77			79	
13·11	34	<25	35*	<25	1	< 1	4	4-7	83			78	
14·0	11	< 5	12*	< 5	1	< 1	3	2-4	76			77	
14·9	13	< 5	left	left	1	< 1	left	left	63			left	
15·1	37	<25	left	left	1	< 1	left	left	78			left	
1963													
11·9*	—	—	17	<10	—	—	—	not given	93			79	
12·0*	—	—	30	<25	—	—	—	1	88			88	

## Girls:

12·0	27	<25	33	<25	1	< 1	9	31-40	81	79
12·8	12	< 5	10	< 5	1	< 1	2	1-2	69	66
13·3	12	< 5	5	< 5	5	7-11	5	7-11	90	86
13·10	15	< 5	34*	<25	4	4-7	6	11-16	79	79
13·11	17	< 5	21*	< 5	4	4-7	1	< 1	62	66
13·11	4	< 5	11*	< 5	1	< 1	not given†	left	52	45
14·4	9	< 5	left	left	3	2-4	left	left	72	left
1963 11·8*	—	—	37	< 50	—	—	7	16-23	83	91

\* Tested in the junior school in 1962.

† This boy was over 14½ years when tested for the second time. Thus it is not strictly accurate to translate his score into a percentile rank from Raven's conversion table, which does not go beyond a chronological age of 14 years. If the table is used this boy's score falls at the percentile rank given, as do those of the six other children (marked thus \* in the Table), who were also outside the age range.

‡ In 1963 GATV was not given to the Junior Remedial Class, most of whom were very poor readers.

years, the test as a whole would be likely to give reliable information about such changes; (c) we used the L form rather than the new L-M since a good deal of data on Form L, based on the results of English children, was already available. The Fraser Roberts Correction Table<sup>5</sup> enabled us to compare the Tristan with British children of all ages. The newly-published American L-M form of the test, which some might consider preferable, seems even more heavily loaded with verbal items than the old L form, and hence was more likely to be unsuitable for the Island children. We used the Performance Scale of the WISC with children between 8 and 11 years, but not for the older and younger, since there is reason to believe that its use with either would not be entirely satisfactory. In 1962 the Island children were hopelessly at sea in the Picture Arrangement (a point of interest). In 1963 they all obtained some sort of score in it, but to permit of comparison between the two years we omitted to score this test in either year and prorated the other scores. Table I gives the results for both years in:

- i. the non-verbal group tests for all ages;
- ii. the verbal test for the secondary school children;
- iii. the Stanford-Binet and the WISC Performance.

#### COMMENTS ON INTELLIGENCE TEST RESULTS

##### *1962 Individual Tests*

1. One of the most noticeable features of the Stanford-Binet results is the decrease of intelligence quotients with age. While six of the seven infants tested and seven of the 16 junior school children fell within the average range, the same is true of only six of the 18 secondary school children. This might be thought to have been due to the particular nature of the items assigned to the various age groups, some penalizing the older children more, but inspection of the successes and failures in different items in the records of the secondary school did not support this. It was possible that the older children may have been less at ease in an individual test situation, especially the older girls who were tested by a man. This may have been true of one or two older girls, just as it was true of one or two younger girls in the junior school. The opinion of the teacher who had been with them on the Island and who knew the children well was sought. She expressed the view, based on her experience of teaching all the seniors and most of the juniors,

that the older group was in fact less intelligent than the younger one.

2. Mention has already been made of the inability of the junior school children to do the Picture Arrangement in the WISC. They seemed unable to grasp the general principle, but this may have been largely due to the fact that the type of situations depicted were outside their range of experience. The I.Q.s in the WISC paralleled those in the Stanford-Binet rather closely. Table II shows the level of intelligence of the children in both tests, grouped into categories.\*

TABLE II  
*Categories of Intelligence as shown by I.Q.s.*

		1962		1963	
		<i>Stanford-Binet</i>	<i>WISC</i>	<i>Stanford-Binet</i>	<i>WISC</i>
<i>Boys:</i>	Low average	Average		Low average	Average
	Inferior intelligence (ESN)	Mental defective		Inferior (ESN)	Borderline
	Limited intelligence	Borderline		Low average	Dull normal
	Low average	Borderline		Limited	Borderline
	Low average	Borderline		Low average	Borderline
<i>Girls:</i>	Low average	Dull normal		Limited	Average
	Limited	Dull normal		Limited	left
	Average	Superior		Above average	Bright norma
	Limited	Mental defective		Limited	Borderline
	Average	Dull normal		Average	Bright norma
	Low average	Average		Average	Average
	Low average	Borderline		Low average	Average
	Limited	Average		Limited	Average
	Average	Average		High average	Bright norma
	Limited	Mental defective		Limited	Mental defective
	Limited	Average		Low average	Dull normal

*Note:* 'Mental defective' in WISC is approximately equivalent to 'inferior intelligence' or 'in need of special educational treatment' (E.S.N.) in the Binet; 'borderline' to 'limited'; 'dull normal' to 'low average'; 'superior' to 'above average'.

\* For the Stanford-Binet the categories agreed upon by the British Psychological Society are used; for the WISC those presented by Wechsler (for the Full Scale), p. 16 of the Manual.

The results for the group as a whole in 1962 indicate that a Performance Test such as the WISC did not give a more valid indication of level of intelligence than a verbal test of the Stanford-Binet type, in terms of a higher assessment. Nine children obtained I.Q.s in both tests of much the same order: four were higher in the WISC and three higher in the Stanford-Binet.

Although the results are not presented here, the same obtains for the older group. The majority did no better in the performance battery than in the Stanford-Binet.

### *1962 Group Tests*

A similar decrease of I.Q. with age is seen in the results of the group tests. The infants were all of average I.Q. while the I.Q.s of many of the juniors were of the same order. This was not the case with the seniors, who as a group did poorly in 1962 both in the Raven's Matrices and in GATV. They were not good at perceptual tests, a number failing in the Stanford-Binet in Memory for Designs and Paper Cutting. Nearly all were poor in Kohs' Blocks and Cube Construction, while their mental ages in the Draw-a-Man Test were usually below their mental ages in the Stanford-Binet.

The evidence from group testing therefore confirmed, on the whole, the later results in individual tests.

How inefficient were the tests in 1962 in assessing the children's capacity? By March 1963 they had been in school in this country for about fifteen months. They had been tested several times and the seniors had also undergone school examinations.

### *1963 Individual Tests*

1. The increase in Stanford-Binet I.Q.s for the group as a whole is not statistically significant, although four children among infants and juniors show increases of over 12 points.

There was no such improvement in the senior group. This confirms the suggestion that the older children were in fact of lower intelligence than the younger children. In 1962 the vocabulary scores were low, as might be expected. These did not improve substantially in the intervening period, and there is no correlation between increase in I.Q. and increase in knowledge of words, whether shown in the vocabulary test of the Stanford-Binet or in other measures of vocabulary used. In 1963 scores on memory items were higher for certain children, and whereas reasoning tests proved difficult for many in 1962 some suc-



ceeded much better with these in 1963. But their general pattern of success and failure did not show consistent trends for the group, either in any one year, or from one year to the other.

2. The WISC I.Q.s of the junior children remained constant for six of the 15 children tested, decreased slightly for two, and improved for seven. In five of these cases, the increase was seven points or more. One of these was an autistic boy whose verbal output was very poor. For the remaining four the result was in line with that obtained in the Stanford-Binet. For three children the Stanford-Binet in both years gave a higher assessment. The Stanford-Binet results taken for the group as a whole gave a somewhat better (in the sense of more consistent and higher) set of assessments than the WISC.

### 1963 Group Tests

The I.Q.s of the infants had dropped, but a different group test had been used, so little comment can be made about these results. The juniors remained very much where they had been. It is obvious that most of the seniors still obtained poor results in the Matrices, though one or two had gone up, one quite strikingly. The results in GATV also show that they were all still somewhat handicapped in verbal knowledge and expression.

1. In summary it can be said that the decrease of I.Q. with age was evident in both individual and group test results for both years, with some exceptions. The 1963 testing confirmed the 1962 results and the observations of the Island teacher (and also those of members of the testing team who had had occasion to observe both younger and older children).

2. Taking the highest result in any individual test in either year as the most valid assessment, only three children out of the total 45 assessed in one or both years\* were of good average intelligence, and in only one test (WISC Performance, 1962) was there an I.Q. above average.

Two of these three children were brothers, the elder being the most intelligent of the secondary school group. This family was an interesting one; its nine members included three defectives, of which the youngest one, a boy of six years, was non-ambulant. A boy of 18 years attended an Occupation Centre, while a girl of 13, at school, obtained very poor scores in all tests. The two brothers mentioned here were both intelligent and lively and the elder one particularly took to school work very well.

\* Three secondary school children left before the second testing; five infants were present only for the second testing.

## EDUCATIONAL PROGRESS

Some of the educational problems confronting the Island children have already been mentioned: altered conditions of learning in much bigger classes, and, in the case of the secondary school children, frequent adjustment to different specialist teachers. The range and variety of subjects in the curriculum were puzzling to them, quite apart from the inadequate knowledge which they themselves brought to the situation. The marked contrast between their two educational environments is emphasized vividly in notes made by the former Island teacher,\* who helped the Islanders during their first six months here. They are summarized below:

On the Island, until about 1960, there was little suitable reading material in the school and even when sets of simple reading books were introduced from England, much time had to be spent trying to explain situations described in them that were outside the children's experience. Poetry, except about mountains and sea and simple ballads, was beyond them, though they have a keen sense of rhythm. To reading they brought limited experience, limited vocabulary, a language which contained words and idioms peculiar to the Island, and an uncertain grasp of the past tense. History, geography and general knowledge were regarded almost as fairy tales, and only began to make sense after the children had been some months in Britain. Arithmetical problems based on unknown experiences caused them considerable difficulty, increased by the introduction† to Tristan, in April 1961, of the decimal monetary system. The change back to the English system complicated matters and a frequent comment made by teachers here was, 'finds arithmetic difficult'. This was especially true of the seniors. Their art work on the Island was fair, their design being original and varied, but artistic development was hampered by limited supplies due to cost of transport. Some of the children were to develop quite well in art while in this country. Games proved less difficult to cope with than academic subjects, for rounders had been enjoyed by all while on the Island, though played with their own set of rules and equipment. Netball and tennis, however, were impossible due to unusually high winds and frequent gales. At times, too, when the children were suffering from food deficiencies, games that entailed much running about and physical effort had to be curtailed, especially as many of the

\* I am most grateful to this teacher for her ready assistance and the valuable information she gave about conditions on the Island and about the children.

† From South Africa.

children were prone to asthma. The boys played football and both girls and boys played hockey on the football pitch, amid large outcrops of rock and a fourteen foot drop to the sea. Despite this previous limited experience, the children took to games and Physical Education in their British schools, many being rated as average and one or two as good.

#### STANDARDS OF ATTAINMENT

The information about the children's educational attainments at the beginning and during their period of stay comes from the following sources:

- a. Results of standardized tests in Reading, Spelling and Arithmetic.
- b. Teachers' estimates rated on a five-point scale.
- c. Examination results and comments on report cards for the secondary school children.

#### (a) *Results of Standardized Group Tests*

##### *Reading*

*Infants* In 1962, six of the seven children in the infant school were given a group test. One girl was a year retarded, while the youngest boy obtained a score of only two points, but four of the seven obtained reading ages close to their chronological age. This was a good start, but progress during the next year, though made by all, was slow for most of them.\* Two made reasonable gains, especially the non-reader of 1962, whose reading age in 1963 was six years three months.

*Juniors* Scores were obtained for 14 juniors in 1962, using the Gates Primary Reading Test for the youngest children and Schonell Silent Reading Test A for the remainder. The results were good. Eight girls were average, two making scores more commensurate with their chronological than their mental age as measured by the Stanford-Binet. One of these girls had an average I.Q. in the WISC Performance Scale, though she obtained lower results in all other intelligence tests. None of the children was seriously backward except one boy and one girl, both non-readers.

How did the children fare in 1963? A number were absent for the reading test and three had moved to the secondary school. For most

\* They were all assessed below average in an individual test in 1963 (Schonell Graded Word).

of those remaining progress had been slow, but there were some striking exceptions. One girl (whose I.Q. in a verbal test altered strikingly from 80 in 1962 to 105 in 1963) raised her reading age from nine years five months, to 12 years. Another improved by 17 months, while a boy pulled himself up from eight years 7 months to 10 years three months (after a second year in the same class). One of the non-readers was just beginning to read, but the other remained stationary. He was not to begin reading until September 1963 when his general emotional condition improved. However the general slow progress must be considered in relation to the other children in the classes. The few non-Tristan children in the older special class were very backward, and some made even slower progress than the Tristan children.

*Seniors* The reading scores of the seniors will be considered in more detail. Table III shows their results for 1962 and 1963 in the Schonell Silent Reading Test B. Mental ages in the Stanford-Binet are also

TABLE III  
*Reading Ages (in years and months)*

	1962			1963		
	C.A.	M.A.	R.A.	C.A.	M.A.	R.A.
J. Rem. B	12·2	8·10	7·2*	13·1	9·4	7·2
G	13·11	5·10	7·0*	14·11	6·2	7·0
I <sub>2</sub> B	12·3	13·0	13·0	13·0	14·9	13·9
I <sub>4</sub> B	11·9	9·2	8·7	12·8	10·10	9·10
B	12·6	11·2	10·1	13·5	11·2	10·10
G	12·0	9·2	10·1	12·11	9·6	10·3
II <sub>3</sub> G	13·3	11·6	11·8	14·3	11·8	13·1
II <sub>4</sub> B	12·8	8·8	7·11	13·8	9·6	8·2
G	12·8	7·8	7·8	13·9	8·2	8·1
III <sub>4</sub> B	13·11	10·10	7·9	14·10	11·0	8·4
B	13·7	12·6	9·10	14·6	12·10	9·8
B	14·0	9·10	10·8	14·11	10·10	11·2
G	13·11	7·6	11·6	14·10	9·4	11·11
G	13·10	9·10	7·8	14·10	11·2	9·5
III <sub>5</sub> B	13·10	9·8	10·10	14·10	11·2	11·4
G	14·2	9·10	10·3		left	
IVP B	14·10	10·0	7·11		left	
B	15·1	11·0	7·11		left	

\* A score of 3.

given. Though mental age and reading age are by no means perfectly correlated, a reading test which measures comprehension as well as fluency and accuracy bears some relation to general intelligence. It is therefore more meaningful to consider the children's results in relation to their general mental level.

*Comments* In 1962 all but one of the Island children were backward in reading in comparison with the average children of their chronological age. The exception was a boy, the most intelligent of all in the secondary group.

One third of the children were also retarded (judged by the rather generous borderline of a reading age eighteen months or more lower than mental age). Five had reading ages above their mental age. One of these was a girl of very low intelligence (probably brain damaged), whose score of three represents a somewhat artificial reading age of seven years two months: the second was a rather puzzling girl who failed to increase her score in reading in 1963, or to raise her I.Q. in the Stanford-Binet, but whose written English was quite good and who made a significant gain in GATV. The third girl, whose intelligence was almost certainly underestimated in the Stanford-Binet, obtained scores above her mental age in both years.

What sort of progress was made during the year that lay between the two tests? Most had made some, though for many it was less than a year of reading age. Indeed, of the 15 retested, only seven made gains of half a year or more. Two children belonging to the same family improved very strikingly. These were brother and sister. The boy, in Class I<sub>4</sub> in 1962, whose reading age was then eight years seven months, made fifteen months' gain by 1963. He was a cheerful, overactive extravert who settled down very well during his second year in school and whose improved level both in reading and intelligence reflects a real adjustment. His sister, in Class III<sub>4</sub> in 1962, made progress of the same order. Another girl in Class II<sub>3</sub> in 1962, though not bright, was a particularly hard worker and made good educational progress all round, including reading.

Much depends upon the school environment in attempting to assess progress, hence it will be convenient to look at the results in Table IV, which gives the scores of each Tristan child together with the mean and s.d. of the class scores.

The class means both in 1962 and 1963 are low, class progress being slow, as may be expected from children allocated to streams for slow learners.

TABLE IV  
*Schonell Silent Reading Test B*  
*Scores of Tristan Children, Class Means and S.D.*

Class in 1962	1962			1963		
	Tristan score	Class mean*	S.D.	Tristan score	Class mean*	S.D.
J. Rem. B	3	9.9	5.1	3	6.4	5.6
G	3	9.9	5.1	3	6.4	5.6
I <sub>2</sub> B	36	33.0	5.7	39	37.5	4.8
I <sub>4</sub> B	13	19.2	3.7	20	22.3	5.5
B	21	19.2	3.7	25	22.3	5.5
G	22	19.2	3.7	23	22.3	5.5
II <sub>3</sub> G	31	31.4	6.5	37	35.2	5.4
II <sub>4</sub> B	9	24.7	5.7	10	28.4	8.3
G	9	24.7	5.7	12	28.4	8.3
III <sub>4</sub> B	7	29.2	7.0	11	31.4	7.2
B	20	29.2	7.0	19	31.4	7.2
B	24	29.2	7.0	27	31.4	7.2
G	30	29.2	7.0	32	32.6	5.3
G	9	29.2	7.0	19	32.6	5.3
III <sub>5</sub> B	25	29.2	7.0	29	31.4	7.2
G	23	29.2	7.0		left	
IVP B	9	29.3	7.5		left	
B	9	29.3	7.5		left	

\* Calculated without the Tristan scores.

The results of the Island children, when compared with those of the English children, show that when they entered the secondary school in 1962, eight were around the mean of their class, three were about 1 s.d. and seven about 2 s.d. below the average. In 1963† all those near the mean had maintained their position and some relative improvement had been made by the brother and sister already mentioned. The increases in reading ages, noted in Table III, are to be considered as creditable. As in the junior school, one or two children had profited considerably from their new educational environment.

† Unfortunately three children had left before the 1963 testing, hence the group has shrunk to fifteen.

A good deal of space has been devoted to reading, since this is the most pervasive subject in the school curriculum, especially in the secondary school. What of the other two basic subjects?

#### SPELLING

Nearly all the Tristan children were weak in spelling, even when they were not noticeably backward in reading. A comment made by a teacher in the junior school points to one of the reasons for this, namely, that their spelling was affected by their own idiom and pronunciation. One child's essay on Christmas, written in December 1962 (and given below) shows this difficulty very clearly and provides a good example of the handicaps that most of the children laboured under when trying to express themselves orally or in free written work.

Frances green.

Monday December 17<sup>th</sup>.

Christmas

When Christmas came we had a shely day beccuse baby Juse his bron in a sabb he has mother Mary and a Daddy Jopher they all live in the same place he daddy was a coper. when Jseus got up he what be a coper to, he fash water for his mother. When Juse got up he was a coper

FIG. 1

#### ARITHMETIC

The majority of children were weak in arithmetic, in the case of the secondary school children this weakness being the most frequent comment in the teachers' reports. Only one girl was above the average

of her class. By 1963 some progress had been made, although most of the secondary school children were still below the means of their classes. A brother and sister, already referred to, showed considerable improvement, however, raising their relative position from about  $-1$  s.d. to about  $+1$  s.d. The others remained in the same relative position below the mean, or dropped still lower. The seniors undoubtedly did find the type of work in school beyond them.

The juniors did somewhat better, four children making good progress; one of these, however, was repeating the work of the class for the second year.

(b) *Teachers' Estimates\**

The teachers' estimates were given with reference to the work of the class as a whole, and mention has already been made of the fact that only one girl in the junior school was placed in the ordinary class for her age, and kept herself in it, while this was true of only one boy in the secondary modern school, in the second of a four-stream year. Teachers' estimates, in the junior school, take into account all the work of the children, not only their attainment in the basic subjects, hence they amplify data yielded by the standard tests. In the secondary modern school, however, the teachers' estimates are more specialized. The class teacher often had little first-hand knowledge of how the children fared, and the opinions of the specialist teachers referred only to attainment in their own subjects. It was therefore difficult to get an assessment of the all-round standards of work of the Islanders. Fifteen junior school children were assessed. Of these, two were rated average, seven as 'poor', and six as 'very poor'. Four of the 16 children rated in the secondary school were said to be 'average', 11 were rated 'poor' and one girl in the junior remedial class was rated 'very poor'. Thus, only six of the 31 children were rated as 'average' for their classes.

Did this picture alter in any way during their second year? The answer is yes, for quite a number of the children. For instance, a girl in the junior school maintained her position in the ordinary class and though still rated as poor was stated to have improved. Her teacher said 'she has done quite well at mechanical arithmetic'. One boy had also improved. The older children had also made some progress, though none were transferred into a higher stream.

\* All the secondary school and most of the junior school children were rated by two teachers. Where two assessments placed a child in different categories, the higher has been given here. This happened in only two cases.



Three children, two boys and a girl, rated 'average' the first year, had achieved a rating in 1963 of 'good'. Two had pulled themselves up from 'poor' to 'average', and one of these received the comments: 'A. works fairly well in II<sub>4</sub>. He works sensibly in practical subjects and gets on with the job in hand. He likes doing jobs about the school and in the class room.' This was an active, strong boy, rather more aggressive than the other Island children. This emerged in the teacher's further statement that 'He likes the competition in school work'.

### (c) Examination Results

A study of examination results shows the same overall improvement for many of the secondary school children. These are given in Table V, which gives class positions in three sets of examinations. Information is available for 13 out of the 18 in 1962.\* Unfortunately, the numbers have shrunk by 1963.†

TABLE V

#### *Position in class examinations*

1962				1963		
<i>Class</i>		<i>No. in class</i>	<i>place</i>	<i>Class</i>	<i>Spring</i>	<i>Summer</i>
I <sub>2</sub>	B	30	23	II <sub>2</sub>	18/34	10/36
I <sub>4</sub>	B	25	9	II <sub>4</sub>	16/25	6/26
	B		23		15	16
	G		12		7	7
II <sub>3</sub>	G	33	30	III <sub>3</sub>	15/36	19/37
II <sub>4</sub>	B	28	28	III <sub>4</sub>	28/30	absent
	G		27			29/32
III <sub>4</sub>	B	30	26	IVPr.	graded as average on year's work	
	B		29		left for Tristan	
	B		30		absent for part of exams.	
	G		27		9/14	absent
	G		28		14/14	left
IVPr.	B	16	15		left	

\* Two children in a special class (Junior Remedial), did not sit examinations, one boy had left at Easter and there are no records for two others.

† Three had left, one to go back to Tristan in March, three were absent, and for one only grade placement was made.

In 1962, 10 of the 13 were near the bottom in the class examinations. In 1963 all but three had pulled themselves up, with one boy, the most intelligent of the Tristan group, showing the most striking change. Two more, of low average intelligence, made excellent progress by great determination and application.

It is difficult to know how much of this improvement was due to increasing familiarity with the lessons and how much to adaptation to the different sort of teaching environment. There is some slight evidence on this point, not from the original group placed in the secondary modern school, but from the progress of three Tristans who moved into it during the next school year. Two of them were upset about changing schools when first seen in December 1962. One was the only Tristan in her new class. She said that she had made no friends and missed her own friends (all Tristans) in the junior school very much. She had settled somewhat by March, but was second bottom and bottom in her class in both sets of 1963 examinations. It was not until September 1963 that she had really begun to settle down, and unfortunately there are no examination results to show whether this social adaptation was reflected in improved class position. The work of a second child was also poor. Though his class teacher did not comment adversely on his general attitude, it was apparent to two members of the psychological team who saw him in his new school that his behaviour had deteriorated considerably. He had become moody and quite aggressive. It is possible that the change of environment from a small and more personal junior school to the impersonality of the larger school was something to which the previous experience of the Tristans made it difficult to adapt. Observation of these changes in children whom we knew quite well showed that they did have difficulties in adapting to the secondary school environment and we may conclude that this also affected the work of some of the older children in 1962, about whom at that time it was difficult to obtain accurate knowledge.

It would obviously be unsafe to venture a firm opinion on what aspect of their new educational environment presented most difficulty to the older Tristan children. The relation between work progress and social adjustment both to peers and teachers is a close one, but the number of problems facing the Tristans was considerable, and without very intimate knowledge of each child in 1962 it would be difficult to hazard even a guess as to their interaction. The examination system itself constituted a new problem. We can be definite, however, about the time it took for these difficulties to be tackled and

there is evidence that by 1963<sup>2</sup> the Tristans were on the way to doing this.

Space precludes more detailed discussion of the efforts of the Tristans to adjust to school. The changes noted in particular children in both schools depended greatly upon differences in application and interest. The acceptance of changed standards and demands was on the whole more striking among the secondary school children than among the juniors, who contrived in their tightly-knit groups to preserve the somewhat lethargic approach to self-improvement that had characterized most of them on arrival. One of the main stumbling blocks in both schools continued to be language, both oral and written work, in which they were still handicapped when they left in October 1963.

### **PERSONALITY CHARACTERISTICS**

What were the children like when they arrived, and what were the changes that were noticed in them during the succeeding year? The information summarized here is drawn from teachers' comments and ratings, and from the Rorschach test, results of which will be presented in a very general form.

#### **TEACHERS' COMMENTS AND RATINGS**

The teachers were asked to fill in two schedules, the first consisting of general points about the Island children as a group, the second about each Island child. On the second form the teachers were asked to rate the child's attitude to work, reactions to discipline, and to the teacher; and his social relationships—both with Island and non-Island children. Space was left for the teachers' own comments, in addition to their replies to the standard queries and ratings. It proved comparatively easy to obtain data from the teachers in the primary school who were in constant touch with the children and who had many opportunities of seeing them not only in the class room, but also in the playground. It proved much more difficult to obtain such information in the secondary modern school. The teachers there had far fewer opportunities of getting to know the children in different learning situations and even fewer of seeing them in the playground. Hence, they felt that they had to confine their assessments to rather general statements which often did not say very much about the personalities of the children. Whereas in the primary school the schedules were supplemented by special ratings for five personality variables, this procedure could not be carried

out by the secondary school teachers, who did not feel they knew the children well enough.

The information collected has been condensed as follows:

*a.* In 1962 all the teachers in the infant, junior and secondary schools mentioned as one of the most striking features of the children their appearance and their dress. They are mentioned often as 'looking like gypsies', and the younger ones especially as being small and thin. Both appearance and dress had altered to a very great extent by 1963. The children in the junior school had filled out noticeably, and all the children had adopted the dress and hair-styles of the non-Island children. Their physical characteristics had not changed, of course, but had become less noticeably different following their adoption of British clothes. They had, too, become more familiar by this time to the English teachers.

*b.* When they first arrived they were all very silent and shy. This persisted for some months, but most of the children altered a good deal during the first year. Their silence seemed to be connected with two things—first, the fact that in general the adult Islanders are not very loquacious, though some do talk readily and at length: the second that the adults were often heard (on visits to their homes to test pre-school children) to say to a child 'you talk too much'. Hence their quietness may have arisen from the adult belief that 'children should be seen and not heard'. Though the children during the second year were much more ready to talk, whether in a group or alone with an examiner, they continued to find it difficult to express themselves well and fluently. This lack of skill, as distinct from an inclination to talk, is no doubt attributable to their hitherto very restricted environment. Some of the secondary school children had just begun to use the school library and read for pleasure when the time came for them to return to the Island. But whatever the school opportunities, many were in homes where their parents were ill educated, an important factor.

*c.* As a group they showed a good deal of passivity. This was commented upon both by junior and senior teachers, e.g. 'They ask for help even when it is not really necessary'. Those who seemed reluctant or afraid to ask others for help would try to copy or imitate another Island child.

*d.* In general the children did not show much aggression either towards each other or to the non-Tristan children. Indeed, aggression was one of the characteristics they most disliked, according to their own statements. It was shown, however, early on by one or two of the

older girls who became definitely hostile towards the non-Island girls. A domestic science teacher reported, 'When they first came they were mixing well, and the others were all over them. After two months they worked on their own . . . and one of them was very spiteful. The other girls came to me and said, "They hate us, Miss".' At a later stage it was noticed by the writer and confirmed by the teachers that these negative attitudes were displayed by nearly all the Tristan children. This was in December 1962 when the Islanders were about to hold their poll to determine whether they should return to Tristan. The Island children, no doubt reacting to tension in their homes, could not run down life in England enough and they retreated from social relations with the non-Island children. This hostility was mainly confined to speech, and diminished when, still later on, return to the Island became definite. Their social relations for the most part were marked by withdrawal rather than aggression.

e. They were generally neat and tidy in their work and accepted class room discipline very well. Their conduct was good and during the period between 1962 and 1963 many developed into willing and helpful members of their class. Most took pleasure in trying to work well, an attitude which paralleled their parents' pleasure in being thought well of at work. This was more evident among the older group.

#### SPECIAL RATINGS IN THE PRIMARY SCHOOL

Ratings on a five-point scale were obtained from primary school teachers for the following variables: curiosity, independence, aggression, competitiveness and gregariousness. The five categories of behaviour, A to E, were carefully defined for each trait, A being the highest, with an anticipated 5 per cent at the extremes. The results are given in Table VI.

TABLE VI

Trait	Category					N=23
	A	B	C	D	E	
Curiosity	0	2	6	8	7	
Independence	2	1	7	11	2	
Aggression	0	0	4	11	8	
Competitiveness	0	2	12	9	0	
Gregariousness	6	4	11	1	1	

It will be seen that the Island children were rated low for curiosity. This is in accord with what has been noted by observers living on the Island. Of the two children rated above average, the behaviour of one, a boy, showed outward signs of curiosity in the form of exploratory poking about, but this was not true curiosity, for it consisted mainly of aimless random movements. He was a boy with a history of epilepsy, whose effective intelligence was low and who was in a somewhat autistic state during the period of stay. He improved a good deal in the course of the month prior to departure. The girl rated above average was a girl of good intelligence whose behaviour was really exploratory and goal-directed.

More than half were rated below average for independence, the two in category A being infants. (Of these, one girl, the eldest in the infant group, was really in charge of the other infants, who clung to her a great deal in the early months.) The general result, like that for curiosity and aggression, confirms the picture of the Island community as lacking in interest in the outside world, and being generally rather passive, dependent, peace-loving and non-aggressive.

The rather high ratings for gregariousness arise from the fact that the children sought each other's company and took obvious pleasure in being together; they do not relate to their social interaction with non-Island children.

By 1963 the children had moved into other classes, and some had moved into the secondary school. Teachers of the special classes had come and gone and it was impossible to obtain ratings for the children from the same teachers who had rated them in 1962.

However, a second set of ratings were obtained for 10 children who were in a special class in the junior school. Their teacher knew them fairly well, but had not rated any of them previously, having joined the staff in the intervening period. The ratings are given below and may indicate genuine trends in behaviour development.

The ratings suggest that the Island children during this second year were showing more interest in their environment, were somewhat less dependent than they had been and were more aggressive than before. There were, of course, individual differences among them, brought out not only in the ratings and teachers' comments, but also in the observations made by the team of psychologists. There was the boy already noted as highly autistic, whose high rating for independence meant only that he was an isolate both in the class and the playground; there were also children who seemed anxious; one little girl,

TABLE VII

Trait	Category				
	A	B	C	D	E
Curiosity	0	3	2	5	0
Independence	1	2	5	2	0
Aggression	0	2	5	2	1
Competitiveness	0	2	2	5	1
Gregariousness	0	7	1	1	1

for example, suffered from asthma a good deal in the first year, was very shy and withdrawing and was often away from school. A history was obtained from her mother which confirmed shy and withdrawing behaviour even at home. Happily she improved both physically and emotionally. One of her sisters went through a difficult stage on transfer to the secondary school and even in the junior school was easily upset and at times aggressive towards the other Island children.

The Rorschach test throws further light on general personality trends and on the presence of anxiety among the Tristan children.

#### RESULTS OF THE RORSCHACH TEST\*

The Rorschach Test was administered twice at an interval of a year to 35 of the children. Of these 35, 21 belonged to the primary school and 14 were in the secondary modern school, the numbers of older children being depleted since some had left and could not be tested in 1963.

As with the other individual tests, no control group from the school could be assessed. The Rorschach is known to be influenced by cultural and social conditions, but to select a suitable control group would have been very difficult, even if time had permitted. The most natural type of control in this country for the Island children would seem to be children from a remotecrofting type of community, far removed from the environment of Fawley.

Since the children in the primary and secondary schools were tested

\* The Rorschach testing in both years was carried out by Miss Silver and Miss Kingerlee of St. James' Hospital, Portsmouth. Grateful acknowledgement is made of their invaluable help.

by two different examiners, the results are presented separately. However, they have a lot in common.

### *Primary School Children, 1962*

These children adjusted well to the test situation, being by that time accustomed to test procedure. They were pleasant, smiled readily, were compliant, did what was asked and answered questions, but rarely volunteered remarks or questions of their own.

#### FEATURES OF THEIR RECORDS

Popular responses were as frequent as might be the case from a British sample of the same age range, while the same cards tended to be rejected. The differences were as follows:

1. The records were shorter.
2. They contained fewer whole responses and more detail.
3. Well organized whole responses were very rare.
4. The content of their responses differed from those of English children in that human movement was less common and the animal movement was conventional or passive. Natural phenomena played a larger part but the content was limited. There was, as might be expected, some reference to the Tristan way of life, though few direct references to the volcanic disaster. Fantasy figures were missing, there were no fairies, giants or witches, and no references to children's story books. Thus, though the children's perception appeared sharpened where nature was concerned, their records show a rather poor imaginative life when judged by our standards. There seemed to be few differences due to age, though the small numbers preclude any definite conclusions about this.

#### PERSONALITY CHARACTERISTICS

On the whole the records were like those of younger children. Colour was used simply and crudely; there was a good deal of perseveration, smoke, fire, blood (perhaps due to the disaster). They seemed to have difficulty in the organization of the parts of the blots, even when these were recognized; and they showed a satisfaction with an obviously incomplete concept not usual in children over five. Several records appear to denote emotional disturbance, shown in many ways, for example by 'm' responses of a sinister kind, by extreme restriction, by



very crude colour, by rejections, by anxious use of shading and lack of energetic 'm' responses.

It seems possible that the whole group in fact was showing in these records the result of shock and uprooting, but that some children showed this more than others. In general, it was difficult to pick out many records which give an impression of active, outgoing and imaginative childhood, such as we think of as 'normal'.

### *Secondary School Children, 1962*

The records of the two age groups are similar in their relative brevity, stereotypy of content, and relative lack of organization and originality. Preoccupation with natural phenomena is revealed by the older children, and an apparent difficulty in establishing human relationships. However, as might be expected of adolescent and pre-adolescent records, they show a wide range of variability in many scoring categories. Six of the twelve children over 13 years of age gave records suggestive of considerable emotional disturbance.

The disturbed children's records indicated some tension, anxiety and impulsiveness. Natural phenomena and animal anatomy occurred with even greater frequency in the fantasy of this group. Whether current adolescent imbalance can explain this or whether the children of this age were more vulnerable to upheaval and had consequently reacted more strongly, it is impossible to say.

There were few positive signs of maturational change between the years. There was little evidence of any increase in the ability to conceptualize nor did any greater capacity seem to be emerging for forming relationships. On the whole the records were poorly organized and immature—and suggest passive dependent personalities, lacking drive. However, they give evidence of self sufficiency, suited to a simple environment where there is easy gratification of instinctual needs and where there are realistic outlets for unruly fantasy.

### *1963 Records*

In the primary school sample 21 children were tested twice. Five of the youngest children's results are excluded from this report, since they were too individual and different to be grouped with those of the other children. Even so, it is still difficult to generalize about the remaining sixteen. Ten children gave records of the same length both times, within one response plus or minus; three were considerably longer and three

considerably shorter. When all the categories are analysed, there are few consistent differences between the two sets of results. The main changes were an increase in animal and animal movement responses, a decrease in nature responses, and a decrease in shading as diffusion. There were also fewer references to Tristan da Cunha (with one or two marked exceptions).

There seems to be some relation between age, and kind and degree of change. The records of the nine- and ten-year-olds, which were the most meagre and restricted, showed little change, and tended on the second test to remain bare, short and lacking in organization and originality. The records of the younger ones, aged eight in 1963, had changed more, showing rather more form and less crude colour, and some slight attempt at organization where none had been obvious before. One or two of these children gave more overtly anxious responses.

Some of these difficulties may be due to maturation; the seven-year-olds in 1962 gave records with colour naming or colour description, and babyish concepts, and in 1963 used more form, which is a change one would expect with normal development, except that with our children this tends to take place about two or three years earlier.

The records of 14 children in the secondary modern school were available for comparison between the two years.

The following trends were observed; some of which are as likely to be due to maturation as to modifications brought about by a year in a new environment.

*a.* There was a slight but insignificant decrease in the time of response and in reaction times to both chromatic and achromatic cards.

*b.* There was a slight overall increase in the number of responses.

*c.* There was a slight overall increase in F per cent.

The last two trends suggest that productivity had risen but that there was more constriction.

*d.* An interesting change was the decrease in the number of 'm' responses. In 1962 six children produced some 'm' while only one did so in 1963. Two boys and a girl aged 12-13 years gave considerably more FM in 1963.

*e.* There was a marked reduction in the use of Nature responses in the content of the records. In 1962 eleven children gave from 1-8 Nature responses. In 1963 five gave none at all.

This reflects the ability of most of the children to acquire a new frame of reference for their fantasy. The results in (*d*) and (*e*) above

suggest a diminution in the free floating anxiety and tension which these types of response are held to represent. These two features were not related to age and maturational level, and seem to be the only ones which can be considered to reflect adaptations which took place in these children during their stay in England.

*f.* The other changes that were noted appear to be related to age, the early adolescents showing them more clearly than children at the stage of late latency or well into adolescence. Such changes may be due either to the children's particular stage of maturational development, or to particular effects of the new environment to which children of this age reacted differently from either the younger or the older. The only clear evidence of adaptation to the new environment is given by those variables found in the records of the group at all ages, namely: the decrease in Nature responses and the decrease in 'm'.

## **SOCIAL INTERACTION**

How did the Island children mix with the English children? Information on this was obtained from the comments of the teachers, by playground observation and by the sociometric assessments carried out in July of each year. In the junior school two such assessments were made, at an interval of about a week, involving the answers to simple questions such as, 'Who would you like best to ask to a party?' and so on. In general it is true to say that the Tristan children did not mix. This was not only observed constantly in the playground of the junior school, but also emerged in the sociometric results. There was little change during the time they were here until well into the summer of 1963, when some of the older girls in the junior school could be seen in small groups with one or two English children. But these tended to be transitory in nature.

Fig. 2 shows the constellation of Tristan girls and non-Tristan girls in the summer of 1963. Fig. 3 shows the interaction among boys in the summer of 1963.

In the senior school much the same state prevailed, although one girl, the only Tristan girl in her class and the youngest of the group, mixed well from the start. As always there were some individual children who formed exceptions to the general rule. The dispersal among classes in the secondary school played an important part in making them seek wider contacts among non-Tristan children. However, when observing groups in the playground of the secondary

(Invite to a party)

Girls' choices:  $\rightarrow$  1st choice

$\rightarrow$  2nd choice

T = Tristan girl

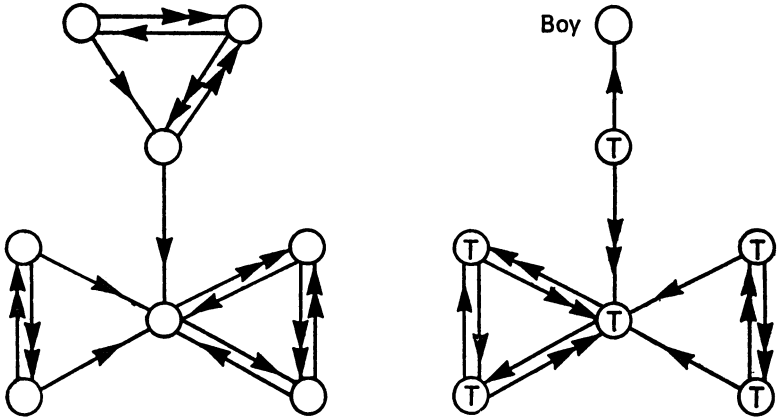


FIG. 2

school, the Tristans were more often than not to be seen in their original strongly integrated groups.

### *Final Comments*

It is difficult to sum up impressions and results in a word or two. There is no doubt that all the Island children, even the youngest, laboured under disadvantages when they first arrived here. They were ill-equipped to deal with the educational setting in which they found themselves. This was especially true of the senior children whose mental capacity was lower than that of the juniors and infants, the results of the intelligence tests being confirmed by informed opinion and observation of the seniors in learning situations. The problems with which they had to deal were substantially greater than those of the younger group. It is clear that a certain number made full use of their new environment, after a rather slow start. It is equally clear that many lacked the capacity to make more than very slow progress. The jobs

(Invite to a party)

Boys' choices:  $\rightarrow\rightarrow$  1st choice

$\rightarrow$  2nd choice

T = Tristan boy

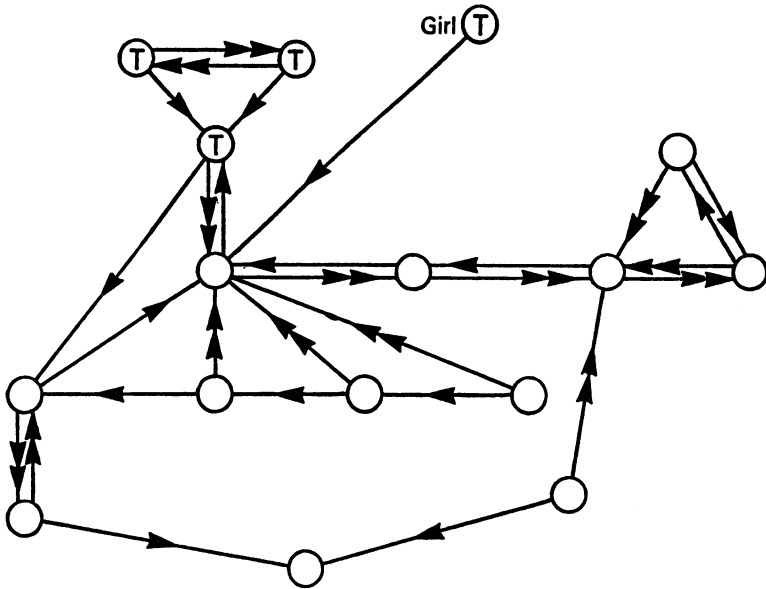


FIG. 3

which the school leavers found were of the unskilled sort, with which they were perfectly content. With but few exceptions they gave no evidence of definitive goals, vocational or otherwise, and their stay in England had not brought out initiative or drive. It is possible, of course, that the time may have been too short for such forces to become operative. It must be remembered, too, that the children, like the adults, never thought of their stay in England as anything but temporary, a fact which might well have reduced any incentives to adapt more fully.

The younger children presented a similar picture, although many of them by reason of higher intelligence and a good start in reading, were better fitted to adapt educationally and perhaps socially. Some indeed, seemed to benefit less than the older children. It may be that

the closeness of their integration with their own group, inside and outside the class room, militated against adaptation.

They showed individual differences, however, which it is impossible to amplify here. Taking the group as a whole they remained for the most part closely integrated, rather passive, peace-loving children, not outstanding in ability, content with their own company and their own ways of life.

### *Acknowledgements*

I wish to record first my grateful thanks to the Medical Research Council, who initiated the research and who have financed it throughout. Secondly, I am indebted to the staff of the two schools concerned for their unfailing help and co-operation during the research: they could not have been kinder.

My deep appreciation for all their hard work, often under difficult circumstances, goes to the members of the testing team: Mr. B. A. Akhurst, Mr. B. R. Barnett and Mr. A. G. Davey, who were chiefly concerned with the individual testing, and Mr. R. Fawcett, Mr. R. Ramsden and Mr. J. Ward, who assisted mainly with the group testing.

Finally I should like to express my gratitude to all the many others who have been in different ways involved in this research, and whose help has been so generously given.

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*Charlotte Banks with the assistance of R. Maliphant  
and C. P. Wallis*

## **Boys in Detention Centres**

The research reported here forms part of a larger investigation carried out by psychologists at University College, the aims of which are (a) to describe and compare boys between the ages of 16 and 20 sent to prison, to Borstal and to detention centres; (b) to investigate social and psychological characteristics related to reconviction after these punishments; and (c) to compare the effectiveness of the three types of punishment in terms of the numbers of boys not subsequently reconvicted after sojourn in each, while taking into account any differences between the kinds of boys sent to these three types of penal institution.

This paper is concerned mainly with 302 boys in senior detention centres\* who were interviewed and tested between July 1960 and February 1962. They were aged from 16 to 20, came from London and the Home Counties and were serving sentences of three months, with one sixth remission.†

The first detention centre for junior boys aged 14 to 16 was opened in 1952; the first centre for senior boys aged 17 to 20 in 1954. Unlike prison or Borstal, where 'trade training' is emphasized, no such training is provided in detention. The régime was originally intended as a 'short sharp shock' and lives up to this name; it consists of hard chores, strict discipline and vigorous athletics. It would appear, and indeed it is generally agreed, that boys need to be mentally and physically healthy to fit into the régime satisfactorily. The number of centres is at present increasing fairly rapidly, in accordance with the Government's intention, embodied in the Criminal Justice Act of 1961, to

\* The total sample is made up of 914 boys—302 from detention, 304 from prison and 308 from Borstal.

† The maximum single sentence that can be given is six months, but about 84 per cent of the boys committed to detention are sentenced to three months.

abolish short prison sentences for boys of this age, and to substitute detention wherever possible.

At the time when this research was begun, there was only one detention centre for senior boys, Blantyre House in Kent; Aylesbury and Aldington were opened in 1961. These were the first three centres available for senior boys in the South of England. By the end of 1961, the year when most of our work was carried out, there were altogether five senior centres in England and Wales, and in that year 1,676 boys went to them, compared with 2,565 who went to prison, and 3,588 who were sent to Borstal. With such a small number of senior centres (three in the South of England and two in the North) and such a large number of boys to choose from (7,829), it is not unreasonable to suppose that at that time the courts were selecting, on the whole, boys suitable for this type of institution. Unfortunately, however, no specialist's report—medical or otherwise—is required by law before a boy is so sentenced; thus, the policies of courts are bound to differ, and their success almost certain to depend on the amount of personal information they are prepared or able to elicit about a boy before consigning him to this form of punishment.

The present research is essentially psychological, based on case-studies, as far as time allowed; it depends largely on the results of personal interviews and standard tests administered individually.\* Three independent sources of information were available to us: the boys themselves, the homes of 80 per cent of them and official files.† The data obtained in the interviews with the boys has provided the basis for this paper, and has been checked, wherever possible, with that from the other two sources.

The plan of the psychological work and the methods owe much to the original and classic investigation of Professor Burt in 1922;<sup>2</sup> indeed, in some ways, the present research could be said to fall short of his. For example, Burt made psychological assessments of his delinquents on the basis of evidence obtained by him personally from the child, his home, his school and so on. Thus he was able to make his final decisions on first-hand information in every case. We, on the other hand, have had to be content with data obtained by different investigators from different sources—by psychologists from the boys

\* Each boy was seen for approximately five hours during a single day.

† We were extremely fortunate in having interviewers from Government Social Survey to visit the boys' homes and the permission of the Prison Commissioners and Home Office to obtain information from records.



themselves, by Government Survey interviewers from the boys' homes, and by the whole jumble of people who contribute from varying sources to the information contained in official records. Many modern students of criminology, and indeed of psychology, would think this a gain in 'objectivity' over more 'subjective' methods, and to be applauded. However, it is arguable that criminology is not at present a field where the modern passion for objectivity at all costs is of paramount value. A glance over present-day research suggests that the more objective psychological methods, questionnaires, laboratory experiments and so forth, when looked at fair and square, have contributed relatively little as yet to criminological knowledge; in fact, an alarming gap in present work in this field appears to be the lack of a psychological approach to criminals as individuals, combined with systematic recording and sound statistical techniques. There are, of course, a number of *psychiatric* studies of criminals, individual studies of great interest<sup>9</sup> where the criminals fit into currently defined abnormal, or semi-abnormal categories; but few psychologists, or psychiatrists for that matter, would claim this of more than 20 to 30 per cent of cases at most.

We do not, then, apologize for the more subjective and psychological aspects of our research—the interviews and the ratings based on them; we regret only that we could not have followed Burt's methods more closely. His work is still spoken highly of to this day, yet much of its intrinsic value—particularly the skilled study of cases—is in practice conveniently ignored.

The results reported here fall broadly under two headings—on the one hand, descriptive information about the boys and a brief comparison of this with similar results from other investigations; on the other, preliminary results of two years' 'follow-up' of the boys' convictions subsequent upon their sojourns in detention and the relation of these to the characteristics studied.

### *Descriptive Information*

For purposes of this paper the descriptive information falls into three broad classes—first, the boys' offences and their criminal potentialities; secondly, their home backgrounds; and thirdly, the unsuitability of some of them for detention, mainly on the grounds of mental or physical ill-health.

## I. OFFENCES AND CRIMINAL BACKGROUND

### OFFENCES

What sort of boys had we to choose from? What crimes had they committed?

In Table I below are all receptions into prison, borstal and senior detention centres in England and Wales in 1961, the year when most of the field work in the present research was carried out. 7,829 boys were received, and were classified according to the offences for which they had been sentenced (*Report of the Commissioners of Prisons, 1961*, slightly modified).

TABLE I  
*Receptions in 1961 classified according to offences*

	<i>Detention</i>	<i>Prisons*</i>	<i>Borstal</i>	<i>England and Wales Total</i>
Murder		2		2
Other violence against the person	191	314	220	725
Sexual offences	32	124	73	229
Breaking and entering	308	477	2,184	2,969
Larceny	520	589	682	1,791
Receiving	27	44	44	115
Frauds and false pretences	12	29	24	65
Other indictable offences	56	164	139	359
Assaults	53	124	8	185
Prevention of Crimes Act	25	60	16	101
Taking motor vehicle without consent	222	274	169	665
Malicious damage	27	31	3	61
Indecent exposure	6	6		12
Cruelty to children		2		2
Living on immoral earnings, etc.		6	2	8
Drunkenness		25		25
Highway Acts offences (Driving)	166	128	13	307
Begging, sleeping out		2	1	3
Other non-indictable offences	31	164	10	205
	<hr/>	<hr/>	<hr/>	<hr/>
	1,676	2,565	3,588	7,829

\* The figures include boys with prison sentences of less than three months and more than three years who were not included in the present research. They number approximately 500.

The fourth column in Table I gives the total numbers of boys received into these institutions during the year 1961, who committed the various offences. For indictable offences we can compare, with the help of *Criminal Statistics*, the number of boys incarcerated with the total number of boys of this age who were convicted of the offences, not all of whom were sent to penal institutions. For example, to take the more frequent of the indictable offences, the figure for breaking and entering, 2,969, represents about 45 per cent of all the boys who were convicted of this offence; 725 represents about 25 per cent of those convicted of indictable woundings and assaults (mostly in fights); 229 represents 21 per cent of all those convicted of sexual offences and finally, 1,791 represents 12 per cent of those convicted for larceny. As these offences are in order of the proportions convicted who are sent to these institutions (the main penal places for boys of this age), we get breaking and entering first, followed by violence, sex and larceny, an order which may (or may not) be indicative of the relative seriousness with which we, the British, regard these offences.

Table II gives the figures in percentages, and permits comparisons to be made of the offences committed by boys in the present sample, and by boys in the population from which they were drawn,\* with those for the whole country and for prison and Borstal.

The main contrast in the table is provided by the much larger proportion of Borstal than of other boys who had been found guilty of breaking into premises in order to steal. It appears that the courts sent 74 per cent of the 2,969 'breakers and enterers' to Borstal rather than to prison or to detention. Boys sent to Borstal are considered to be primarily in need of 'training'. In accordance with the Criminal Justice Act of 1961 these lads serve an indeterminate sentence, of which between six months and two years may be spent in the Borstal institution, depending on their behaviour while they are there, followed, on release, by two years under the eye of After-Care—usually a probation officer. According to official figures, about 60 per cent of Borstal boys are ultimately reconvicted after release,† a somewhat disappointing proportion, as the emphasis in Borstal more than in either prison

\* The population is defined as all boys from London and the Home Counties, sentenced to three months' detention and sent to Goudhurst, Aylesbury or Aldington between July 1960 and February 1962.

† See Report of the Prison Commissioners, 1961. Our own reconviction rates, one year after each boy's release, are for 302 boys in detention, 36 per cent; for 308 prisoners, 55 per cent and for 246 Borstal boys, 46 per cent. (There are still 62 Borstal boys who have not yet been 'out' for a year.)

TABLE II  
*Numbers of boys (expressed as percentages) committed to detention, prison and  
 Borstal in England and Wales, 1961, convicted of various offences\**

	Detention		England and Wales, 1961			
	Present Sample	London and Home Counties	Detention	Prison	Borstal	Total
Murder				<1		<1
Other violence against the person	6	5	11	12	6	9
Sexual offences	<1	<1	2	5	2	3
Breaking and entering	21	20	18	19	61	38
Larceny	30	29½	31	23	19	23
Receiving	4	3	2	2	1	1
Frauds and false pretences		1	1	1	<1	1
Other indictable offences	3	2½	3	6	3	5
Assaults	2	3	3	5	<1	2
Prevention of Crimes Act	1	<1	1	2	<1	1
Taking and driving a motor vehicle	14	16	13	11	5	8
Malicious damage	3	2	2	1	<1	1
Indecent exposure		2	1	<1		<1
Cruelty to children				<1		<1
Living on immoral earnings				<1	<1	<1
Drunkenness	<1	<1		1		<1
Highway Acts (driving, etc.)	12	10	10	5	<1	5
Begging, sleeping out				<1	<1	<1
Other non-indictable offences	3	5	2	6	<1	3
Per cent	100	100	100	100	100	100
Numbers	302	478	1,676	2,565	3,588	7,829

\* This table is a modified version of one that has already appeared in *Current Legal Problems*.<sup>1</sup>

or detention is on reform rather than punishment. However, boys convicted of breaking and entering are rather more likely to be subsequently reconvicted, whatever punishment they receive, than are those who commit other kinds of offence. This may have something to do with the disappointing results.

One other difference should be noted in Table II, namely the

relatively low figures, among detention centre boys, for indictable violence against the person, both in our sample and in the population of boys from London and the Home Counties (columns 1 and 2) compared with that for England and Wales as a whole (column 3). This is not, as might appear, a regional difference due to variations in the criminal behaviour of boys in and outside London and the Home Counties, nor is it likely to have anything to do with variations in the sentencing policy of the courts in these two areas. Our random sample, and the population from which it was selected, contained only boys sentenced to three months' detention, who constituted about 84 per cent of the total number committed. Of the remaining 16 per cent, nearly all had been given sentences of six months, and a rather higher proportion of these boys had been convicted of indictable wounding against the person.

However, the present sample proved not to differ significantly from boys in London and the Home Counties who were not chosen, and the population of boys from this area may be said to correspond fairly closely to that for the country as a whole, as far as crimes are concerned.

It is worthy of note that 33 of the 302 boys asserted their innocence of the offences of which they had been convicted. We believed 10 of them.

#### CRIMINAL BACKGROUND

By criminal background we mean first, the number of occasions on which the boys have been convicted in the past, and secondly, their criminal associates—relatives and others—and their attitudes to their offences.

##### *Number of Previous Convictions*

We have two sources of information about previous convictions—the boys' own statements and official reports based on criminal records from Scotland Yard.\* Below, in Table III, the data from the two sources are compared. The correlation between them is .71.

It can be seen that, on average, the boys claimed more previous convictions than were allotted to them by official sources. Twenty-four understated their previous record, 143 gave numbers identical with the official ones, while 135 appeared to over-state the case. No such bias is

\* We are most grateful to the Home Office Research Unit and the Criminal Record Office for obtaining these details for us.

observable in the results for the 304 young prisoners, although the corresponding correlation is virtually identical. The boys in detention were, in general, younger than the prisoners, had fewer previous convictions, and were considered by the psychologists to be less criminally inclined. They showed, too, a much lower reconviction rate a year after release, 33 compared with 55 per cent of prisoners. It is highly likely that they were more honest in recounting their past convictions and may have owned up to minor incidents that had not reached Scotland Yard at all. In addition, a few were Irish and claimed offences committed in that country that were unknown to the British police. In any case, it is not as easy as might at first appear for a boy to give a completely accurate account of his criminal record, even if he wishes to be honest, for he may confuse offences committed or tried

TABLE III  
*Number of previous convictions (a) claimed by the boys, and (b) available in criminal records*

		Numbers in official files										
		0	1	2	3	4	5	6	7	8	9	
<i>Numbers claimed by the boys</i>	10		1				1				1	3
	9				2	2						4
	8						1					1
	7				3	1	2		1			7
	6		3	2	3	3	4	2	1			18
	5	1		1	4	3	2			1		12
	4	2	2	14	7	7	3	2		1		38
	3	2	15	12	25	2						56
	2	3	23	29	5	1						61
	1	17	48	3	1							69
	0	29	4									33
		54	96	61	50	19	13	4	2	2	1	302

on the same and on different occasions, especially if these go back to the age of eight or ten.

### *Criminal Associates and Attitudes*

These are perhaps best shown by the correlation table on page 181 (Table IV). This table does not include all the characteristics assessed, but it gives what appear to be the main findings, and indicates briefly an intelligible pattern of clusters and corresponding factors.

At the foot of the table are the numbers of boys claiming each characteristic, and in Table V are the factor saturations obtained by simple summation.<sup>3</sup>

TABLE IV  
*Inter-correlations*

	1	2	3	4	5	6	7	8	9
Boy thought his sentence fair	—	.25	.55	.46	.03	.24	.15	.10	—
He blamed himself for the offence	.25	—	.40	.59	.16	.12	.06	.10	—
He thought the findings of the court fair	.55	.40	—	.24	.30	.03	.07	.07	.05
He admitted undetected crimes	.46	.59	.24	—	.26	.51	.32	.25	.28
He thought he would be in trouble again	.03	.16	.30	.26	—	.27	.27	.23	.27
Said he knew receivers	.24	.12	.03	.51	.27	—	.63	.22	.30
Said he knew adult criminals	.15	.06	.07	.32	.27	.63	—	.36	.35
Said he had relatives who had been convicted	.10	.10	.07	.25	.23	.22	.36	—	.68
Said he had relatives who had been 'inside'	.30	.28	.05	.28	.27	.30	.35	.68	—
Numbers of boys claiming each characteristic	159	187	266	147	97	155	105	135	86

TABLE V  
Factor saturations—simple summation

	I	II	III
Said he had relatives who had been 'inside'	.41	.64	.35
Said he had relatives who had been convicted	.54	.35	.32
He thought he would be in trouble again	.43	.10	.18
Said he knew receivers	.65	.26	.41
Said he knew adult criminals	.60	.38	.21
He thought the findings of the court fair	.47	.41	.47
Boy thought his sentence fair	.41	.50	.03
He blamed himself for the offence	.36	.46	.04
He admitted undetected crimes	.83	.31	.29

The clusters of correlations are fairly distinct and intelligible. Unfortunately we have not had time to rotate to a group factor solution. According to the analysis by simple summation the first factor is a general factor, which appears to be criminality in so far as this was claimed by the boys. It is best represented by the admission of undetected crimes and of acquaintance with adult criminals and receivers. This factor contributes 29 per cent to the total variance.

The second factor distinguishes boys with criminal contacts, in and outside the family, from those who said they accepted the blame and punishment for their present offence. The factor contributes 16 per cent to the total variance.

Finally, the third factor, which contributes only seven per cent, distinguishes, on the one hand, criminal contacts within the family, thinking the courts findings fair and that one may offend again, from criminal contacts outside the family and admission of undetected crimes, on the other.



## 2. HOME BACKGROUND

We have defined an 'unbroken home' as a home where both the boy's own parents, married to each other, are living together, whether or not

TABLE VI

		<i>Boys in Detention</i>
Mother known to be dead		19
Father known to be dead		39
(Father died of war injuries)		(10)
Both known to be dead		<u>4</u>
		62
Parent(s) deserted, separated or divorced		85
Mother deserted		5
Father deserted		6
Both deserted		<u>8</u>
		19
Parents separated or divorced:		
Mother left		23
Father left		<u>43</u>
		66
Boy is illegitimate		9
Boy suspects he is illegitimate		<u>12</u>
		21
Adopted		8
Fostered		<u>10</u>
		18
Has mother and stepfather		62
Has father and stepmother		29
Has only mother		35
Has only father		9
Has only substitute parents		<u>23</u>
		—
		<i>Per</i>
Total number of boys with:		<i>cen</i>
Broken homes	134	(44)
Unbroken	168	(56)

the boy lives with them. Any other states of affairs we have judged as 'broken homes'. Homes may be broken in a number of different ways—death, desertion, divorce and so on; and some of these occurrences may overlap. A boy's father may have deserted and since died, or the mother who did not desert the boy may have remarried and then died herself, or both parents, after divorce may have remarried and so on (Table VI).

An interesting fact emerges on comparing the numbers of boys living away from broken and from unbroken homes. If for this purpose we exclude the married boys, we are left with 294. Table VII below gives the results.

TABLE VII

	<i>Home not broken</i>	<i>Broken home</i>	
Away from home	35	45	80
Not away	130	84	214
	—	—	—
	165	129	294

The figures are significant ( $\chi^2$  gives  $p < .02$ ) and suggest an association between having a broken home and living away from it.

Dr. Mannheim and Mr. Wilkins<sup>7</sup> found that although more boys entered Borstal from broken homes than were to be expected, those from broken homes were no more likely to be reconvicted after release than those from ordinary homes. Our own results support these conclusions.

An association between 'broken home' and delinquency has been well known to psychologists for many years and appears to stand the test of time. Unfortunately, however, neither in Mannheim's and Wilkins' research nor in the present investigation was a non-delinquent control group available. Mannheim and Wilkins compared the homes of their Borstal boys with those of 1,390 adolescents, subjects of an investigation carried out by Government Social Survey, the results of which were reported by Wilkins in 1955.<sup>10</sup> Approximately 22 per cent of the Borstal boys had a 'broken home' compared with 14 per cent in the adolescent sample. However a 'broken home' was defined as a home from which one parent (or substitute parent) is missing—a home, in fact, which contains only a father (or step-father) or a mother (or step-mother). Thus, a home containing a mother, step- or substitute father and siblings was classified as 'unbroken'. This is unfortunate, for

as long ago as 1925 Burt in his study of young delinquents drew attention to the 'marked recurrence of . . . the presence of a foster-parent . . . in most instances a step-mother, in others, a step-father . . .'. In addition, the Social Survey classification, although headed 'family structure', dealt only with the family structure of homes in which the young people were living. As all but three per cent of the ordinary adolescent sample *were* living at home, this was of small importance for them; but as many as 27 per cent of the Borstal boys were living away from home, thus, in their case, this classification led to a considerable amount of information being lost. In Table VIII below are percentages given by Social Survey and by Dr. Mannheim and Mr. Wilkins; our own figures for boys in detention have been included for comparison.

TABLE VIII

<i>Family structure</i>	<i>Normal adolescents</i>	<i>Borstal boys</i>	<i>Boys in detention centres</i>
Only child with both parents	17	6	2
Mother, father and siblings	62	45	53
Father and stepmother			
Mother and stepfather			
No father	11 } 14	17 } 22	11 } 15
No mother } broken home			
Not living with either parent	7	27	30
per cent	100	100	100
numbers	1,390	572	302

There are even fewer 'only children' among the boys in detention than in the normal sample; the percentage (55) of detention centre boys living in 'unbroken homes' is very close to that of Borstal boys, lower than the figure for adolescents; and the number of boys from homes where one parent is missing is almost identical with that in the normal group. Accepting, as we have done in this instance, the rather narrow definition of 'broken home', we get a figure of 15 per cent, almost identical with that of the Social Survey adolescents. But our results, when compared with the figures for the adolescents, show a significant difference over the whole table, due largely to the much greater number of boys not living with either parent.\*

\* We have classified 'only children at home' with 'unbroken homes' and described boys who have only substitute parents as 'not living with either parent'.

However, if we use the definition of a broken home given on page 184 of this paper, a definition customarily used by psychologists, psychiatrists and social workers, namely—a broken home is one which does not contain both natural, married parents—then we obtain interesting results, comparable with those of other investigators. Our over-all figure for broken homes becomes 44 per cent, very close to the 46 and 45 per cent obtained by Dr. Gibbens<sup>5</sup> and by Dr. Rose<sup>8</sup> for Borstal boys. Dr. Gibbens subdivides broken homes into those broken by death and those due to separation or divorce. We give below, in Table IX, figures (in percentages) based on this broad classification from a number of investigations, some with control groups, some not, and in addition the results of a recent survey in Bethnal Green reported by Dr. Michael Young and Mr. Wilmott<sup>11</sup> and, more recent still, unpublished figures from a national survey carried out by Dr. R. J. W. Douglas.\*

There are two obvious trends observable in the table; first, judging by dates of birth, the steady decrease among the 'non-delinquent' groups in the percentage of homes broken by death, from the beginning to the middle of the century; and secondly, the considerably larger percentage of homes broken by separation or divorce among the delinquents than among the other groups.

#### HOMES BROKEN BY DEATH

Of the 'non-delinquent' subjects† born early in this century and seen by Burt and by Young and Wilmott, 17 and 19 per cent came from homes broken by death; by the time Rhodes' control group is reached the figure has dropped to 10 per cent, and in the national sample, born in 1946, it is as low as 4 or 5 per cent. This rather striking decrease has a considerable effect on the corresponding total percentages of broken homes, to which death seems to have contributed so much more in the earlier days; and it seems to illustrate Professor Titmus' assertion, quoted by Young and Wilmott, that 'it is highly probable that the proportion of broken marriages . . . is, in total, smaller today than at

\* I am much indebted to Dr. R. J. W. Douglas of the M.R.C. unit at the London School of Economics, and to Dr. Hammond, of the Home Office Research Unit, for the opportunity to use these results.

† Dr. Douglas' survey included some delinquent children and no doubt there were a few ex-delinquents in the Bethnal Green Group. The proportions, however, are likely to be very small, I have called the samples 'non-delinquent' to distinguish them from the specifically delinquent boys seen by Burt, Rhodes, Gibbens and by us.

TABLE IX

Figures (in percentages) of homes broken by death and by separation or divorce among delinquent and non-delinquent groups

Born circa—	1907-1914		1906-1920		1929-1937		1933-1938		1941-1944		1946	
	Burt* Del. Controls	Young & Wilmott	17	19	17	19	Rhodes† Del. Controls.	Gibbens‡ Borstal	Detention Centre	Total Sample Industrial Manual Workers	Industrial Manual Workers	National Survey Number of Industrial Manual Workers weighted up
Home broken by—												
Death	21	17	17	19	13	10	18	19	(14½)	4	5	4
Separation or Divorce	12	3	1	1	10½	3	26½	25	(22)	5	4	5
Total broken	33	20	18	20	25	13	44½	44	(36½)	9	9	9
N	197	200	273	294	989	1,000	200	302	(273)	2,400	>6,000	1,322
Ages	8-15	8-15	Adults	8-16	8-16	17-20	17-20	17-20	17-20	15	15	15
Column number	1	2	3	4	5	6	7	8	8	9	10	11

\* Professor Burt's figures for broken homes and for parents separated or divorced may be a very slight underestimate, for it excludes a small number of illegitimate boys for whom he does not give details. The same applies to Dr. Rhodes' numbers for 'separated and divorced'—we have excluded 'other' which includes illegitimate boys.

† Dr. Rhodes (4) gives percentages of deaths, separations and divorces only for boys in his Class 'D', that is for boys from broken homes with one head of household. I have assumed the same proportions in his Class 'C', boys with broken homes and two heads of household (one or both not a natural married parent), and estimated the figure given above.

‡ I conclude that Dr. Gibbens has used 'separation' to cover cases where the boy is illegitimate and where the fate of the parents is unknown. We have a small proportion of cases of this kind and have classified them with 'separation'.

any time this century'. As Young and Wilmott go on to say, 'Disease is less deadly. Childbirth is less dangerous. The Second World War killed far fewer fathers than the First.' Thus, the numbers of broken homes, in non-delinquent control groups and in the general population, appear to have decreased considerably, entirely, it would seem, on account of a marked decrease in the death-rate of parents.

#### HOMES BROKEN BY SEPARATION OR DIVORCE

In Burt's and in Rhodes' groups considerably (and significantly) more delinquents than non-delinquents had homes broken by separation or divorce. Can a similar, more up-to-date comparison be made, between our own group of boys in detention and the recent figures from the National Survey?

The percentage of boys in detention with homes broken by separation or divorce is almost identical with that obtained among Dr. Gibben's Borstal boys. However, these are all incarcerated delinquents (thus not strictly comparable with Burt's and Rhodes'); they are older than the boys in Dr. Douglas' survey, who were born after the War, and who were only followed up to the age of 15; and they include twins and illegitimate boys who were not included in the National Survey. In our own sample, therefore, we have excluded boys whose homes were broken after they were fifteen years of age, boys whose fathers were killed in the War and illegitimate boys.\* Recalculation of the percentages gives the figures in brackets in column 8.

Dr. Douglas took originally all the children born in England and Wales between the 5th and 9th of March, in 1946, and then followed up, to the age of 15, a randomly selected 1 in 4 of those who were children of industrial manual workers (by far the largest group) and all the rest. The percentages in the last three columns of Table IX are given for: (a) all the boys followed up, those with fathers employed in industrial manual work represented as 1 in 4 (column 9); (b) all the boys followed up, the numbers of children of industrial manual workers being multiplied by 4, to give a more representative proportion (column 10); and (c) all boys who were children of industrial manual workers only (column 11). It is evident that it makes very little difference to the percentages which group one takes for comparison.

It seems that the boys in detention, born a short time before the boys

\* We have no twins in the sample and the number of boys who have a twin not seen by us is less than 1 per cent.

in the National Survey, have among their numbers rather more than three times as many whose homes have been broken by separation or divorce.

The difference in time—our boys were born on average about 4 years before Dr. Douglas'—might have widened the gap a little, and it may be, too, that the detention centre boys are drawn, on the whole, from somewhat lower and less fortunate social strata than were the boys in the National Survey. A further possibility must also be considered, namely, that some of the 'separations' have been missed in the very large National Survey, but not among the boys in detention, who are smaller in number and perhaps better documented on the whole, probably as a direct result of their offences. All three factors, if operating, would artificially increase the observed difference, but it is hard to see how their correction could reduce it to anything approaching insignificance. They serve, however, to emphasize the advantage of using matched control groups in addition to national figures such as those of Dr. Douglas. Nevertheless, his data should prove invaluable to all research workers wanting to compare their own results with a large, representative sample such as his.

In conclusion, more research needs to be done to study what appears to be a much larger proportion of homes broken by separation and divorce among delinquent boys in detention (and among Borstal boys) than that found in a large, national sample.

#### DIFFERENCES BETWEEN PERCENTAGES OF HOMES BROKEN BY DEATH AMONG DELINQUENTS AND NON-DELINQUENTS

Professor Burt and Dr. Rhodes found small differences of this kind in favour of non-delinquents. Our own figures, when compared with those of the National Survey, show a bigger discrepancy in the same direction—a difference of about 10 per cent compared with one of 3. However, older incarcerated delinquents (in detention or Borstal) may well have a rather larger percentage of dead mothers and/or fathers than the younger, more representative delinquents of Burt and Rhodes, and, in addition, the death rate of parents in the National Survey may be somewhat lower than that which would be obtained in a matched control group. Anomalies such as these could have contributed to the observed difference and it is not possible at this stage of our research to distinguish between them.

#### CONCLUSIONS

1. The death rate of parents in the general population appears to have decreased considerably during the first half of this century, and, since

it contributed a great deal in the early days to the break up of homes, seems to have been accompanied by a fair-sized reduction in broken homes.

2. There appears to be a generally low relation between homes broken by death and delinquency as defined by Burt and Rhodes, such a relation being somewhat higher when boys in detention are compared with recent national figures. The reasons for this increase, however, cannot at this stage be established.

3. There seems little doubt that a high percentage of incarcerated delinquent boys aged 17 to 20 come from homes broken by separation compared with boys in Dr. Douglas' National sample. Though the National Survey is not an ideal control group for our own sample, it is unlikely that this has substantially increased the large difference observed.

4. Boys in detention tend to be away not just from their homes, but from 'broken homes', and nearly a third of boys from such homes have already been 'in care' for a year or more. Moreover, a greater number of boys away from home tend to get themselves into Borstal than would be expected (Dr. Mannheim and Mr. Wilkins' sample).

All these facts suggest that special care is needed to discover the nature of the home difficulties of younger boys who come before the courts either for an offence, or beyond control or in need of care and protection; to determine the effects of such family difficulties on the boys themselves; and, finally, to do the utmost possible to resolve the problems at an early age and to help the boys to steer clear of trouble.

Finally, this attempt to compare 'family composition' in only a small number of researches has made it quite clear to us that comparison would be much easier if investigators would keep separate in their analyses first, the fate of the natural parents, secondly, whether or not the boy is away from home, and thirdly, the structure of what he regards as his home whether or not he is away from it.

### **3. BOYS CONSIDERED UNSUITABLE FOR DETENTION**

On the basis of all the evidence available—in most cases from the boy, his home and from official records—we have classified those boys, 78 in number, whom we considered ought not to have been sent to a detention centre. The details are given in Table X below.

Nearly all these boys presented a variety of conditions, but we have tried to classify each according to the main characteristic that he



TABLE X

<b>A. Not really criminal</b>	
i. believed to be innocent of the offence(s) of which they were convicted	10
ii. sentence considered too severe	11
	<u>21</u>
 <b>B. Unsuitable for the régime in detention centres</b>	
i. <i>Suffering from physical handicaps</i>	
foot sores and swellings	1
limp	4
hand injury (permanent effects)	1
asthma	1
asthma, eczema and history of T.B.	1
epileptic*	3
probably epileptic	3
severe head injuries possibly related to gross instability	5
	<u>19</u>
ii. <i>Suffering from severe psychological handicaps</i>	
psychotic	2
indications of psychosis	2
neurotic	23
hostile and aggressive	8
very anxious and/or depressed	7
grossly deprived	4
very withdrawn	2
other	2
very unstable	3
'psychopathic' (could be dangerous)	2
borderline defective	2
enuretic	3
took drugs regularly	1
	<u>38</u>
Total	78
	<u>        </u>
Percentage of 302	26

\* One epileptic boy has been classified under Aii above, making four in all.

presented and have given priority to the various features in the order of the sub-divisions in which they are presented. For example, some boys classified under A showed symptoms that fell under Bi or ii, or both; and some with physical handicaps classified under Bi also presented psychological symptoms classifiable under Bii but these secondary features have been omitted. Thus there is no overlap in the table, that is to say no boy has entered more than once. And we have included only the more extreme cases of what we considered to be injustice or handicap. We have selected a few for fuller description by way of illustration.

The boys we considered innocent or whose sentences we thought were too severe, need little explanation. A number of the latter had been convicted of motoring offences (driving without insurance or while disqualified), and were not suspected of more serious crimes. Some were first offenders, one an illiterate, mentally defective, epileptic boy with very bad sight, who had been described by a chief constable as 'a local ringleader'. Another, who was almost completely deaf, deserves special mention. He could hear virtually nothing, could only lip-read very slowly, and was in constant difficulty at the centre because he couldn't hear any of the orders. This was a perpetual source of anxiety to him. His wife had had her first baby a week after he came to detention. His offence was driving while disqualified. According to him his disqualification was 'up' the day after he committed the offence. He had been to fetch his motorbike and was pushing it home on a hot evening when he succumbed to the temptation to ride it the rest of the way.

Fortunately, since this research was carried out, Section 110 of the Road Traffic Act has been amended, a fact which may, it seems, reduce the number of custodial sentences imposed for driving while disqualified and facilitate the imposition of fines for this offence. This should assist in keeping boys with nothing but convictions for motoring offences out of institutions where they meet boys experienced in more criminal behaviour.

#### PHYSICAL HANDICAPS

The most important group in Table X are undoubtedly the boys suffering from physical handicaps, for they form the most obvious group who should not have been sent to a detention centre at all. Physical handicaps are, on the whole, easier to observe than innocence or psychological abnormalities, and in the absence of medical reports, courts should, perhaps, make special efforts to detect them by asking the pro-

bation service for information, or even watching the boy walk into the dock. Of the four boys who limped badly, one had suffered from a tubercular knee which was seriously scarred, another was a victim of the after-effects of poliomyelitis (this boy appeared, too, to be seriously depressed). The boy with the history of tuberculosis, who had eczema and asthma as well, was recorded by the medical officer in the prison where he was remanded as unfit for detention because he had a history of spitting blood. He went to detention nevertheless.

The four epileptic boys had all been previously diagnosed as such. This information was contained in their past records, and presumably could have been made available to the courts who sentenced them. We thought that one of them had been given too severe a sentence quite apart from his epileptic condition. Two were illiterate, of very low intelligence, and were, according to their own accounts, bullied unmercifully by the tougher boys.

We were of the opinion that boys with histories of 'black-outs' or severe head injuries, who showed possible symptoms of brain damage or uncontrollable behaviour, should have been investigated medically before being committed to detention. To give but one example—a boy whose responses in the interview were obviously delayed, and who, according to him and his mother, had suffered from dizziness and severe headaches since he sustained a serious head injury at the age of seven. He had been playing on an old-fashioned iron mangle which fell and 'scalped' him, an accident which necessitated 48 stitches in his head. He was in hospital for six weeks, and although his skull was not fractured, he had suffered haemorrhage from eyes and nose.

#### PSYCHOLOGICAL HANDICAPS

Two boys we considered to be actually psychotic at the time of interview. Both have since been reconvicted, put on probation and have accepted conditions of residence at mental hospitals.

We give below brief outlines of one of these boys, and of others who presented, in our view, psychological symptoms which made them unsuitable for the régime in detention centres.

1. One of the boys considered by the psychologists to be definitely psychotic showed schizophrenic symptoms. In the interview with him it was impossible in the whole five hours to get a direct answer to any question. His replies were full of irrelevancies on politics and literature, and also 'wise' sayings—for example, when asked what made a person a criminal, 'It depends on who bends the tree, not the tree itself.'

Other replies were clearly delusional—he claimed to have fallen 40 feet once when climbing Ben Nevis, and he described how up to the age of 14 he was convinced he could live under water. He also said he had attempted suicide on a number of occasions by throwing himself out of windows and by jumping off a ship.

Little can be firmly established about his early background. He makes the unlikely claim of having been born in Alaska, but has an Irish name and has lived in Ireland. He appears to have been adopted. At the age of six he went into a children's home for four years because, he says, his parents were separated, and when he was ten he went to live with friends, and finally joined the Royal Navy at sixteen. Here he seems to have had a turbulent career, being put in naval detention, and having some form of psychiatric oversight in a Royal Naval hospital. He was finally discharged from the Navy as unsatisfactory. Since his discharge he has drifted around with no fixed abode, having no contact with friends or relatives and maintaining himself by doing odd jobs in London. He has no record of previous offences, and his present offence demonstrates his inadequacy as a criminal. Having robbed the porter of his hostel with a threat of violence, he returned quite openly to the same hostel the following evening and stole some property while the police were being called.

At the detention centre he was wild and uncontrollable and was placed on report many times answerable to charges of violent and insubordinate behaviour. From the centre he was admitted to a mental hospital, where he received E.C.T. and drug therapy. Unfortunately he discharged himself at the end of his sentence before the completion of his treatment. The psychiatrist at the hospital reported that he was very anxious about the boy and foresaw the possibility of a recrudescence of criminality, violence and overt psychotic behaviour. In the event of the boy being reconvicted, he was of the opinion that the question of certification or committal to an institution like Broadmoor might have to be considered. The psychiatrist concluded, 'he (the boy) unfortunately belongs to a class of people who can only be dealt with after some damage has been done, as at the moment we couldn't find enough to certify him, nor would any tribunal uphold a certificate.' Fifteen months after his release from detention this boy was reconvicted of breaking and entering, and taking and driving a motor vehicle. He was put on probation for two years and accepted a condition of residence in a mental hospital.

2. A neurotic boy, described by the psychologist as very aggres-

sive, presenting so-called 'psychopathic' characteristics. He seemed to be a pathetic victim of rejecting, violent parents who showed a complete inability to look after themselves, let alone their children. His mother is dull and inadequate, his father lazy, drunken and violent and his elder brother mentally defective. At the age of nine the boy was committed to the care of the local authority for being beyond control, and was in children's homes for the next six years. In the homes he appears to have been quite uncontrollable, fighting with the staff, smashing things and absconding (according to official records) about 140 times. On his subsequent return home the same pattern of violence was repeated, and this behaviour has led to dismissal from most of the 45 or so jobs he has held. He drinks every night and often gets drunk, a condition in which he claims he is not violent. He confesses to difficulties with girls with whom, he says, he is too shy, and has a hatred of homosexuals, who, he claims, have made advances to him. His criminal career started early—at the age of nine he appears to have been a bold and accomplished shop-lifter—and he had three previous convictions prior to the current offence of breaking and entering. A very deprived, aggressive, uncontrolled boy, he rejects his parents and they reject him. He shows strong resentment against anyone who attempts to assist him.

This lad was given a poor prognosis by the psychologist. He has subsequently been reconvicted on five charges, including one of violence, and sent to Borstal.

3. One boy with a long history of fits, dizzy spells and black-outs had already been diagnosed as epileptic by a psychiatrist in an approved school who recommended treatment, which the boy apparently never received. The psychologist thought that he showed psychotic and psychopathic characteristics. He was an unwanted child who had been denied any parental interest and affection. His father died in the war, and he has had two step-fathers. The first of these was cruel and violent, assaulting the children and inflicting humiliating punishments on the boy, finally causing the family to be evicted by refusing to pay the rent. The second step-father threw the boy out of home at the age of 13 or 14. The mother, described as keeping 'an intolerably filthy and poorly furnished home', appears completely to have rejected him. His two younger siblings have been taken into care under a 'fit person' order and have been subsequently convicted of offences and sent to penal institutions. It would appear that for this boy nature has been no kinder than nurture. Blind in one eye, he has a long record of

severe headaches, discharging ears, fainting spells, nightmares and periods of extreme depression in which he has attempted suicide. He has had severe pneumonia and was reported by a prison Medical Officer as unfit for a detention centre because of a history of spitting blood. His criminal record includes gross and apparently meaningless offences—indecent assault on a five-year-old girl, stealing a fishing boat and so on—crimes which seem to bear witness to his mental instability. He has drifted from job to job, until finally a family befriended him and took him into their home. He stole from them and made their daughter pregnant. Following release from detention he committed two further offences and was sent to Borstal in 1962.

The psychologist considered the boy extremely unstable, oscillating between periods of severe depression and outbursts of violent and uncontrollable temper. A very deprived and withdrawn boy, he is quite unable to cope with the many adverse features of his life. He was thought to be in need of further investigation into his condition, and of skilled medical and psychological attention.

4. A very deprived, enuretic boy considered to be in need of skilled psychological attention for severe depression. He suffered from extreme frustration and bitterness at having no home and no family interested in him. He was an illegitimate child whose parents, though living together, never married. Between the ages of 14 months and seven years he was in hospital with coeliac disease, dysentery and mastoids. He returned to a discordant home, to parents who often quarrelled violently, and who deprived him of interest and affection. His father, who was sent to prison for two years during this period, inflicted harsh punishments on the boy for trivial misdemeanours and sometimes for none at all, and when he was eleven his mother had him placed in a children's home because he stole. She seems to have had little affection for any of her nine children. She said, for example, that she did not believe in keeping any of the children at home unless 'absolutely necessary'; even after they had been to the dentist to have teeth out, they were not allowed to stay at home during the day. The boy's mother subsequently left his father because of his cruelty, and married another man.

Since leaving the children's home at seventeen the boy has seen virtually nothing of his parents. He joined the Merchant Navy for a while, and afterwards lived in many different lodgings. He has a hatred of his father and is bitter and resentful towards his mother, feeling, rightly it would seem, that she has rejected him. His stealing

may well be symptomatic of his personality difficulties. His mother reports that in the hospital where he spent over six years the children owned their toys in common, and on returning home he treated his siblings' toys as his own—evidence, she says, that in hospital he had been 'thoroughly spoiled'. She said this 'toy stealing' was the start of the trouble. Shortly after being sent to the children's home the boy attended a Child Guidance Clinic for a couple of years because of his habit of stealing. His criminal record consists of small and apparently senseless thefts from an employer and from a landlady who had rejected him, crimes for which he made little attempt to avoid detection. Indeed, there runs a strong self-punitive element throughout his behaviour; though a boy of rare ability, his ambition runs no higher than worthless labouring jobs. The psychologist thought him immature, with a strong sense of inferiority, but considered that his criminal habits were not well established. Fortunately, in the year following his release from detention, this boy has not, according to criminal records, been reconvicted of any further offences.

5. A boy with a history of 14 years institutional care, described as very 'inadequate' and 'deprived' and unable to cope with the difficulties in his environment. Since the age of two, when first committed to the care of the local authority, he had been in eight or more children's homes, from some of which he absconded. An attempt to place him in a foster-home was a failure; so, too, was his attempt to return to his mother when he was seventeen. His mother has spent nine years in mental hospitals, and frequently her condition is such that she is incapable of recognizing her own son, an apparent rejection which the boy finds deeply wounding. His father, whom he hardly ever saw, died when he was eight, and his mother's subsequent three 'husbands' have shown no interest in him. Latterly he has drifted around with a group of 'beatniks', frequently sleeping rough, taking drugs and moving from one casual job to another. He has had five previous convictions of a very minor kind, mostly loitering and vagrancy. A very depressed boy, who has attempted suicide, he seems almost entirely submerged by his problems, and was deemed by a psychiatrist at a remand home to be in need of psychiatric care. The psychologist wrote, 'He will attach himself to anyone who shows any interest in him. Not a criminal boy, but might be led by others into crime, or steal in order to survive'. Since his release from the detention centre the boy has been sentenced to a month's imprisonment for stealing a bottle of milk.

## *Subsequent Conviction*

One hundred boys (33 per cent) were reconvicted within a year after their release, 143 (47 per cent) within two years. Sixty-six of the characteristics assessed in the interview and in the psychologist's ratings were found by  $\chi^2$  to be significantly related to reconviction one year after the boys' release from detention, and 64 were significant after two years. Despite the fact that, during the second year 'at risk', 43 more boys were added to the 'failures', 75 per cent of the characteristics significant after the first year remained so after the second. Briefly, if only the characteristics significant beyond  $p = .02$  on *both* occasions are included, then the boys reconvicted during a two-year period following release from detention presented one or more of the following features.

### CRIMINAL BACKGROUND AND ATTITUDE TO CRIMES

They thought their sentence (to detention) was fair, claimed to know 'receivers' and had previously been in a penal institution. The psychologists rated them as 'interested in ways of obtaining easy money' considered that their criminal habits were unlikely to change and that they were unlikely to achieve their ambitions within the framework of the law.

### SCHOOL

They hadn't gone regularly to school and had often truanted.

### HOME

They had, at some time or other, been thrown out of home and had slept rough; they hated or were indifferent to their fathers,\* and had no respect for their opinion or judgement. They had fathers who were violent, and angry with the boys for 'getting into trouble'.

### PERSONALITY

They were rated by the psychologists as immature, opposed to all forms of authority, aggressive and liable to frequent temper outbursts. They were thought to have been severely deprived of parental interest and affection, to be self-centred and to have little regard for others. They were probably evasive and not always honest in the interview and seemed incapable of forming a normal close relationship with someone else.

\* 'Fathers' includes step- or substitute fathers.



## NEEDS

The psychologists considered them unlikely to benefit from ordinary penal training, and in need of individual attention with case work. Both they and their parents were thought to be likely to need much help and support after the boys' release from detention.

## PSYCHOLOGISTS' PREDICTION

They were rated as likely to be convicted again.

It has not yet been possible to inter-relate all these characteristics, nor to relate them to others not significantly associated with reconviction. Thus, at the present time we do not know (a) to what extent they over-lap with each other, or (b) what wider clusters or factors they may represent.

Judged by the simple relation of each to subsequent conviction, the general pattern would appear to include unsatisfactory situations at home, particularly with regard to fathers and their substitutes; interest in, and experience of crime for gain, coupled with criminal contacts; a good deal of aggressive instability, and deprivation of affection; and considerable need for more individual care during and after punishment than is or can at present be provided by the ordinary detention centre and statutory after-care.

Dr. Max Grunhüt<sup>6</sup> in his interesting analysis of the records of boys in both junior and senior detention centres tested the relation between, on the one hand, subsequent conviction after a period of two years, and on the other, number of previous convictions, type of punishment received for these, and personal background. He found all three of these characteristics significantly associated with reconviction among junior boys, and the first two significant among the seniors—the group directly comparable with our own.

Our results confirm an association between 'failure' or reconviction and past residence in penal institutions. However, we find numbers of previous convictions unrelated to subsequent 'failure', both one *and* two years after release; and our results provide, too, some evidence that poor family background is related to 'failure' among boys of this age, whereas Dr. Grunhüt found 'personal background' so related among junior, but not among senior boys. However, his assessment of 'personal background', which was based largely on probation reports, combined facts about home, work and leisure, whereas our significant figures are for home only. This could easily account for the difference between his finding and our own. In addition, when his research was

carried out, between 1952 and 1954, a sizeable proportion of boys in detention were from the Army, where they had been doing their national service, whereas our sample contained no boys of this kind. In this sense then, the two samples are barely comparable, a fact which might also contribute to the difference between our results and his.

## *General Conclusions and Suggestions*

### **OFFENCES AND CRIMINAL ATTITUDES AND CONTACTS**

Boys in detention tended to be younger and less criminal than young prisoners. They had fewer previous convictions, and were more honest about them. The results of a factor analysis gave a factor of general criminality, best represented by confession of 'undetected' crimes and acquaintance with adult criminals and receivers. Further factors suggested sub-classification into boys who accepted blame and punishment on the one hand and on the other those with criminal contacts in and outside the family.

### **FAMILY BACKGROUND**

Nearly half the boys came from families broken by death or separation, a proportion almost identical with figures for Borstal boys. Comparison with results obtained by others indicates that death of one or both parents has little to do with delinquency or with entry into Borstal, though it may be associated with committal to detention. Separation and divorce are certainly so related.

### **SUBSEQUENT CONVICTION**

The boys have been followed up for one and two years after release. Of 66 characteristics significantly related to 'failure' during the first year after release, 50 remained so after two years. Twenty-seven of these were significant beyond  $p = .02$  on both occasions. Broadly speaking, boys reconvicted are characterized by unsatisfactory home relationships, particularly with fathers; criminal contacts and interest in and experience of crime for gain; by aggressive instability, deprivation of interest and affection, and need for more individual care during and after punishment than is or can yet be provided.

## BOYS UNSUITABLE FOR PUNISHMENT IN DETENTION CENTRES

Perhaps the most disturbing fact of all is that the courts, when in a position to select a small number of boys for detention centres (on account of the limited accommodation available in these places some three years ago), managed, nevertheless, to send to them over 20 per cent not well suited, it seems, for this kind of punishment as then defined. The figures in Table X indicate some over-severity of punishment—even perhaps a few innocent boys, whose cases could bear a second looking into; and a succession of boys physically and psychologically handicapped—two even mad, and two more, possibly so.

These results suggest two possible courses of action. First, that the courts should be discreetly informed *post hoc* of the boys whom they have sent to detention who turn out to have been unsuitable for it, on the old-fashioned principle that one of the best ways of learning to modify one's behaviour is the knowledge of having made an error. It would then soon become apparent what kinds of disabilities were being over-looked, and courts could pay special attention to this problem. Secondly, the bench might find that it needed more specialized information in certain cases before coming to a decision. A good educational psychologist, trained and experienced in recognizing handicaps, working with the probation service and having discretion to refer cases for further medical and psychiatric investigation, could do a great deal to reduce the numbers of rather pathetic misfits who, it seems, find their way into a régime designed primarily for the mentally and physically fit.

The question then arises, if such boys are unfit for detention, where are they to go? At present, assuming they need only a short period of penal training, they can go to a young prisoners' centre for three or four months, and serve about the same period of time as they would in detention, in a régime less strenuous and demanding. But if the Queen, by Order in Council (Criminal Justice Act 1961, 3 (5)) abolishes short prison sentences for boys of this age, the only custodial alternative will be Borstal, in which the minimum time to be spent is six months, and where most boys committed thereto stay for an even longer period, on average about 15 months. Once the Order in Council is made, there will, it seems, be no penal institution where a boy, physically or mentally handicapped in the sense we have described, can serve a short term.

Any attempt to keep boys aged 16–20 out of ordinary prisons is to

be commended, but it is only boys serving less than three months who go to local prisons and mix with older prisoners. Boys sentenced to three months or more go to young prisoners' centres, usually separate wings within ordinary prisons, where trade training is provided. It is a pity that the Act of 1961 does not allow an Order in Council to be made abolishing prison sentences of *less* than three months, a measure which would allow sentences longer than this to be retained, specifically perhaps for boys unsuitable for detention, until more suitable training and accommodation could be introduced for them.

As for the small number of boys who may be innocent, or too severely punished: while boys remain inadequately or totally unrepresented in court and unable themselves to speak fluently in public, then only the prosecution's side of the case is heard, and the human element is almost entirely missing from the proceedings. Over 70 per cent of our 302 boys claimed that they were undefended. This, if true,\* means that in at least 70 out of 100 times courts hear only one side of the case; a fact which may well have a cumulatively adverse effect on their attitudes towards the people that they judge.

While courts can make decisions the results of which remain hidden to them, they have, in fact, power without responsibility for the consequences—an unsatisfactory state of affairs to say the least. And nothing can bridge the gulf between the bench and the accused—separated as they are by social class, accent, cultural background and so on—except greater understanding, first of the accused himself and secondly of the effects upon him of the punishments meted out.

Finally, if the handicapped, the innocent and the not really criminal boys could be distinguished at an early stage in the proceedings, then the law and the penal system might progress more rapidly in the study of criminal boys for whom, it is not unreasonable to suppose, custodial punishments were really designed.

### *Acknowledgements*

The research has been carried out largely with the generous help of the Ford Foundation.

I am much indebted to Mr. R. E. Rawles, senior assistant to the research, and to Dr. A. P. E. L. Sealy, Mr. J. C. Freeman and Mr. J. Whetton, who, with Mr. R. Maliphant, carried out the testing and interviewing.

\* It bears out my own observations.

I owe a great deal to Mr. L. T. Wilkins and to Sir George Benson, who were responsible for the original outline of the investigation.

It is impossible to thank adequately the staff in the detention centres, who, despite our intrusion into a very crowded régime, gave us every help and consideration.

The Home Office Research Unit, in particular Mr. S. Klein, have been most helpful in obtaining 'follow-ups' for us with the co-operation of the Criminal Record Office at Scotland Yard.

Our biggest debt, however, is to the boys and to their families.

#### Note

The Home Office is not responsible for any of the views expressed in this paper.

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## Emotionality in the Rat: a Problem of Response Specificity

### INTRODUCTION

The concept of emotionality has a long and honoured history in the psychology of personality. It appears already in the ancient doctrine of the four temperaments, often attributed to Galen and later popularized and extended by Kant and Wundt. This is what Wundt<sup>30</sup> has to say about this typology:

'The ancient differentiation into four temperaments . . . arose from acute psychological observations of individual differences between people. The four-fold division can be justified if we agree to postulate two principles in the individual reactivity of the affects: one of these refers to the *strength*, the other to the *speed of change* of a person's feelings. Choleric and melancholic are inclined to strong affects, while sanguinic and phlegmatic are characterized by weak ones. A high rate of change is found in sanguinics and choleric, a slow rate in melancholic and phlegmatic.

'It is well known that the strong temperaments are predestined towards the *Unluststimmungen*, while the weak ones show a happier ability to enjoy life. . . . The two quickly changeable temperaments . . . are more susceptible to the impressions of the present; their mobility makes them respond to each new idea. The two slower temperaments, on the other hand, are more concerned with the future; failing to respond to each chance impression, they take time to pursue their own ideas.' (pp. 637-8.)

The theory of the four temperaments was based on unaided observation and arose, of course, from no form of statistical analysis. Indeed, when Wundt wrote the words quoted above the notions of correlation and regression were just being worked out in statistical detail by Galton and Pearson, and Spearman was just beginning his fundamental

researches on factor analysis. At about the same time two Dutch psychologists, G. Heymans and E. Wiersma,<sup>19</sup> were carrying out the first extensive numerical studies on the ancient problem of temperamental types. Their method of collecting data would not now be exempt from criticism; they arranged for some 400 doctors each to pick out a family and rate the members of it for a large number of traits by a simple method of underlining or double-underlining. Nor would their numerical treatment be acceptable by modern standards; in their experimental work, too, they lacked some of the rigour which would now be demanded. Nevertheless their studies constituted a pioneering effort, many of the results of which have since been confirmed.

Details of their results, and of subsequent work, have been given elsewhere.<sup>10</sup> Fig. 1 gives a general idea of these results, and their relation to the Galen-Kant-Wundt system. The traits in the outer circle are related to each other in the manner shown, i.e. the closer together they are, the higher is the correlation between them (in technical terms we are concerned with their scalar products). The correlation between any two traits is equal to the cosine of the angle separating them, so that a right angle between two traits would indicate a zero correlation, and angles larger than a right angle a negative correlation. The dimension labelled 'stable-unstable' is also often referred to by the terms 'emotionality' or 'neuroticism'; it refers, in Wundt's terms, to the *strength* of a person's feelings, or the *lability* of his emotions. It is with this dimension that we shall be concerned in the present paper.

While the honour of having originated empirical work and numerical analysis in this field belongs to the Dutch, factor analysis and correlational analysis in general were first applied to ratings of personality by members of the London school. Burt<sup>8</sup> reported in 1915 an investigation of eleven traits (viz. McDougall's primary emotions) for both adults and children; when the correlations between the assessments were subjected to factor analysis they revealed, first, a factor of general emotionality (*e*), analogous in the personality field to Spearman's notion of general intelligence in the cognitive field, and secondly, two bipolar factors—one for asthenic (i.e. introverted) versus sthenic (i.e. extraverted) tendencies and the other for pleasurable versus unpleasurable tendencies. He illustrated these factors by a 'clock diagram'<sup>9</sup> similar to Fig. 1 opposite, and drew attention to the similarity between his factors and the ancient scheme of temperamental types. Webb,<sup>28</sup> in the same year, reported his extensive studies which resulted in a stability



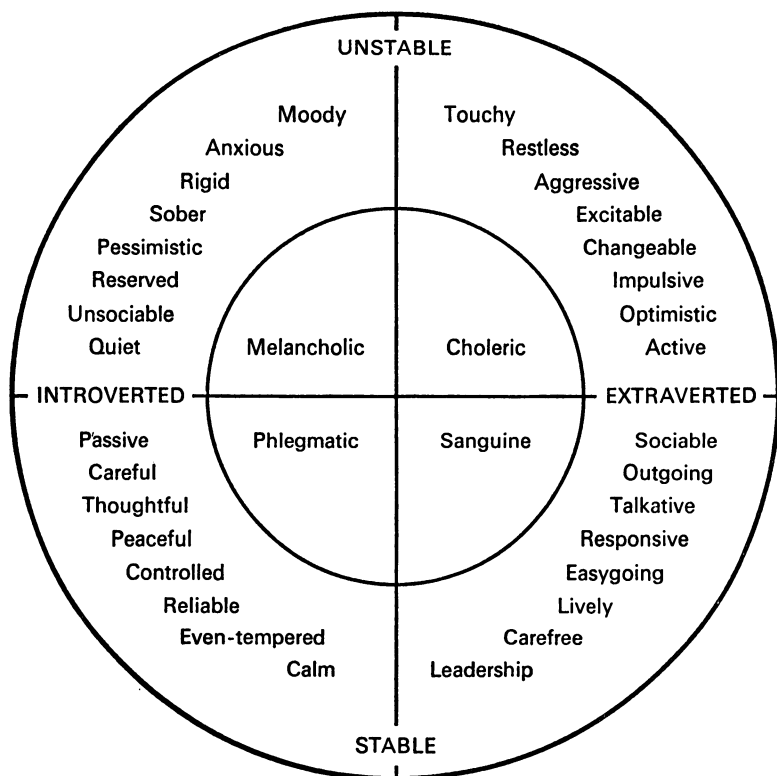


FIG. 1

factor ( $w$ ), which is almost the inverse of emotionality, and also in an extraversion factor, labelled ' $c$ ' by Garnett<sup>15</sup> who reanalysed Webb's data in 1918. There is no doubt, therefore, that the notion of 'general emotionality' as an important dimension of individual differences is closely identified with Sir Cyril Burt and the London school; it is altogether fitting, therefore, that we should have chosen an empirical investigation of certain problems relating to this trait as our contribution to this volume.

### THE NATURE OF EMOTIONALITY

Since the days of James and Lange psychologists have been aware of the fundamental relation between emotion and the functioning of the autonomic nervous system. Although Cannon and later workers may

have presented somewhat more acceptable versions of this relationship, no one will doubt the early observations of the close dependence of emotional experience on autonomic reactivity. Any investigation of emotionality, therefore, must pay attention to empirical work related to the activity of the autonomic nervous system, and it should be recognized that this has raised certain important problems for the psychological theory of emotionality.

Pre-eminent among these problems is that of *response specificity*. It is inviting to think of the autonomic system, or at least of its sympathetic branch, as acting in a unified fashion in conditions of anger and fear, but the evidence adduced by such writers as Lacey and summarized elsewhere by one of us<sup>10</sup> shows that this view is very much over-simplified. When different types of stress, mental and physical, are applied to different individuals, and their autonomic reactions measured by means of a polygraph, it is found that each person tends to be characterized by a highly specific pattern of responsiveness. In one person the result of stress may be an increase in heart beat; in another it may be a rapid increase in the electric conductivity of the skin; in the third it may be a marked tensing of the skeletal musculature, while yet other patterns are observed in a fourth, fifth or sixth subject. Indeed, reactivity may be even more specific than that; for the tensing of the muscles is not general but restricted to one particular muscle group. Thus in one person it may be the frontalis muscle which becomes tense, while in another it may be the arm muscles, or the leg muscles, or the back muscles. So, while each person produces an 'emotional' reaction, this emotion expresses itself physiologically in different ways, making it difficult to compare one person's reaction with another. In one way, of course, this specificity is of considerable help to the clinical psychologist in explaining why different neurotic patients show different types of symptom. Patients who under stress show a tensing of the frontalis muscle tend to complain of headaches, those who show a tensing of the back muscle complain of backache. Patients who repress a great deal of aggressiveness may complain of aches of the muscles of their arms while those with sexual problems often complain of aches in the muscles of the legs. Similarly for the more directly autonomic reactions—the specific reaction to experimental stress preferred by the organism tends to be related to the particular neurotic symptom chosen.

This specificity is also apparent in the factor analytic study of autonomic reactions, where inter-correlations between different measures tend to be very low indeed,<sup>29</sup> and it is also found in the analysis of data

from questionnaires and ratings where typical correlations between individual questions or ratings are below rather than above 0.3. Questionnaires on neuroticism and emotionality tend to have a high reliability and validity because they are built up of a large number of questions; single items tend to be relatively useless.

It is not known whether this response specificity is largely innate or acquired through experience, or whether in fact both hereditary and environmental factors have to be considered. It is obviously possible that each organism inherits an autonomic nervous system, the individual parts of which differ with respect to their lability, so that one particular sub-system would tend to be the weakest and, therefore, the one through which stress is expressed most quickly and in the most extreme form. It is equally plausible to imagine that a specific experience, involving a specific response pattern, may condition the organism to respond in this particular way to any future stress. While it would be our view that both these factors are likely to co-operate in any particular neurotic symptom, there is at present too little evidence to justify any confident conclusion. As long as experimental work is restricted to human beings it will obviously be difficult, if not impossible, to arrive at any confident answer.

### THE EXPERIMENT

It seemed to us that some interesting data relating to this problem could be obtained by experimental work using rats. Admittedly the word 'emotion' has, to many people, an experiential context which would seem to make it inapplicable to animals who cannot communicate, and may not be able to feel, emotions in the same way as human beings. Such a view would disregard the biological foundations of psychology which Burt has always so much stressed in his work, as had Mc Dougall before him. Rats have an autonomic nervous system, and their reactions to stress-producing situations are similar to those of human beings in many ways. Thus, when subjected to fear-producing stimulation, they tend to urinate and defecate. Similarly, man also loses control over elimination in fear-producing situations. Here, for instance, is a quotation from an eye-witness account (quoted by Hall<sup>17</sup>) of a battle in early Persian history in which the behaviour of the enemy commanders during flight is described as follows:

'To save their lives they trampled over the bodies of their soldiers and fled. Like young captured birds they lost courage, with their urine they defiled their chariots and let fall their excrement.'

More acceptable perhaps than this ancient epic is an empirical study by Stouffer *et al.*<sup>26, 27</sup> who studied American soldiers under battle conditions during World War II. From an investigation of the different physiological fear symptoms reported they concluded that '... the symptoms come from a single universe and permit a rank ordering and correspondence along a single continuum. There is an intrinsic interdependence among the different fear symptoms which permit them to be ordered from more to less severe. In this case the underlying continuum is probably physiological...'<sup>27</sup> Loss of control over elimination is found to be the extreme fear symptom on this continuum. Nine per cent of the men urinated in their pants, and twenty-one per cent reported loss of control over bowels when under fire, as compared with, for example, fifty-seven per cent feeling sick and eighty-four per cent experiencing violent pounding of the heart.

This 'elimination' index of emotionality was originally used by Hall<sup>16, 17, 18</sup> for his pioneer study of selective breeding, in which he used it in the form of the open-field test. This test has been restandardized for use in our department<sup>1, 3</sup> for the purpose of selecting strains of 'emotional' or reactive and 'non-emotional' or non-reactive rats, and a brief description of the test in the form it was used in the present experiment will be given here.

It consists essentially of an arena or 'open field' on the floor, in which the rat is exposed in a strictly standardized manner to uniform sound and light fields of controlled intensity for two minutes a day on four successive days. The history of the procedure, the standardization of the test, and the apparatus used are discussed in detail by one of us elsewhere.<sup>4</sup> Two scores are obtained: the defecation score is simply the daily average of the number of fecal boluses or pellets deposited by the rat while in the arena, and the ambulation or 'walking around' score is derived from observing the number of marked radial segments which the rat enters, and is expressed in metres travelled per day. The reliabilities of the scores have been computed as 0.82 and 0.75. It will be recalled that it is the former, the defecation score, which has been the criterion for selection in the establishment of the Maudsley Strains of Reactive and Non-reactive rats; strains designated MR and MNR by Jay.<sup>20</sup>

The subjects for the experiment to be reported were taken from rats selectively bred on the basis of this test. Details of the procedures used in rearing and testing the successive generations for the purpose of the selection experiment are described in detail in the reference given above.

An additional account of some aspects of the husbandry routinely employed in our laboratory will be found in the appropriate entries—that is, 163f for the reactive strain and 163g for the non-reactive—in the *Laboratory Animals Catalogue*.<sup>21</sup> Briefly, these background conditions feature not only standardization of diet, temperature, light/dark cycle, but also a fixed routine relating to the amount of handling each animal is subjected to, the age at which this is done, the size of cage and the number of its occupants at each age. In this way it has been possible to ensure a reasonable constancy of the background environmental variables, not only over individual animals in the same generation, as is crucial to the success of any psychogenetic experiment, but also over animals in different generations, a constancy which is of especial importance in a selection experiment the term of which is measured in years rather than months. The subjects for the present experiment were reared strictly in accordance with these procedures, and in consequence were not only experimentally naïve in the usual sense, but also the product of a uniform environment, both physical and, insofar as present knowledge indicates, psychological.

The 16th generation ( $S_{16}$ ) of the selection experiment comprised 267 rats, 90 of which were from the emotionally reactive strain, and 177 from the non-reactive.<sup>5</sup> Of these, the 60 oldest animals, 30 male and 30 female of each strain, were assigned to each group studied, a total of 120 altogether. They contained representatives of 12 different litters in each case. They were not selected in any way except by age (see later), though a few animals of both sexes from the non-reactive strain were omitted, since they were currently in use for breeding the next generation. It need not be anticipated that this factor introduced any bias into what was intended to be a representative sample of our selected strains, since the breeders in question had been chosen by use of a table of random numbers, instead of from a consideration of individual subjects' scores. This was because selection for high and low defecation as such had been suspended after  $S_{15}$ . Reference to Fig. 2 which illustrates the development of these strains will make this point clear.

Having formed groups of rats selectively bred for defecation, either high or low, our primary interest was in the problem of whether these groups of rats would behave differently when subjected to other tests where previous experimentation had suggested that stress and emotional connections play an important part. If such behaviour could be reliably predicted from knowledge of a given rat's strain, then clearly it would be permissible to argue that our genetic experiment had suc-

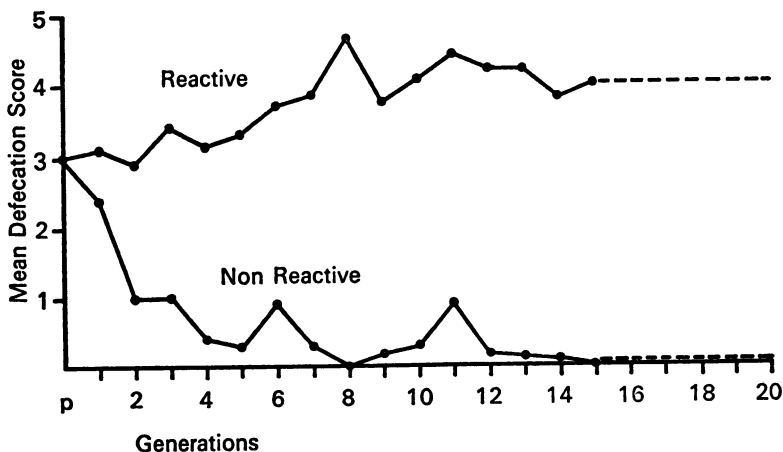


FIG. 2

ceeded in breeding truly emotional and non-emotional animals rather than simply defecating and non-defecating animals. This point is clearly important in relation to our discussion of 'response specificity'; in its extreme form the doctrine of 'response specificity' would seem to suggest that breeding for one type of response should not affect other types of autonomic response. From our previous work (see e.g.<sup>6, 11</sup>) we felt fairly safe in predicting that 'response specificity' would only be partial, leaving a considerable amount of generality to be accounted for. Our prediction, therefore, was that the emotionally reactive and non-reactive rats would have different scores in some at least of the other tests administered to them. Others had not been previously administered to our selected strains but were nevertheless chosen with this possibility in mind. Measurements from several tests in addition to the open-field test already described were obtained. The tests are described below.

#### ESCAPE-AVOIDANCE CONDITIONING

The present version of the shuttle box was developed by Levine.<sup>24</sup> The apparatus allows the fully-automated presentation of stimuli to the rat and the recording of its acquisition of the escape, and, later, the avoidance response. The unconditioned stimulus was an electric shock to the feet of a maximum current density of 0.25 mA. Its onset was preceded, eight seconds before, by a buzzer, the conditioned stimulus (CS). A rat making an avoidance response during that time, terminated the

CS and avoided the shock; failure to respond meant that a shock was delivered to the grid floor of whichever compartment the rat was in, and both shock and buzzer were then maintained until the escape response was made terminating both. Twenty trials in this apparatus were given in a single session at irregular intertrial intervals averaging 50 seconds. Three scores derived from this test were used: first, the number of the first trial on which the subject made an avoidance response; secondly, the total number of avoidance responses; and thirdly, the average latency of these responses, computed in seconds from the time marked on the associated recorder between the onset of the CS and the crossing to the other side which terminated it.

#### RUNWAY

The simple runway test has been employed in the present investigation in the form of a 'conflict' situation of the approach-avoidance sort.<sup>25</sup> Approach tendencies are set up by training the rat to run for a food reward, after which it is given a single electric shock at the goal. The decrease in speed on a subsequent run reveals the extent to which conflict between the approach and avoidance tendencies has thereby been created. The rat, under a food deprivation schedule by which the task was run after 16-20 hours fasting, was trained on eight successive days to run a three-foot runway, painted black and illuminated internally, for a food reward of two pellets, made of rat diet and sucrose in equal proportions and weighing 20 mg. each. By the sixth day, ten trials per day were being given with an intertrial interval for each subject of approximately 30 seconds. On the ninth day, by which time all animals had had 50 approach trials of the sort described, the animal was given a shock limited to a maximum of 0.67 mA. current density through three plates in the floor of the goal box which contained electrodes, after it had eaten the pellets. This constituted the avoidance training, and five further approach trials (i.e. with food reward alone, as before) were immediately given to test the strength of the conflict. Timing was accomplished automatically throughout: if the animal refused to leave the start box on any of the five test trials for a period of five minutes, testing was discontinued, and a latency of 300 seconds scored for that and any subsequent trials remaining. Two scores derived from this test were used; first, the mean runway speed, in seconds, of the last 10 of the 50 approach trials given (which approximates directly to speed in view of the standard distance run), and secondly, the conflict decre-

ment in seconds, measured as the decrease in speed shown by the mean for the five test trials described.

#### SWIMMING SPEED

Rats readily learn to swim a channel underwater, and it has been shown that the speed of swimming is a linear function of the amount of time they are detained underwater before being released.<sup>2</sup> A 10-foot long tank,  $7\frac{1}{2}$  inches wide, is filled to a depth of 9 inches with water maintained at 20° C. A stainless steel wire mesh 'ceiling' is fixed below the water level, thus forcing the rat to swim eight feet under water from the release cage to the ramp, where it can regain the surface and resume normal breathing. Five successive days were spent in training the subjects: two further days' trials, a total of 20 in all, were given and constitute the test trials for determining the speed measure which, like running speed, is expressed in seconds, in view of the constant length swim, and given as the mean speed per trial. The intertrial interval was maintained relatively constant for all animals on a given day.

#### STARTLE

Measurement of the startle response in rodents has been used from time to time as an indicator of emotional responsiveness. The method we employed is adapted from that used by Brown.<sup>7</sup> A dietary spring balance of 500 g. capacity with a sensitivity of 2 g. was fitted with a plastic box covered with muslin. An additional pointer moved clockwise only, thus indicating on the scale the maximum excursion occasioned by the jump the rat in the box makes when subjected to a sudden intense sound, measured within the box as 121 db. ref 0.0003 dynes/sq. cm. This was made by firing a blank cartridge 50 ins. away from the rat at the same height as the box. The rat was conveyed from the colony room to the experimental room set aside for this test and its weight, measured by this balance, was recorded before the stimulus was applied. The startle measure was obtained by noting the maximum displacement recorded, and correcting by the body weight in 100 g.

#### WATER INTAKE

This is a simple test which is based on the rationale that fear will interfere with consummatory action. The version we employed follows Levine<sup>22, 23</sup> and the arrangements made for conducting the test involved removing the subjects from their home cages, weighing and placing them in individual cages provided with food and water *ad libitum*. At



5 p.m. on the evening of the same day the water bottles were removed and not replaced until the following morning at 11 a.m., 18 hours later. The amount drunk by the rat in the next ten minutes was measured, and the bottles were allowed to remain *in situ* until 5 p.m. The procedure was repeated on the following day, after which the rats were re-caged in the usual groups of three per cage. The measure of water intake obtained is the average water consumption in ml. on the two successive days, corrected for body weight in 100 g. from the measure taken before the water deprivation was imposed, since this procedure results in weight loss as the thirsty rat restricts its food intake.

#### WEIGHT LOSS

This is a combined physiological and behavioural measure which has its rationale in the fact that animals subjected to disturbance of various kinds, all of which might be considered as mildly fear-provoking, will tend to eat less than they normally would in their home cages and, in consequence, to lose weight.

It was decided to capitalize on the fact that our subjects had to be sent by road from the Animal Psychology Laboratory at the Bethlem Royal Hospital, where the major part of the investigation was carried out, to the Maudsley Hospital, some eight miles away, for sacrifice for the determination of gland weights, etc. (see below), to record the weight loss noted between the last measurement, usually on the day they left one laboratory, and the weight at death in the other. The weight loss measure obtained, as described above, was expressed as a percentage of the original value taken before transportation.

#### WEIGHT

Each animal was weighed before leaving the laboratory as described above in connection with the loss measure. The weight is expressed in grams.

#### PITUITARY, ADRENAL, THYROID AND GONADS

After sacrificing the subjects with an overdose of ether anaesthesia, they were quickly dissected and the endocrine organs noted above removed. The measures are in each case corrected for body weight in 100 g., the terminal weight at death being used for this purpose.<sup>12, 13, 14</sup>

The age of the groups was  $88.6 \pm (\text{SD}) 1.4$  days for the reactive strain, and  $88.9 \pm 3.6$  for the non-reactive strain on the day they were

re-caged for the purpose of the water-intake test, which was given first in all cases. All the other tests, save for the physiological measures which were necessarily terminal, were administered in a partially balanced sequence lasting 27 days to minimize the possibility of order effects influencing the outcome.

## RESULTS

Table I gives the means and standard deviations of the two groups for the measures, and the significance of the differences between them. Not all the distributions were normal, unfortunately, and consequently it was necessary to resort to the transformations indicated. Significance was tested by means of two-way *t*-tests. The table is divided into two halves giving data for male and female rats separately; this was done because previous work had shown that sex often plays a very important part in types of emotional reactivity and it was suspected that there might also be sex differences in 'response specificity' and inheritability of emotional responses.

It will be seen that our general hypothesis of response generality is borne out very powerfully by the data. Taking the males first, we find that the reactive and non-reactive strains differ not only with respect to defecation, as expected, but also in regard to ambulation, number of avoidances, conflict, swimming speed, body weight and weight loss, and thyroid and gonad weight. The females show significant differences between reactive and non-reactive strains on two avoidance measures, startle, weight and weight loss, and all four hormonal indices. It is clear, therefore, that genetically there is a considerable degree of generality to the notion of 'emotionality', in that breeding for one type of response produces marked differences in other responses as well.

It will also be clear that the genetic pattern is by no means identical for males and females. In several cases there is a significant difference between the two strains for the males but not for the females, and *vice versa*. The following tests give significant differences for the males only: ambulation, conflict, swimming speed; the following give significant differences for the females only: first avoidance, startle, pituitary and adrenal weight. It is not possible in the present state of our knowledge to do more than draw attention to these differences, and indeed comment would probably be unwise until replication of the experiment had shown them to be reliable.

TABLE I

Means and standard deviation for the groups in the various measures and the significance of the differences between them

Measure	Transformation			Males			Females		
	Reactive	Non-reactive	Signif.	Reactive	Non-reactive	Signif.	Reactive	Non-reactive	Signif.
	M	SD		M	SD		M	SD	
Defecation	0.27	0.06		0.07	0.11	***	0.22	0.09	***
Ambulation	8.72	1.92		10.28	1.72	**	10.84	2.18	n.s.
First avoidance	6.70	5.48		7.53	5.12	n.s.	9.67	4.44	**
Avoidances	4.60	2.90		8.77	4.22	***	6.10	3.76	**
Latency	1.11	0.26		1.11	0.34	n.s.	1.14	0.23	n.s.
Running speed	0.72	0.23		0.72	0.43	n.s.	0.64	0.25	n.s.
Conflict	2.82	0.99		2.78	0.01	**	2.81	0.09	n.s.
Swimming speed	6.18	0.66		6.72	0.74	**	7.28	0.67	n.s.
Startle	41.7	24.02		57.03	57.58	n.s.	45.23	27.55	***
Water intake	3.34	1.02		2.95	1.16	n.s.	3.57	1.54	n.s.
Weight loss	1.61	0.06		1.65	0.04	**	1.60	0.09	**
Weight	240.63	19.93		209.33	19.08	***	160.67	13.06	***
Pituitary	3.20	0.50		3.27	0.38	n.s.	5.24	0.97	***
Adrenal	18.46	2.61		18.29	1.78	n.s.	32.67	4.10	***
Thyroid	7.26	1.13		6.20	0.86	***	10.31	1.48	***
Gonads	958.06	61.43		1,162.93	128.24	***	32.77	9.65	***

Note: \* = significant at the 5 per cent level.

\*\* = significant at the 1 per cent level.

\*\*\* = significant at the 0.1 per cent level.

n.s. = not significant.

## DISCUSSION AND CONCLUSIONS

We have in this paper discussed certain problems relating to the concept of emotionality, regarded as one of the fundamental dimensions of personality. Assuming that individual differences in emotionality are closely related to physiological differences in the autonomic system, and that these are largely of genetic origin, we have argued that many of the problems which arise can best be answered by animal experimentation, in spite of the obvious restrictions which the choice of a non-human organism imposes on the experimenter.

Our main concern has been with the problem of specificity of autonomic reaction, and the possibility of demonstrating at least some degree of general emotional responsiveness. The particular method adopted by us has been to breed selected strains of rats characterized by a high and low defecation in fear-producing situations; it was argued that under conditions of extreme 'response specificity', selection for the elimination response ( $R_1$ ) would not lead to any differentiation between the strains with respect to other emotional responses such as ambulation ( $R_2$ ), startle ( $R_3$ ), avoidance ( $R_4$ ), etc., or to differences in physiological characteristics such as pituitary, adrenal, thyroid or gonad weight. On the hypothesis of a general factor of emotionality, however, it was predicted that selection for  $R_1$  should lead to significant differences in  $R_2, R_3, R_4 \dots R_N$  as well. In other words the hypothesis we were testing has some similarities to Spearman's original conception of general and specific factors; we were postulating, indeed, the existence of a general factor of emotionality akin to that of Burt, in all the situations used, as well as specific factors confined to each situation investigated. Such a hypothesis can be investigated either by means of factor analysis (and some as yet unreported work has indeed given rise to inter-correlations and factors which could be interpreted in line with our hypothesis) or by means of a genetic experiment of the type outlined here, in which generality is shown by a spread of effect from the particular test on which selection is practised to other tests essentially independent and dissimilar to the selected instrument. Our data seem to show that 'response specificity' is indeed a limiting concept rather than a universally valid one; the tests used involve specific factors but they also involve a general factor which can best be understood as one of general emotionality. This work, therefore, essentially bears out the early factor analytic studies of Burt and others on humans, and suggests the importance of emotionality as a fundamental concept in the description of personality.

## SUMMARY

A sample of 120 rats from the Maudsley Reactive and Non-reactive Strains which have been bidirectionally selected for emotional elimination in the open-field test was given a battery of other behavioural and physiological tests. The results presented indicate a considerable generality of emotional responsiveness, in that the strains differ in several of the other tests. In addition marked variations between the two sexes in the patterning of these strain differences was observed.

## Acknowledgements

We are deeply indebted to Dr. Umaymah S. Yaktin for her considerable assistance in the design and execution of the experiment reported here. Dr. George Feuer also contributed greatly by making the physiological measurements of endocrine weights reported, and Mr. Alan Hendrickson, Miss N. Hemsley and Mrs. V. Beavers with the statistical analysis. Mr. K. B. Wraight and Miss Sylvia B. Parsons also assisted with the testing. Dr. Seymour Levine kindly advised on the selection of tests and loaned the escape-avoidance conditioning apparatus used. The investigation was supported in part by the Ecology Fund and by a U.S. Public Health Service Research Grant No. M-6014 from the National Institute of Mental Health.

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*Raymond B. Cattell*

## **Higher Order Factor Structures and Reticular-vs-Hierarchical Formulae for their Interpretation**

### **THE NATURE OF FACTORS**

With the clear and comprehensive formulation by Sir Cyril Burt<sup>5, 6</sup> of a hierarchical concept of ability structure in man, the stage was set for the exploration of factor structures, and as a result many psychological concepts have been extended and amplified: for example, the notion that there are not one, but two factors of general ability ('fluid' and 'crystallized' intelligence<sup>13</sup>), and the emergence of important theories concerning higher order structure in factors of personality. In this paper, illustrations will be mainly from the field of personality, but the chief purpose is to develop concepts and theoretical models for higher order factor structure in general. The treatment culminates in new formulae, notably the Cattell-White alternative to the well-known Schmid-Leiman transformation.

What is the relevance to purely psychological interests of this clarification of theoretical and mathematical models? Although the average student of personality and ability, particularly in the educational and clinical arts, is prone to theorize and practise without explicitly stating the formal model he uses, his theories cannot be taken very seriously until he specifies their properties. When criticized for not doing so, he is unfortunately apt to defend himself by saying that certain issues in factor analysis are 'esoteric'; yet the fact remains that in this field the fateful choice between different psychological theories turns on highly technical and statistical considerations. Social and educational psychologists, while recognizing that vast practical and political decisions on, say, nuclear fall-out depend on complex technical calculations in nuclear physics, nevertheless act as if their own psychological advice, affecting large numbers of children, patients and citizens, can be given

without any effort to understand the true complexities of formulae such as we have to consider here. This is not to say that the average psychologist has to be an expert factor analyst, but it means that, to be considered qualified for his tasks, especially those dealing with the assessment of personality and ability, he *should* have clear concepts of the *logical* issues involved.

A deeply grateful student\* of Professor Burt, the present writer finds himself in disagreement with him on a few issues, one of which concerns the relative advantages of orthogonal-vs-oblique factors. Sir Cyril holds that the purpose of factor analysis is classification and that, in the cognitive field particularly, the hierarchical arrangement of factors obtained by 'principal axes' and by 'simple summation' often gives not only a more economical but a truer picture of mental abilities than do oblique factor methods.

Elsewhere<sup>8</sup> I have put forward arguments (perhaps, one might say, as 'spokesman of the loyal opposition') for believing that simple structure is inherent in natural data (Cattell and Dickman<sup>18</sup>) and that, when it is discovered, and exactly adhered to, it normally yields oblique factors. These *are* free to 'go orthogonal' as a special case, but the chances of exactly zero correlations between them, even in populations, are infinitely small—if factors are meant to model nature. By the operational definition of simple structure (Cattell<sup>8</sup>), a definition somewhat different from Thurstone's,<sup>39</sup> a factor is given the property of something other than a mere classificatory principle. As part of a scientific model, with more properties than a purely mathematical model, it is given the status of an *influence or cause*, accounting for the covariation observed in the manifest variables affected by it. (Notably, of course, in the 'salient' or 'marker' variables, loaded most highly and significantly.) Such an influence is likely to leave untouched the majority of variables in any well-designed experiment, and to reveal, by leaving a galaxy of points forming a hyperplane of zero-loaded variables in hyperspace, the proper position to which it needs to be rotated.

However, the location of oblique factors (and therefore the proof that they are oblique) does not rest merely on the criterion of simple structure. For the independent resolution of results of a correlational research by the main alternative principle—confactor rotation (Cattell<sup>14</sup>)—can also lead to the same result. Indeed, the notion that oblique factors will be the common outcome in scientific investigation does not

\* It was a series of lectures by Professor Burt, in 1925, which turned the writer from post-graduate work in the physical sciences to a career in psychology.

rest only on experimental evidence in this narrow area itself. It rests on the general scientific proposition that in an interacting, unsegregated universe most influences will tend to show some correlation. The weight and volume of the planets, or the temperature and pressure taken at a hundred meteorological stations, will normally show significant correlations. It is the task of factor analysis to reveal and define these distinct, but correlated, concepts. If we insist on entities which are statistically uncorrelated they may well be conceptually contaminated.

### THE NATURE OF HIGHER ORDER FACTORS

If factors can be correlated, then obviously one can find factors among factors. Those derived from the primary matrix of correlations between factors we call second order or secondary factors. There is no mathematical or logical reason why this process should not be repeated, leading to tertiary and quaternary factors, etc. Indeed, it has already been shown that one *can* get simple structure at these higher orders (Cattell,<sup>10</sup> Humphreys<sup>31</sup>), and that such higher order factors, like primaries, are consistent in pattern from one experiment to another, and correlated. It is an historical curiosity that pursuit of these higher orders in the personality field has developed almost simultaneously with similar work in the much older realm of research into abilities.

There are now no fewer than fourteen researches, recently surveyed and critically compared by Gorsuch,<sup>25</sup> on higher order factors among the primary factors fixed by the Sixteen Personality Factor Questionnaire. They agree extremely well in defining five second order factors, two of which, anxiety and *exvia-invia* (the precise extraversion-introversion factor defined by Warburton, 1962) are very easily recognized in terms of the classical Freudian and Jungian concepts. Moreover, they have been confirmed by clinical evidence and by their good alignment with first order factors obtained from objective tests (O-A Battery Cattell<sup>15</sup>) and factors U.I. 24 and U.I. 32 (Universal Index Numbers, Cattell<sup>10</sup>).

In this quick glance at the substantive illustration of these structural concepts we may note that Knapp, Cattell and Scheier<sup>33</sup> have explored second-order structure in the 21 objective test factors extending from U.I. 16 to U.I. 36 and have reached agreement on seven second order factors. Some of these make good sense in terms of psychoanalytic concepts, while others present new constructs around which post-psychoanalytic theories of personality can be developed. Recently, Pawlik and Cattell<sup>35</sup> have carried the O-A Battery studies to the third

order analysis and have found, at what may be a final level, unambiguous structure in *three* major factors, which bear a distinct resemblance to the Freudian trio of id, ego and super ego.

Unfortunately, psychologists working in learning theory, clinical psychology, perception, etc., who are unfamiliar with factor analysis, have failed to avail themselves of the theoretical and experimental possibilities which the measurement of these definite factors would bring to their work. It would appear that they are confused by the technical controversies among factor analysts; first, over the relations between three common personality questionnaires—the 16 P.F., the MMPI and the Guilford-Zimmerman; secondly, in the sphere of behaviour ratings and objective tests, by the methodological disputes on the nature of hierarchies and the definition of higher order factors. Such disputes continue, despite published researches containing well substantiated primary and higher order factors, for example the work of Burt,<sup>6</sup> Cattell,<sup>8</sup> Digman,<sup>21</sup> Eysenck,<sup>22</sup> Harman,<sup>28</sup> Humphreys,<sup>31</sup> Peterson,<sup>36</sup> Vernon<sup>41</sup> and others.

The first question—that of alignment of factors from different questionnaires—is not particularly relevant here. The Guilford-Zimmerman and the 16 P.F. can *never* be aligned, for the one has been aimed at orthogonal, the other at oblique factors. The MMPI, on the other hand, deals with *surface* traits rather than with factors as here defined and these are resolvable into five *source* traits (rotated factors—Cattell<sup>10</sup>) which lie in a sub-space among the 16 source traits (factors) subtended by the 16 P.F.

The second area of debate—on higher order structure—involves more complex and extensive issues, which it is the object of this paper to clarify.

### THREE POSSIBLE CONCEPTS OF FACTOR HIERARCHY

Formulation of the concepts of second order factors had a poor start because of the almost accidental circumstance that in the pioneer studies, including Thurstone's on primary abilities, simple structure was applied at the level of the primaries but not at the level of higher order factors. Yet surely simple structure (or confactor) unique rotation should be used at *all* levels if the term 'factor' is consistently to retain its meaning. A primary factor is here defined as an influence among variables, and we propose to define the next order of factors as influences upon factors. It follows that they must have simple structure on the primaries.

Failure to appreciate this may lead to an additional confusion—the assertion that a very ‘broad’ primary factor is the same thing as a second order factor. The latter view is found in Adcock’s,<sup>1</sup> Eysenck’s<sup>22</sup> and Peterson’s<sup>36</sup> writings. These writers either state or imply that, for example, by taking out only one or two massive and general factors at the first order level—such as is often achieved by not rotating the first big centroid or principal axis—they are reaching the same result as they would obtain by taking out many primaries and then finding broad second order factors which cover the former. It is true that there is a very broad resemblance in the patterns of such alternative factor loadings, but these can scarcely serve as a basis for refined scientific concepts. Indeed, the alternative methods lead to vital differences in the concepts of neurosis, anxiety and ego strength between the personality theories of Cattell and Scheier<sup>20</sup> on the one hand, and Becker,<sup>2</sup> Eysenck<sup>22</sup> and Peterson<sup>36</sup> on the other.

The rest of this chapter deals with the problems which reside in the analysis itself and attempts to answer the question—what structures *could* exist and what are the methodological and statistical conditions of their existence within the factor analytic model?

If we accept the basic model in which correlations (including communality estimates) are resolved into common, specific and error factors, there exists already a considerable ‘taxonomy’ of stereotyped possibilities, beginning with Burt’s designation of the ‘bipolar factor’ type of solution, Spearman’s positive general factor and Holzinger’s ‘bifactor’ solution, and extending to many others, for example, Guttman’s ‘simplex’, and Cattell’s ‘co-operative factor structure’. These are essentially sets of standard ‘mosaics’ of factor patterns and, though never ideally attained in practice, provide useful terms by which specialists in the field can refer to a particular factor resolution.

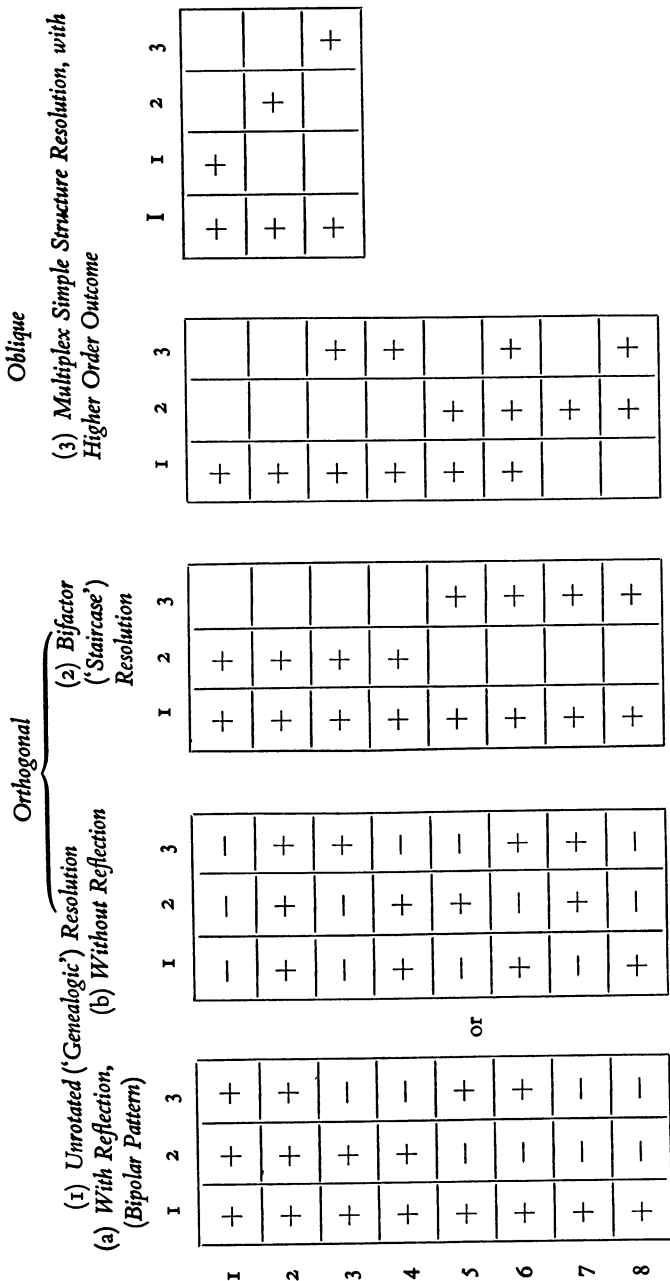
Broadly conceived, these abstract mosaics differ in pattern in four main respects:

1. The number of common factors operating;
2. The number of variables they influence;
3. Their mutual overlap in influence on the variables;
4. Their algebraic sign patterns.

Naturally, the number of possible combinations constituting such Stereotyped Influence Patterns (or SIPs) is very great. The three most discussed and, indeed, most important at the primary factor level are set out in Fig. 1; others, sampled also from the still

FIG. 1

Relation between Dimension and Variable  
Three Basic Resolution Patterns in the Structure of Personality and Abilities



or

larger number which appear when we combine the many possibilities at higher order levels, are shown in Fig. 2.

The first of these stereotyped patterns is nothing more than the primitive unrotated centroid or principle axis itself. Burt has made much use of it for logical, classificatory purposes, for, as he points out, the orderly arrangement of bipolar factors has the logical classificatory scheme of a 'tree of Porphyry'. (The present writer has suggested the term 'genealogic' because the successive divisions are like ancestors in a family tree.) Few, however, would consider this likely to correspond to *functional* psychological influences, and, of course, if variables are reflected back to original meanings, as is usual after a centroid analysis (and as illustrated in (1b) by reflecting every other variable), the main logical relations themselves become obscured. The second, or Holzinger bifactor resolution, is obtained by a special rotation from (1a), and, as Burt points out,<sup>5</sup> it preserves essentially the same relations, but eliminates the bipolarity by dropping negative loadings, though requiring more factors to represent the same complexity.

The third pattern, or SIP, has been called a *multiplex* by the present writer, because it gives equal importance to all factors and a random but multiple determination of variables by all factors. It is a multiple factor, simple structure influence pattern, i.e. with zeros in every column, and its essence is that it does *not* precisely stipulate degrees of overlap and signs, but accepts random overlap and a random pattern of signs. The structure of the multiplex also accepts any angle among the factors (oblique or orthogonal) and can thus support a second or higher order factor structure. The general multiplex is probably the most widely used, and useful resolution in psychological research.

It will readily be seen that resolutions (1) and (2) lend themselves to a 'hierarchical' view of personality or ability structure—in that one positive general factor dominates. When some psychologists speak of 'a hierarchy' they refer only to such a structure in this first order realm and to the special orthogonal case. Others, however, use the term to refer to the oblique case and the arrangement of the additional higher order factors in a sort of pyramid, which may appear in such circumstances. It would clarify the position if writers would refer to the first as a 'dominant general factor' solution and reserve hierarchy for super-imposed higher orders.

Even in the latter, however, there are *two* senses of hierarchical, as shown in Fig. 2. In system (1), which follows the typical solution from a true use of simple structure on successive orders, the hierarchy





means nothing more than a series of factor orders, plus the pyramidal structure formed by the diminishing number of factors that are recorded as higher orders are reached. This pyramid may be incomplete however, in the sense of not finishing in a single factor to make a true pyramid, but in two or three, as in Fig. 2 (1). This most common result of the common multiplex base we may call a *tapering oblique hierarchy*. As we shall argue later, its tapering is accidental and artificial.

The psychologist who wants to produce the second type of SIP in Fig. 2 will do his best to rotate the primaries to be non-overlapping. He is thus obtaining a 'dominant general factor' resolution at the higher order. To get this special arrangement he may choose, perhaps unconsciously, variables which happen to give a single, general, second order factor plus specifics (extreme right of Fig. 2 (2)). The result in this case will be a very neat resolution of each and every variable into a loading on one of a number of orthogonal primaries (not the original primaries,  $1'$ ,  $2'$ , etc., but the specific factor remnant,  $1'_1$ ,  $2'_2$ , etc., corresponding to each) and on a general factor.

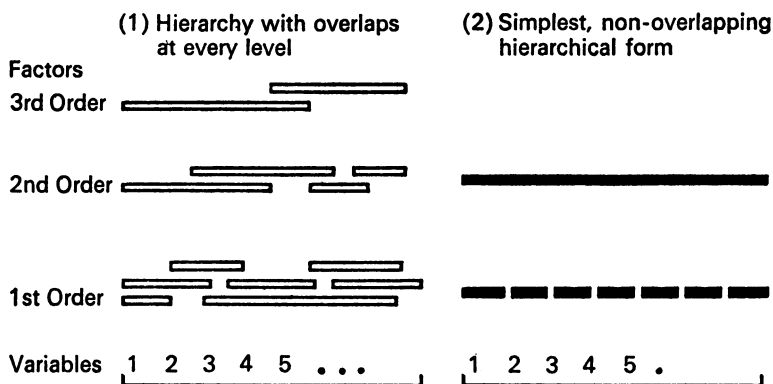


FIG. 3

*Two plans of hierarchy among factor orders*

A second manner of showing these same structures, in Fig. 3, shows that in what we may call the 'pure pyramid or monarchical hierarchy', the primaries will neither overlap nor leave gaps between them, and thus will yield a more orderly hierarchy than any other arrangement. This has the same utility, as a logical classification scheme, as Burt's bipolar system, except that signs are not specified. Not all centroids can be rotated into this form, but many which would other-

wise be naturally represented by a tapering oblique hierarchy can be transformed by what is known as the Schmid-Leiman formula into a monarchical hierarchy.

Unfortunately, as I have indicated in the introduction to this paper, to get such a hierarchy one has to be selective or lucky in one's data—notably in being able to end with a grand monarchic general factor. A more important criticism of this SIP form is that although the pattern undoubtedly appeals to neat and tidy minds, the factors may not correspond to those realities in nature which will be constant from matrix to matrix. For the second order general factor here, like that in the first order centroid (the bipolar or bifactor general factor), is specific to the matrix; it is dependent on the particular choice of variables and is not to be fixed by simple structure because there is no hyperplane to rotate it by. The 'specifics' from the primaries are therefore equally arbitrarily truncated entities.

As we shall see, the position and nature of the lower order factors, when transformed into orthogonality by the Schmid-Leiman formula, are not arbitrary, neither are they non-overlapping, though those of the factors at the top of the pyramid are. But, with this exception, neither the 'hierarchy' at the first order (which we have defined as a 'dominant general factor solution') nor the true hierarchy across orders, in the monarchical sense, are normally anything but artificial creations, whose factors lack the constancy from matrix to matrix which we require of scientific concepts. Whether the one remaining form—the tapering oblique hierarchy in Figs. 2 (1) and 3 (1)—deserves the designation of a hierarchy we shall now discuss.

### **HIERARCHY OR NETWORK?**

No matter how starkly we define a hierarchy operationally in the initial stages, it will tend to carry connotations of a broader and even of a philosophical nature to most who use the term. Among these connotations are the implications that i. the factor *higher* in the hierarchy must *always* be broader in its influence; ii. it is more *important* for prediction; iii. it is more *constant* in its form and appearance; iv. it is more *fundamental* for psychological theory, and v. it is more 'real'. We propose to show that these implications vary from insufficiently defined or inaccurate conclusions to completely unwarranted illusions.

Before discussing the possible meaning of a hierarchy of factor orders, notably of the third—tapering oblique—and only surviving sense of a hierarchy, one must ask whether it exists in nature at all. For

what is commonly overlooked is that the pyramid structure, tapering to fewer factors at higher orders, could be an inevitable artefact of normal mathematical-statistical rules and need have no real existence at all in nature. One mathematical rule, when communalities are used, is that one cannot take out as many common factors as there are variables. Consequently, a hundred variables may define, say, only twenty primaries, and twenty primaries must yield fewer second order factors, and so on. But the fact that a number of higher order factors as great as the number of variables or lower order factors cannot be mathematically defined for lack of a sufficiency of variables is no proof that they do not exist. Indeed, the onus of proof that there is only this smaller number of higher factors at work lies on the psychologist who chooses to assert that the real structure of nature is a pyramidal hierarchy. Actually, I have given elsewhere (Cattell<sup>16</sup>) ample reasons for believing that the number of factors operating on a set of variables is normally decidedly greater than the number commonly taken out from  $n$  variables (provided we count real influences of small variance). These apply as much to the transition from primaries to secondaries as to the transition from variables to primaries. Recognition of this follows from acceptance of the fact that the number of real influences in a situation is one thing, and the number of factors we may take out, in accordance with mathematical and statistical restrictions, is quite another.

As a result of these considerations I shall propound a view of the influence pattern of the entities sought in factorization quite different from that hitherto accepted. Briefly, this theory is that the influences interact in what may be described in the most general terms as a *network* or *reticule*, and that the so-called hierarchy is an arbitrary piece chopped out of the network, which converges on fewer points at higher orders merely because of the mathematical rules which govern the 'cutting' of the network. This is illustrated in Fig. 4, where the only assumptions made are: 1. that causal effects operate in one direction only—that required by our definition of simple structure—from factors to variables and from higher order to lower order factors; 2. that there is no difference in frequency between factors and variables; and 3. that each variable is accounted for by more than one factor and each factor influences more than one variable.

In such a structure as that given in Fig. 4—which has the essential qualities implied by the terms *network* or *reticule*—the application of the usual procedures of factor analysis to four variables,  $\nu_2$ ,  $\nu_3$ ,  $\nu_4$ , and  $\nu_5$ , would result in the discovery of two primary com-

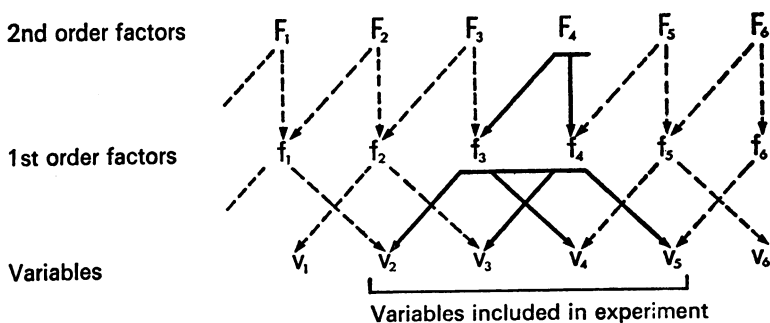


FIG. 4

*False hierarchy in an essentially reticular structure*

mon factors,  $f_3$  and  $f_4$ , and the factoring of these would in turn result in the discovery of only one second order factor,  $F_4$ . These connections are shown by firm lines, whereas the undiscovered connections are left as interrupted lines. The firm lines clearly yield a hierarchy, yet the total structure is obviously not a hierarchy, there being as many higher order as lower order influences. Although I do not deny that tapered oblique hierarchies (or even monarchical ones) may at times be present, it would appear that most of the claims to demonstrate a hierarchy rest on nothing but the illusory effect just described. For example, the inclusion of variables  $v_1$  and  $v_6$  in an experiment would at once show that the four variables really operate under the influence of four first order factors,  $f_2, f_3, f_4$  and  $f_5$ . This knowledge would result in the addition of  $f_2$  and  $f_5$  at the second order factoring, and this in turn would demonstrate that three second order factors,  $F_3, F_4$  and  $F_5$ , are really operative on  $f_3$  and  $f_4$ .

If, on broadening the base of the variables (or first order factors) one does not find more higher order factors, then one can conclude that a true hierarchy is present. The hierarchy of which we then speak would be different from the three types of hierarchy discussed above—dominant general factor, monarchical and tapering oblique—which have no proof of reality. It would be definable simply as: 1. a series of orders of factors, oblique at each level, and normally mutually overlapping in influence within each level; and, 2. one within which each order has fewer factors than the next lower order. The result would then be that higher order factors in such an *oblique pyramid hierarchy* will affect more of the initial sample of variables than lower order factors. But this will happen even in an ordinary network, for *regardless of which direction one*

*moves in a network* (and of the separate existence of any real, excised pyramid) this broadening of influence necessarily occurs.

It may be that the structure of the mind in certain areas, notably the cognitive area, *does* correspond to a pyramid hierarchy, in the way that Burt,<sup>4</sup> Humphreys,<sup>31</sup> Vernon<sup>41</sup> and others have claimed. Even if wider search fails to broaden the basis of primary factor (and thus shift the top of the pyramid above the monarchical factor supposed to sit there), the status of the monarchical factor is still theoretically unsound. For if it affects all of the penultimate stratum of factors it is unrotatable—there is no hyperplane of unaffected factors by which its meaning can be uniquely determined. Of course, by some *other* method of factor analysis, in which additional penultimate stratum factors are deliberately introduced (Cattell<sup>13</sup>), a unique resolution of the last factor could be obtained. But this has not been done by those writers who propound a monarchical theory of general ability.

On the other hand, notably in the more complex fields of personality, motivation and learning, I would argue that a demonstrated hierarchy in any of these senses simply does not exist. And even in the cognitive area, recent evidence (Cattell<sup>13</sup>) that there is not *one* general ability factor but *two*—fluid and crystallized intelligence—upsets the monarchical hierarchy theory. The higher order structure which probably exists in the personality realm, and in many scientific realms, when examined by factor analysis, is decidedly more complex. I have paid this attention to pseudo-hierarchies because their true complexity will get due attention only if psychologists recognize that the traditional hierarchical notion is a fiction, created by the artificial limitations of calculation imposed by the rules in the factor analytic textbook.

#### **OPERATIONAL IMPLICATIONS OF DEFINING A FACTOR AS A DETERMINER**

The statement that the model assumed in testing hypotheses by properly-designed factor analytic experiments is more than a mathematical model, will now be clearer. Our model involves influences and causality, and actually defines independent and dependent variables, of which mathematics has no knowledge. Furthermore, interaction of influences may take place in all possible directions and connections. The only general model we can initially accept is, therefore, a reticular one, in which different orders of factors may interact in all kinds of ways, the hierarchy being a special case of the reticule, requiring special proof. The problem now before us is: 'By what means, in factor

analysis, or with other experimental methods in addition to factor analysis, can we infer the particular system of causal connections—the structure—existing in any such reticule?’ The postulated connections are quite general, put in this abstract way, but Fig. 5 will help to summarize the situation. In this diagram the assumption is made that every factor or variable acts on every other in all mathematically possible ways, including positive and negative feedback. (The positive and negative ‘loadings’ on each ‘influence’ arrow are not shown.)

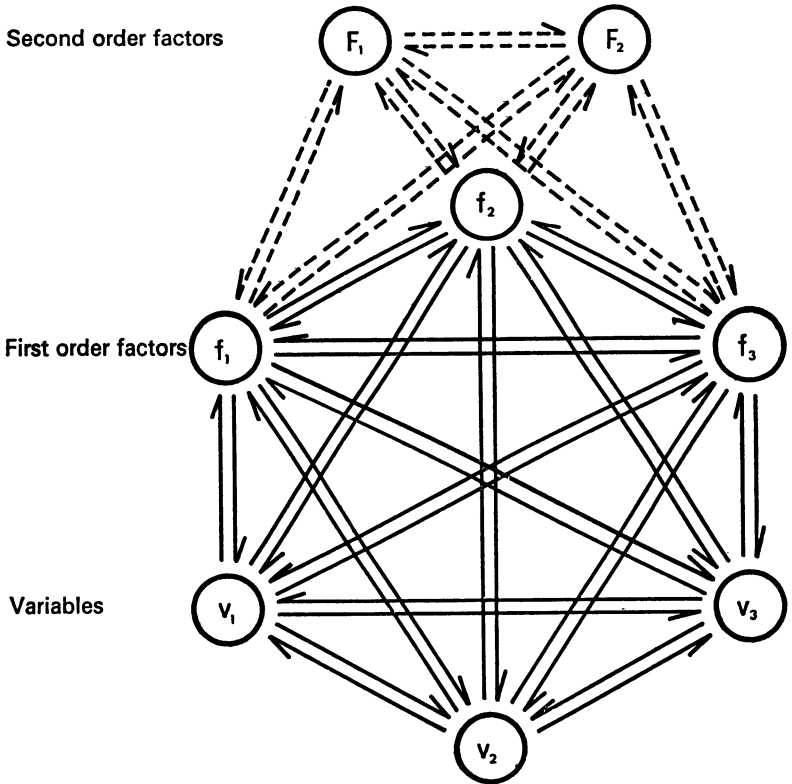


FIG. 5

*Possible interactions of a set of eight distinct influences*

To avoid overcrowding the sketch (a) interactions of second order factors directly with variables are omitted, and (b) only three variables are set out of the larger number which would be necessary to define three factors.

No systematic treatment of the general problem of defining the inferences about causal entities and directions of influence from factor

analytic operations appears yet to have been attempted. Experimental studies, however, are replete with inferences on an inexplicit basis, particularly on that facet (one of three) which concerns the interaction of factors and tests. (The remaining facets concern interactions of factors with factors and variables with variables.) The schema which have actually been presented, namely SIPs such as the bipolar pattern, the simplex, the multiplex, etc., have usually connoted nothing more than a particular mosaic of correlations or loadings, with an occasional causal assumption that the effect is due to some real influences in a scientific model. Indeed, as they stand, such idealized mosaics as the bipolar, bifactor, simplex, radex and circumplex patterns are of no more than descriptive use, for the inferences that could be made from them to a scientific model have not been stated. Even descriptively most are frequently as misleading as useful, for real matrices can rarely be made to correspond exactly with them.

Nevertheless, it would be a service to factor analysis to have a taxonomy of ideal, stereotyped influence patterns, or SIPs, more systematically worked out than has yet been done. As I pointed out when I introduced this topic, one can vary the number of factors, the coverage of lower order variables, and the pattern of overlap with other factors, etc. When this is extended to include higher order factors the number of possible SIPs is very great. By way of a beginning, what appear to be the twelve most important have been systematically set out in Fig. 6. In this case the representation is at the level of stereotyped *influence* patterns, which go beyond the descriptive mosaics, though they imply quite specific mosaics. However, since the authors of some mosaics have never stated their assumptions in terms of an underlying scientific model, the use of older titles, like bifactor and circumplex, may be debated.

Let it be said forthwith that beyond SIP III no methods have yet been demonstrated whereby factor analysis could go directly to the scientific model from the data. One can infer what mosaics the later SIPs would imply in the actual matrices, but not, conversely, what the matrix mosaics would unambiguously imply in terms of SIPs. Parenthetically, SIP V, and others at that level, destroy any possibility of categorization as factors and variables. Any measurement, or estimated measurement, could be either or both.

It behoves us, at this point, to define more closely the status and meaning of the term factor. The epistemological status of a simple structure factor is that of an empirical *construct*,<sup>10</sup> but it commonly has

- represents a variable
- represents a primary factor
- represents a higher order factor

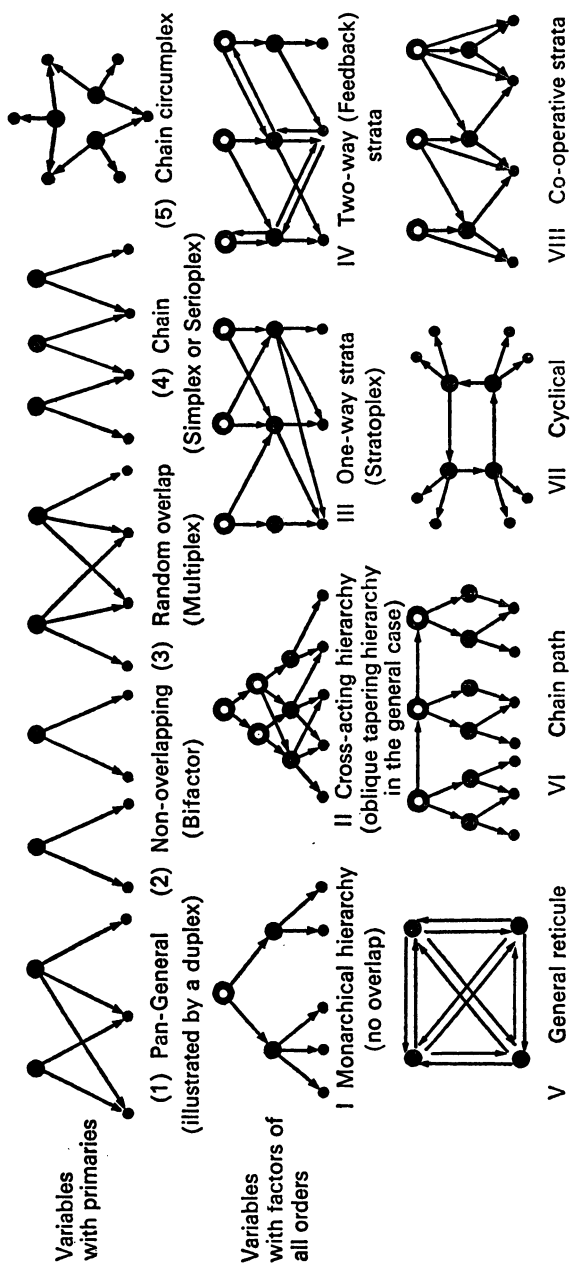


FIG. 6

*Some outstanding Stereotyped Influence Patterns (SIPs) for describing factor and variable interrelations*



degrees of 'surplus meaning', borrowed from beyond the immediate system, which can turn it into a theoretical concept (see also Henrysson<sup>29</sup>). However, for such uniquely determined common factors in general, our view is that their only surplus meaning, beyond what is given by the properties of a *mathematical factor*, is that they are *influences*. Operationally, this additional meaning is derived from simple structure and confactor rotation operations.

The expression 'influence' seeks to define a broader concept of which both 'condition' and 'cause' are sub-species. In the Thurstone box problem,<sup>39</sup> for example, one would scarcely, in normal semantics, call 'length' a cause, and, in the Cattell-Dickman ball plasmode,<sup>18</sup> the weight factor is again a debatable cause—by some uses of 'cause'. The required generic expression is perhaps *determiner* rather than *influence*; for the variables loaded by length could not exist if a box had no length, while the change of velocity which one colliding ball will impart to another is determined if not caused (partly) by the weight of the ball. A *cause* is thus reserved for that special determiner in which we have additional *temporal sequence data*, which justifies the idea of one determiner acting upon another. To go beyond this would land one in metaphysics.

A factor can only be recognized as a cause when, in addition to the correlational evidence from *simultaneous* measurements, we possess evidence of an invariable *sequence* of the two in the relation which resulted in the correlation, for any correlation between A and B can mean that A affected B, that changes in B caused changes in A, or that some third cause produced changes in both of them. However, we argue that in factor analysis inferences about causal direction can be made *even when no actual time observations are available in our experimental data*. They can be made from *indirect* evidence at a high level of probability.

This evidence is of the same general nature as that invoked by the archaeologist, the astronomer, the geologist and other scientists denied the advantage of actually being present when the causal actions of interest to them occur. It depends on the fact that time sequences may be translated or preserved in other media, i.e. in space or temperature differences. Thus an archaeologist infers that Troy IV followed Troy III, because its deposits are spatially above those of the latter, and an astronomer infers that a red star was formed before a blue one. So here, we use the structural pattern of the mathematical relationships to infer that one factor is a determiner or causal influence operating upon

another. On closer examination it will be seen that the two main independent principles proposed for uniquely fixing rotation and the resolution pattern, namely, simple structure and confactor congruence, operate strictly on the assumption that a factor is a determiner. Thus in the former what is typically a cause will affect only a minority of a random sample of variables, while in the latter it is assumed that a unitary influence, as it becomes more powerful, will affect *all* the things it normally affects with proportionately greater variance contribution.

The crowning purpose of a fully developed factor analytic technique should be, by such devices, to trace the causal connections among the factors and variables. Our aim, then, is to seek the basis for such inferences in the general case where no ulterior information is available.

The problem is, therefore, 'What inferences can be made about directions of causal action among variables and higher and lower order factors *in simple, R-technique* factor analysis?' The nearest approach to a systematic attack on this problem is Sewall Wright's<sup>42</sup> development of the *path coefficient* in relation to ordinary correlations. However, Wright's method *assumes* the direction of causal action entirely from ulterior evidence, and therefore contributes nothing to the problem of inferring such action retrospectively from the structure of the correlations.

## THE EVIDENCE OF STRUCTURE OBTAINABLE FROM DV MATRICES

No solution is attempted here for the full possibilities of mutual interaction depicted in Fig. 5, or the later SIPs of Fig. 6. Instead we propose the less ambitious problem which arises if we assume an influence structure such as is found in the simpler scientific models given in Fig. 4, and in the examples (1) to (4) in Fig. 6. That is, a system in which the factors are the independent and the variables the dependent variables. If we restrict discussion of method to simple structure, then what kind of simple structure pattern must appear in order for us to infer the particular number of factors, overlaps, etc., which occur in such a relation?

The postulate that a factor is a cause *when it affects only a minority of variables* in a widely representative selection, leads to the conclusion that the rotation which locates the factors as causes will be one in which every column has a maximum number of zeros. This leads to the in-

evitable arithmetical consequence that in seeking a solution which maximizes zeros in the *columns* we are automatically maximizing the zeros in the matrix as a whole and therefore maximizing zeros in the *rows* of the same matrix.

The effect of this conclusion upon our attempt to infer causal direction is disastrous, for whereas a predominance of zeros in *columns* argues for factors being causes, a predominance in the *rows* argues for the *factors* being dependent and the variables being the influences. This difficulty can be resolved, however, if we follow the logic of oblique solution to its ultimate conclusions. Indeed, the failure of simple structure when thus applied to the orthogonal case is only one more proof that the orthogonal resolution of mathematical factor analysis is wrong in the scientific sense, i.e., that it is not a model which can fit even the most general requirements of a causal system.

The next step in our argument depends upon the general relations among the six main dimension-variable (DV) matrices (Cattell<sup>11</sup>). These variants of the dimension-variable relation matrix, possible in the oblique case, are as follows:

1. The reference vector structure matrix, written  $V_{rs}$ . This is the usual matrix obtained by rotation for simple structure, yielding *correlations* between variables and reference vectors.

2. The factor pattern matrix,  $V_{fp}$ , which gives the *loadings* of factors on variables. It is proportional by columns to  $V_{rs}$  and retains the same simple structure:

$$\text{i} \quad V_{fp} = V_{rs}D$$

3. The factor structure matrix,  $V_{fs}$ , namely, the *correlations* between factors and variables:

$$\text{ii} \quad V_{fs} = V_{fp}C_f$$

(where  $C_f$  is the matrix of correlations among factors).

4. The reference vector pattern matrix,  $V_{rp} = V_{fs}D^{-1}$ .

5. The factor estimation matrix,  $V_{fe}$ , which sets out the weights to be given to the variables to obtain the best estimate of each factor. This can be calculated either in the usual way,

$$\text{iii} \quad V_{fe} = V'_{fs}R^{-1}$$

(where  $R$  is the correlation matrix among variables) or in Tucker's fashion—

$$V_{fe} = (V'_{fp}V_{fp})^{-1} V'_{fp}$$

6. The dissociated factor pattern matrix,  $V_{afe}$ , the contribution made by a factor to the variance of a variable in *dissociation from the effect of higher order factors*.

Now, as we suggested above, an  $r$  of, say, 0.8, may mean that 64 per cent of the variance of  $a$  will disappear when  $b$  is held constant or that 64 per cent of the variance of  $b$  will disappear if  $a$  is held constant. The former is nonsense if  $a$  is the cause of  $b$ , since nothing we do about 'holding' the consequence  $b$  need have any effect on  $a$ . With this in mind we may note first that the  $V_{rs}$  and  $V_{fs}$  matrices (as well as  $V_{afe}$  Cattell<sup>11</sup> not here described) are non-committal statistical figures, simply stating correlations, but a *theory of causal action* is implicitly written into the formulae  $V_{fp}$ ,  $V_{fe}$  and  $V_{afe}$  if they are derived by simple structure.

The factor pattern matrix,  $V_{fp}$ , tells us how much each factor contributes to the variance of each variable, considering how much is also contributed by other oblique factors.  $V_{fe}$  tells us how we can weight the variables to maximize the multiple correlation of the estimate of the factor with the true factor—assuming the factor position to be that settled upon in  $V_{fp}$  rotations. There is thus no mathematical principle whatever which requires that simple structure shall appear in  $V_{fe}$ . This matrix gives the contributions of the variables to the factor assuming, not that they are causal, but that they are manifestations or constituents from which the factor can be estimated.

However, we occasionally meet instances where we may strongly suspect on psychological grounds that a certain first order factor is a causal contributor to the second order.\* That is to say, one of the alternative possibilities already written into Fig. 5 (such as  $f_1 \rightarrow F_1$ ) is suspected, in which the direction of causal action is opposite to the usual direction, either simultaneously with or as a substitute to it. A general solution of the reticular influence model is beyond the scope of this paper, but we suggest that a solution may be profitably pursued by making comparisons among the six DV matrices (Cattell<sup>11</sup>), though this will obviously be extremely complicated. Some awareness of and reference to these matrices is necessary even in the solution of the 'one-way strata' model on which we shall now concentrate.

Such a 'stratoplex' model appears likely to have a good fit, at least as a first approximation, in the field of ability and in the field of motiva-

\* As in the case of the anxiety factor considered later in this paper.

tion where later acquired habit systems will subside to (or be reinforced by) earlier systems, as in the concept of the dynamic lattice (Cattell<sup>10</sup>). It also has application to group dynamics and to other areas of social psychology. In short, although compared to the general network model, it makes simplified assumptions, the methodology for handling it is well worth investigating.

### **THE CONCEPT AND OPERATIONAL RECOGNITION OF FACTOR STRATA**

On the assumption that only the reticular model is fitted to the interpretive use of factor analysis in the universal case, we have rejected the monarchical hierarchical model for anything but investigations which claim information from ulterior sources. However, we recognize that the general reticular solution is too difficult, and we suggest thorough treatment of the one way strata SIP. Examples (1) and (2), in Fig. 6, show that the monarchical and the tapering hierarchy are special forms within the general one-way strata model III, but thereafter all SIPs include either a two-way (feedback) action, or an action of factors upon others of the same level, or a leap-frogging in which higher orders set directly on more than one lower order.

In the strata model we assume an indefinite number of factors operating at each of a number of operationally-defined levels. Theoretically there could be influence in both directions, making a two-way as well as a one-way model possible, but we shall consider only a one-way influence model (higher order affecting only lower) which for brevity we will call the stratoplex model. The 'peer status' of factors in one and the same stratum is defined initially by the functional condition that all members of the same stratum influence only members of the next 'lower' stratum. They do not influence each other or members of the peer group above them.

In investigating the operational steps necessary to obtain evidence in terms of the stratoplex, one must begin by recognizing that factor 'order' is not the same thing as factor 'stratum'. Order is given immediately in terms of operations: the factors, say *A* to *D*, obtained by factoring entities *a* to *k*, are one order higher. Stratum indicates an influence relation in a model, and probably cannot be safely inferred from a single factoring operation, though it is true that operational proof of order is a large part of the evidence.\*

\* The difference becomes evident if one thinks of order studies in a network which happens to be a circumplex, where successive orders are not strata, but rings.

The main reason why order operations and strata steps are not the same is that in the initial sampling of variables, at any level, one cannot be sure that one has started with variables that are themselves all in the same stratum.

There is not space here to deal with all the alternative possibilities that may befall the investigator. Suffice it to say that if the sample of variables chosen does indeed contain nothing but members from one and the same basic level, then the first order factors will be all on one stratum higher. In the other cases, four in number (see Fig. 7), it can be shown that i. accidental inclusion of one higher stratum measure in the factoring of a set from a lower level will result in the appearance of the former as a specific, i.e. as a factor with only one variable loaded on it, whereas *two or more* will result in the appearance of a set of factors some of which are from one stratum level and some from another; ii. the factors themselves, as individual factors, will not be mixed, and iii. what appears operationally is either a second stratum or a third stratum factor; though a considerable problem remains in recognizing (a) when this heterogeneity of strata in the single order of a single experiment has happened, and (b) which factor is at which stratum level.

Elsewhere (Cattell<sup>10</sup>) the problem of defining the stratum to which a factor belongs has been decided on the basis of the *density* of representation occurring in the sample of variables, and thus rests on our ability objectively to define a *population of variables* and a *density of representation* of such variables, in a factor analysis. If the variables are close together, highly similar, and with a high 'density of representation' they will factor largely into lower stratum factors, whereas if widely separated and diverse, i.e. of sparse density, the first factor analysis will in the main go direct to higher stratum-order factors. It is known, for example, in some instances (Cattell,<sup>10</sup> Cattell and Scheier<sup>20</sup>), that second order factors in questionnaires become first order factors in objective personality tests, possibly because questionnaire items make fine distinctions in behaviour which in a miniature situational response is covered by a single variable.

However, the concept of distance between variables in the density definition must be quite independent of correlational operations, *which yield distance only in the sense used in Mahalanobis' generalized distance function,  $D$ , or Cattell's pattern similarity coefficient,  $r_p$* . Without this, no independent check would be gained. The advantage of an exact density concept and measurement operation would be that one could from the

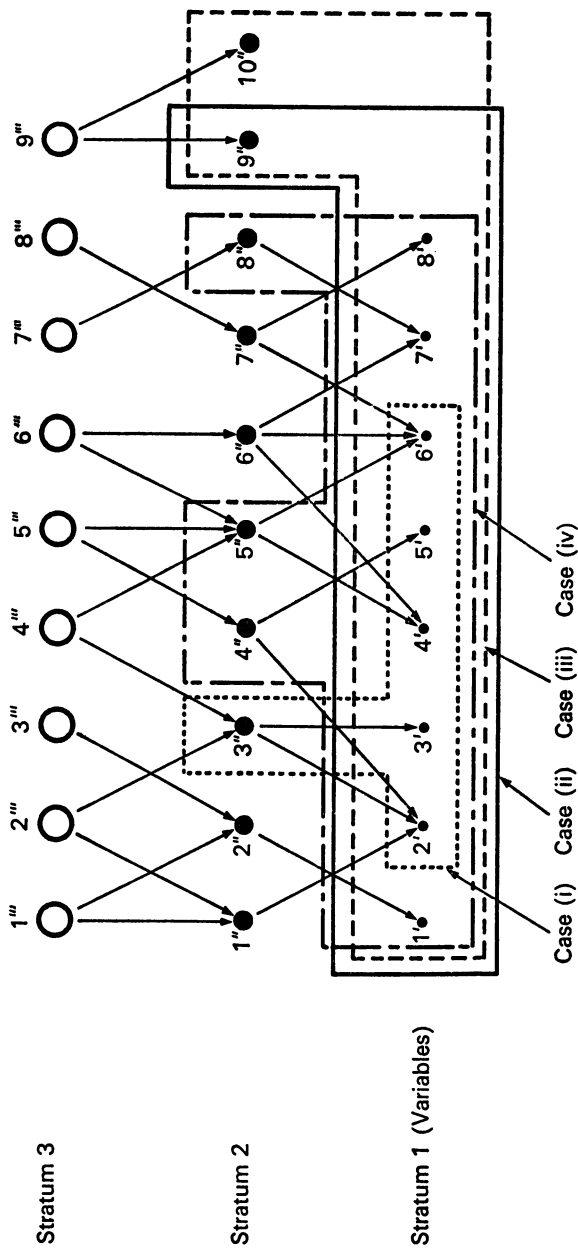


FIG. 7

*Relation to inferences about factor strata of operation revealing factor order*

Case (i) One primary factor from stratum 2 is accidentally included with variables from stratum 1, and it has psychological representation in the other first stratum variables. No problem arises.

Case (ii) One primary factor from 2 included in 1 (as above), and it has no psychological representation in stratum 1 variables.

Case (iii) Two or more primaries from 1 accidentally included in 2, with no psychological representation in first stratum variables.

Case (iv) Two or more primaries from 1 included in 2, and they have psychological representation in first stratum variables.

beginning, without evidence from factoring, predict what order of factors one would be obtaining. Suggestions for independent operations for defining density of representation of variables have been made elsewhere (Cattell<sup>10</sup>). However, as these operations are different, the strata concepts reached might not necessarily rest on the notion of *influence* direction. Indeed if the criterion of strata order is that variables in stratum X influence variables in stratum Y, and we can depend upon simple structure to witness this, then the present approach, though difficult, is sufficient.

The stratoplex model is only a specialized and simplified form of the reticular model and there is no more than a probability that it will hold in psychological experimental situations. If it holds there are methods, as we have seen, of establishing, by examination of successive factor analyses, what the strata structure in a given set of variables really is. The simple procedure of saying that the strata are identical with the operational orders is insufficient and incorrect. A more subtle manner of inference is required, from planned and re-planned experiments, comparing operational orders in each.\* The evidence that the model itself is fitting must come from the consistency of the strata picture it yields in different samples of variables.

While investigation and formulation of the problem of stratum order may challenge some of the substantive psychological concepts already formulated, yet it is nevertheless reassuring in more general terms. For it shows that, granted strata, our factor analytic operations will not merely yield sets which are *mixtures* of first and second strata factor variables, neither will it produce individual factors which remain conceptually neither at one level nor the other.† In short, it does *not* produce strata results which are purely at the mercy of accidents of sampling, though factors from different strata will sometimes

\* The past confusion between order and stratum suggests that a number of conclusions about order need to be re-examined, for it is likely that some factors now thought to be of the same order are actually at different strata levels. A probable instance is that factors B (intelligence) and G (superego strength), considered as primaries in the 16 Personality Factor Questionnaire, tend to stand out as specifics in the second-order factoring (Gorsuch,<sup>28</sup>) and are therefore probably cognate with the second stratum factors of *exvia-invia*, anxiety, *cortetia*, etc., found in questionnaires and objective tests.

† There could, of course, be an *intermediate* stratum running part way between two strata, like a half floor in the contemporary 'split level' house. This, however, involves problems of the general reticular model and is best considered in that context.



*initially* be produced together in one order. Indeed, simple structure criteria, pursued with alert co-ordination among successive studies, and with an eye to the possibility of an initially heterogeneous bag at any one order, seems fully capable of yielding definitive knowledge of strata relationships.

#### FORMULAE FOR CONTRIBUTIONS OF FACTOR VARIANCE ACROSS A NETWORK

The first aim of basic research in any scientific area must be to determine the reticular structure of functional relationships. Only when the pattern and sequence of interaction among such factors is accurately mapped and understood is it possible to seek laws or check quantitative relations assumed to obtain between varying influences by holding constant this or that factor. Still more, perhaps, in applied research, it becomes necessary to estimate how much an influence  $X$  will contribute to a dependent variable  $Y$  when it acts through several intervening variables.

A complete situation for the stratoplex is given below.\*

$R_0$ ,  $R_1$  and  $R_2$  have been used respectively for the successive matrices of correlations between variables, factors and still higher orders, as the emphasis here is on an orderly succession.  $V_{fp}$  and  $V_{fs}$  indicate factor pattern and factor structure variable matrices respectively.<sup>16</sup> First stratum, second stratum, etc., factors are called primaries, secondaries, tertiaries, etc.

Since it is our purpose to deal with variance contributions via the mutual, variable-to-factor and factor-to-variable  $V$  matrices ( $V_1$ ,  $V_2$ ,  $V_3$  for the ascending orders), and with results in terms of the score matrices and the estimation matrices for factor and variable scores, we shall use the symbols  $Z_0$ ,  $Z_1$ ,  $Z_2$ ,  $Z_3$ , etc., for the standard score matrices (for  $N$  people), the subscripts indicating that the scores are respectively for variables, primaries, secondaries, tertiaries and so on.

The basic formula  $v$  below, which relates factor loadings to correlations among variables, derives its orthogonal origins from the analysis of a correlation matrix into latent roots and vectors. Although this extraction of orthogonal factors is the initial step in factor analysis, we deal with the final oblique resolution which the psychologist is accus-

\* Wright's path coefficient is also useful (Wright<sup>42</sup>). But the investigator using the stratoplex model is not simply asking about correlation, but about variance contribution acting in a specified direction.

tomed to use once simple structure rotation conditions have been met. The orthogonal position can always be readily handled as a special case of this. For example, in formula v,  $R_1$  reduces to an identity matrix (in effect disappears) when factors are orthogonal, and  $V_{fp}$  then becomes identical with  $V_{fs}$ .

$$\text{v} \quad R_{ro} = V_{fp \cdot 1} R_1 V'_{fp \cdot 1},$$

where  $R_{ro}$  is the *reduced* correlation matrix, and the subscript  $r$  stands for 'reduced'.

To use the scores on these factors to restore scores on the original variables we proceed with:

$$\text{vi} \quad Z'_0 = V_{fp1} Z'_1 + U_1 Z'_{u1},$$

where  $Z_0$  is the test-score matrix,  $Z_1$  the common factor score matrix,  $U_1$  the matrix of orthogonal, specific (unique) factors for the  $n$  variables in the rows of the  $V_{fp}$  common factor matrix, and  $Z_{u1}$  is the score matrix of  $N$  people on the  $n$  unique factors (specific plus error). (Parenthetically,  $Z$  is an  $N \times n$  matrix ( $N$  people,  $n$  variables) and  $Z_1$  is an  $N \times k$  ( $k$  factors).)

The successive steps carrying correlation and score relations only to the third order (see equations vii and viii below) suffice to establish the generalization up to any order. The successive (primary and secondary) correlation matrices can be analysed just like the variable correlation matrix in v, thus:

$$\text{vii} \quad \begin{aligned} (a) \quad R_{r1} &= V_{fp2} R_2 V'_{fp2} \\ (b) \quad R_{r2} &= V_{fp3} R_3 V'_{fp3}. \end{aligned}$$

The successive factor score matrices beyond vi above will relate as in viii:

$$\text{viii} \quad \begin{aligned} (a) \quad Z'_1 &= V_{fp2} Z'_2 + U_2 Z'_{u2} \\ (b) \quad Z'_2 &= V_{fp3} Z'_3 + U_3 Z'_{u3}. \end{aligned}$$

From these we can formulate the restoration of a correlation matrix among variables or factors at any one stratum from the factor loadings and the correlations for an immediately higher stratum by the recursion formula:

$$\text{ix} \quad R_{rx} = V_{fp(x+1)} R_{(x+1)} V'_{fp(x+1)} = R_x - U^2_x,$$

where  $R$  stands for the correlation matrix with ones in the diagonal, and  $R_r$  for the reduced matrix.

Similarly, the relations between scores of any two successive strata can be generalized, as in x:

$$x \quad Z'_x = V_{fp(x+1)}Z'_{(x+1)} + U_{(x+1)}Z'_{u(x+1)}.$$

From these simple, single-stage recursion formulae we can proceed to the formulation of a transformation over any number of stages, i.e. to express the contribution to a set of variables or factors of a factor remote from it in the stratoplex. In accordance with our general model we use the  $V_{fp}$  matrix because of the assumption (Cattell<sup>111</sup>) that influence is in the direction of factors upon variables. By the statistical properties of factoring we are also compelled to assume that our formulae deal with the usual tapered-off hierarchy—a chopped-out triangular wedge from the total strata.

The general formula for restoring the lower order correlation matrix, when beginning with factors  $n$  orders higher, now becomes:

$$xi \quad R_{ro} = V_{fp1}V_{fp2} \dots V_{fpn}R_nV'_{fpn} \dots V'_{fp2}V'_{fp2}.$$

In the case where the general reticulum happens to present a true tapering pyramidal hierarchy, finishing at the  $n$ th factor extraction with a *single*, massive, general factor (as some believe to be true in the general cognitive field) then the highest matrix,  $R_n$ , becomes unity (one factor identity matrix) and drops out of the formula. It must be stressed, however, that unless one has ulterior evidence about the notation of this final single factor its character is fictional. A general factor exists, but it is indeterminate. Fortunately this does not affect the determinacy of the lower order factors, even in the tapering hierarchy, but it affects the true monarchical hierarchy if an attempt is made to rotate the specifics of the lower order factor into a special conformity to it.

To estimate *scores* on lower order from scores on higher order factors we use formula xiii below.

From vi we can proceed, by substitution for  $Z'_1$  as in viii (a) above, to:

$$xii \quad Z'_0 = V_{fp1}V_{fp2}Z'_2 + V_{fp1}U_2Z'_{u2} + U_1Z'_{u1},$$

and from vi, xii and x, it will be evident that a general formula can be written for restoration of a lower stratum score matrix from the data of higher strata, at any degree of distance,  $d = n$ , as in xiii below:

$$xiii \quad Z'_0 = V_{fp1} \dots V_{fpd}Z'_d + \sum_{d=1}^{d=n} V_{fp1} \dots V_{fp(d-1)}U_dZ'_{ud} + UZ'_{u1}$$

If desired, the notation of supermatrices can be used (see Horst, 1963<sup>43</sup>), thus:

$$V = V_{fp1}V_{fp2} \dots V_{fpd}:V_{fp1}V_{fp2} \dots V_{fp(d-1)}U_d \dots :V_{fp1}U_2:U_1,$$

and

$$Z' = Z'_d:Z'_{ud}: \dots Z'_{u2}:Z'_{u1}.$$

Then xiii can be written:

$$\text{xiv} \quad Z' = VZ'.$$

Here the observable scores,  $Z$ , are written as the product of a factor pattern and a factor score supermatrix.

FIG. 8

*The higher order projection matrices: contrast of Cattell-White and Schmid-Leiman loadings of higher order factors on variables*

*By Cattell-White formula*

	First order				$h^2$	Second order		$h^2$	3rd	$h^2$
	1'	2'	3'	4'		1''	2''		order	
1	.00	.00	.00	.50	.25	.00	.30	.09	.21	.04
2	.00	.50	.00	.00	.25	.00	.00	.00	.00	.00
3	.00	-.50	.60	.00	.61	.36	-.30	.09	.09	.01
4	.50	.60	.00	.00	.61	.25	.00	.06	.21	.04
5	-.50	.60	.00	.00	.61	-.25	.00	.06	-.21	.04
6	.60	.00	.50	.00	.70	.60	-.25	.24	.33	.11
7	-.60	.00	-.50	.00	.70	-.60	.25	.24	-.33	.11
8	.00	.00	.60	-.60	.79	.36	-.66	.28	-.16	.03
9	.00	.00	.00	.50	.25	.00	.30	.09	.21	.04
10	.00	.00	.00	.60	.36	.00	.36	.13	.26	.07

*Inter-correlations of first order factors (Cattell-White)*

	1'	2'	3'	4'
1'	100	00	15	18
2'		100	00	00
3'			100	-08
4'				100

FIG. 8 (continued)

## Inter-correlations of second order factors (Cattell-White)

	1''	2''
1''	100	60
2''		100

## By Schmid-Leiman Formula

	First order				$h^2$	Second order		$h^2$	3rd order	$h^2$	$Eh^2$
	1'	2'	3'	4'		1''	2''		1'''		
1	.00	.00	.00	.40	.16	.00	.21	.04	.21	.04	.25
2	.00	.50	.00	.00	.25	.00	.00	.00	.00	.00	.25
3	.00	-.50	.52	.00	.52	.19	-.21	.08	.09	.01	.61
4	.43	.60	.00	.00	.55	.13	.00	.02	.21	.04	.61
5	-.43	.60	.00	.00	.55	-.13	.00	.02	-.21	.04	.61
6	.52	.00	.43	.00	.46	.32	-.18	.13	.33	.11	.70
7	-.52	.00	-.43	.00	.46	-.32	.18	.13	-.33	.11	.70
8	.00	.00	.52	-.48	.50	.19	-.47	.26	-.16	.03	.79
9	.00	.00	.00	.40	.16	.00	.21	.04	.21	.04	.25
10	.00	.00	.00	.48	.23	.00	.25	.06	.26	.07	.36

For notes on Fig. 8 see Addendum on p. 266.

The formulae given above appear to contribute the first *general* exposition of lower and higher strata interrelationships in scores and correlations, although a well-known formula dealing with a special aspect of this problem has been devised by Schmid-Leiman.<sup>37</sup> They developed a 'procedure for rotating an oblique simple structure into a hierarchical factor solution' (*op. cit.*, p. 56). The procedure they describe is directed specifically to a pure hierarchical structure finishing in a single third-order factor. They derive a factor matrix,  $B$ , which differs from our formulae xii, xiii and xiv in disregarding  $U_1$ . They do not point out, as we have done here, that their orthogonalized factors correspond to the higher order primary factors along with the lower order unique factors, for their approach has a different purpose and conceptual framework.

The aim of the Schmid-Leiman formula was to determine the loadings of higher order factors directly upon the variables after the

factors have been rotated into *orthogonal positions*. In these positions all but the monarchical factor are merely the truncated specifics of the original oblique factors they represent, i.e. specifics after the higher order variance is abstracted.

Here we propose to contrast the Schmid–Leiman version of what we may call the Higher-Order-Factor-To-Variable, or, Higher-Order-Projection matrix, with the Cattell–White alternative formulation.\*

The main differences are as follows:

1. Although both normally *begin* with factors rotated to simple structure, the Cattell–White transformation retains at every stratum these unique oblique structures, whereas the Schmid–Leiman extracts at each stratum the higher order variance, and leaves at each lower level only the orthogonal residual specifics from the oblique factors, not the factors in their full variance. The final step in the Schmid–Leiman is to end in a single general factor or a set of orthogonal factors.

Thus the loadings of the second order factors on the variables, in the Higher-Order-Projection matrix, which, we will symbolize as  $V_{fpII}$ , become by the Cattell–White formula xvi below, equal to  $V_{fp1}$  times  $V_{fp2}$ , whereas in the Schmid–Leiman it is  $V_{fp1}$  times  $V_{fp2}$  times  $U_2$ . In the latter, the loadings on the variables are not those of the true secondaries but only the projections of the truncated remains of these factors after much of their variance has been taken into factors of a still higher order.

2. The Schmid–Leiman HH Projection matrix cannot reach stability and completeness unless and until the successive factorings end either in a set of factors whose simple structure is naturally orthogonal—an extremely rare condition in our experience—or in a single general factor. The corresponding Cattell–White HH Projection matrix has a functional completeness when terminated at any stratum.

3. In the Cattell–White HH Projection matrix the summed squares of loadings of factors on a variable will *not* sum to the communality of the original unrotated matrix whereas in the Schmid–Leiman it will. This contrast is part of the conceptual difference that the  $V_{cs}$  is a matrix of loadings only, not correlations, in the C–W case, but of correlations with orthogonal factors in the S–L case.

The  $h^2$  values are smaller in the S–L, since they are confined to one stratum with all higher order variance taken out, whereas in the C–W

\* This formulation was developed while Owen White was a research assistant to the writer.

they include the variance for the given stratum in addition to that which would go into all higher strata. Contrast of two successive  $h^2$  columns in the C-W shows how much predictive power would be lost by dealing only with the factors at a higher order.

4. The loadings in the S-L matrix, as far as the first order factors are concerned, will correspond to those defined as the dissociated factor matrix,  $V_{dfc}$ , above. The simple structure on these will be the same as for the ordinary oblique factors, but the significant loadings will all be reduced by the same constant ratio,  $V_j$ , on any one factor. Thus the S-L factors will have smaller loadings than those in the C-W, and will yield, too, a different rank order of contribution to the variables. A row in the S-L composite will thus look quite different from, and have no simple relation to, a row in the C-W, except that zeros will appear in the same places.

The derivation of the Cattell-White formula is clearest, perhaps, if one considers the general formula xi above, for restoring the reduced correlation matrix from factor pattern matrices. If we symbolize the Cattell-White loadings of the  $n$ th order factors *directly on the variables* by  $V_{fp \cdot N}$ , this too will restore the correlation matrix and we can write:

$$\text{xv} \quad R_{r0} = V_{fp1} \dots V_{fpn} R_n V'_{fpn} \dots V'_{fp1} = V_{fpN} R_n V'_{fpN},$$

whence

$$\text{xvi} \quad V_{fpN} = V_{fp1} \dots V_{fpn} \text{ (C-W formula),}$$

which forms the systematic basis of calculation for all the values in the Cattell-White Higher-Order-Projection matrix, which differentiates it from the corresponding Schmid-Leiman matrix.

### THE SIMPLE STRUCTURE OF HIGHER ORDER FACTORS AND THE PROBLEM OF PSEUDO-SECOND ORDER FACTORS

The main uses of these two higher order projection matrices would appear to be: (a) to predict variable scores from factors; (b) to calculate formulae ( $V_{fe}'s$ ) for estimating higher order factors directly from variables; (c) to decide when it is worth while—in terms of accuracy lost for economy gained—to shift from measuring several first order to fewer higher order factors; (d) to provide an alternative basis for the rotation and identification of higher order factors; and (e) to provide an alternative basis for interpreting higher order factors.

The last two uses—(d) and (e) above—require us to fish in rather deep

theoretical waters. Several writers have wanted to use the C-W or S-L matrices to provide a new basis for rotating higher order factors by rotating them for simple structure *directly on the variables*. The logic of this seems faulty. If the factors influence directly only *the next lower stratum* of factors, then they should be explicitly rotated to *simple structure on that stratum*, an assumption underlying formula xvi. But does this not also imply, as many psychologists seem to have tacitly assumed, that when the corresponding loadings on variables are examined these will *also* show a simple structure pattern? In other words, is simple structure on  $V_{fpn}$  automatically a simple structure on  $V_{fp(n-1)}$ ?

Let us take a specific level, that of the second order factor projections on the variables. By virtue of the simple structure in the first order  $V_{fp1}$ , and in the second order  $V_{fp2}$ , will there be simple structure in the second order? Applying the Cattell-White formula we have:

$$\text{xvii} \quad V_{fp \cdot II} = V_{fp1} \cdot V_{fp2}$$

$(n \times 1) \quad (n \times k) \quad (k \times 1)$

If, on average,  $1/p$ th of the variables in a row of  $V_{fp1}$  and a column of  $V_{fp2}$  are zero, the chances of getting  $1/p$ th of the row-column products to be zero is less than unity. The fraction of *single* products which are zero is *greater* than  $p$ , namely  $\frac{2p(1-p) + p^2}{1} = 2p - p^2$  (where  $p < 1$ ).

But in the matrix multiplication we take  $k$  products at a time and the chance of one of these containing nothing but zeros is far smaller. Except for some quite special and fortunate relation between the positive and negative values in the row and column, a zero will appear in  $V_{fpII}$  only when *every* product of a  $k$  row by a  $k$  column happens to be a zero. In practice the hyperplane entries will not be *exact* zeros, but if we set the same standard of hyperplane width in  $V_{fp1}$ ,  $V_{fp2}$  and  $V_{fpII}$  this will not affect the issue. To demonstrate this concretely we have taken in Fig. 9 a hypothetical example, in which 60 per cent of the variables are in the hyperplane of the primaries,  $V_{fpI}$ , and 50 per cent in the hyperplane of the secondaries,  $V_{fpII}$ .

As the theoretical introduction would suggest, the outcome on the  $V_{fpII}$  proves to be much poorer than the hyperplane on the primaries and secondaries. There is a progressively poorer hyperplane *on the initial variables* as we move to higher order factors. It might be that a peculiar property of real data is that simple structure on the primary and secondary is also maximally simple on  $V_{fpII}$ . To test this we



have taken a well-known 44 variables example ( $N = 300$ ) consisting of 44 items from the Sixteen Personality Factor Questionnaire used in a number of cross-cultural researches (Cattell, Pichot and Rennes<sup>19</sup>) to check the cross-cultural constancy of factor structure on the eleven factors represented. The original primaries stand up well and have been factored to six second orders (already well known from other factor-

FIG. 9

Calculations of higher order loadings on variables by Cattell-White formula, showing effects on simple structure

$V_{fp1}$					×	$V_{fp2}$			=	$V_{fpII}$	
<i>First order factors</i>						<i>Second order factors</i>				<i>Second order factors</i>	
Variables	1'	2'	3'	4'		1''	2''		I	II	
1	0	0	0	5	×	1'	5	0	0	30	
2	0	5	0	0		2'	0	0	0	0	
3	0	-5	6	0		3'	6	-5	36	-30	
4	5	6	0	0		4'	0	6	25	0	
5	-5	6	0	0		Good (50 per cent) Hyperplane count			-25	0	
6	6	0	5	0					60	-25	
7	-6	0	-5	0					-60	25	
8	0	0	6	-6					36	-66	
9	0	0	0	5					0	30	
10	0	0	0	6		0	36				

Good (60 per cent)  
Hyperplane count

Poor (35 per cent)  
Hyperplane count

FIG. 9 (continued)

$V_{fpII}$			×	$V_{fp3}$		=	$V_{fpIII}$	
	I	II			I'''			I'''
1	0	30		1''	85		1	21
2	0	0		2''	71		2	00
3	36	- 30					3	09
4	25	0					4	21
5	- 25	0					5	- 21
6	60	- 25					6	33
7	- 60	25					7	- 33
8	36	- 66					8	- 16
9	0	30					9	21
10	0	36					10	26

Poor (35 per cent)  
Hyperplane count

Still poorer  
(25 per cent)  
Hyperplane  
count

ings of the 16P.F.). These represent Anxiety, Exvia-Invia (Extraversion-Introversion) and two other factors we need not discuss here. The loadings of these four on the items have been calculated by the Cattell-White formula and some have been set out in Fig. 10. From the full table we find that 60 out of 176 loadings stand in the  $\pm .10$  hyperplane, a value of 34 per cent compared with 59 per cent in the same hyperplane width in the 484 loadings of the primary factors (Cattell, Pichot and Rennes<sup>19</sup>). The result again suggests that no simple structure exists on  $V_{fpII}$  comparable to that on  $V_{fp}$ .

FIG. 10

Loadings (Cattell-White formula) of 16 P.F. items directly on second order factors of Anxiety, Exvia—Invia, etc. (Data from Japanese sample)

	Anxiety $F_1$	Exvia $F_2$	Path. $F_3$	Unknown $F_4$
1 Would you rather (if salary, prestige, etc., were equal) do the work of (a) a physicist, or (b) a salesman for some invention? (An A Factor variable: loading .81.)	30	43	43	— 52
2 Is your health unpredictable, forcing you frequently to alter your plans? (A C Factor variable: loading .60.)	— 28	08	— 18	— 11
3 Are you considered a lively, enthusiastic (perhaps too lively and enthusiastic) person? (An F Factor variable: loading .64.)	15	52	00	— 10
4 Would you like the kind of job that offers change, travel and variety (in spite of other drawbacks)? (An F Factor variable: loading .31.)	31	23	— 07	— 18
5 Do you like generally (a) to assume that you can meet difficulties as they arise, (b) or to plan a piece of work to meet all difficulties? (A G Factor variable: loading .31.)	05	17	06	16
6 Do you find it difficult to address or recite to a large group? (An H Factor variable: loading .71.)	— 46	60	22	10
7 Are you inclined to worry without any reason for doing so? (An O Factor variable: loading .69.)	40	10	— 13	— 07
8 Do you have periods of feeling grouchy when you just don't want to see anyone? (a) very rarely, (b) quite often. (An O Factor variable: loading .26.)	22	07	43	32
9 Do you frequently get in a state of tension and turmoil when thinking of the day's happenings? (A Q4 Factor variable: loading .30.)	18	— 04	33	— 12
10 Do you tend to get angry with people rather easily? (A Q4 Factor variable: loading .13.)	35	05	— 20	01

The second order anxiety factor loads the items from the primaries  $C(-)$ ,  $H(-)$ ,  $O$  and  $Q_4$  just as one would expect from the loadings of the latter on the second order. However, the loadings are lower than on the first orders and the hyperplane of zero loadings one would expect on the extraversion contributors  $A$  and  $F$  fails to appear. The *exvia* (extraversion) factor also loads the items of its primaries,  $A$ ,  $F$  and  $H$ —in this case as well as they load their primaries—but the hyperplane is again poorer than with primaries though still surprisingly good. The conclusion would seem to be that one might hope to infer the nature of second order factors from their projections directly on variables, but not with such accuracy as from the structure on primaries.

There are three alternative resolutions to second order structure and position—resolutions that various psychologists have entertained at different times by examining:

1. Simple structure of true second orders on the primaries;
2. Simple structure of true second orders on the variables;
3. Simple structure of pseudo-second orders on the primaries.

The second alternative is not necessarily the same as finding that degree of simple structure on the variables that corresponds to true simple structure on the primaries. It is possible that a *better* simple structure could be found *directly* on variables. We have rotated both examples to maximum simple structure, by the oblimax programme, with results shown in Fig. 11. The structure (Row 4), due largely to the unusually poor (36 per cent) count for secondaries on primaries in the second example, is a shade better (mean of 46 per cent versus 43 per cent) than that fixed by the simple structure on the primaries.

However, by the Bargmann test,<sup>3</sup> neither the 45 per cent nor the 47 per cent is significant.

The third alternative—resolution into pseudo-second orders—requires some description. In work published during the 40's and 50's, and even the 60's, the practice has been to take out only as many factors at the first order as one guessed there were factors at the second. It is then asserted that these, rotated in the greatly reduced space, have, for all practical purposes, the same meaning as second order factors. To take only those which have been used as a basis for more extensive theories, there are Eysenck's dysthymia and extraversion,<sup>22</sup> Peterson's general ego-super-ego-vs-delinquency dimension<sup>36</sup> and Becker's attempt to treat the 16 P.F. as a four factor scale.<sup>2</sup> Related, but not identical, are Spearman's general ability at a first order level, and, in

FIG. II

*Hyperplane counts on experimental matrices comparing possibilities of second and first order rotation*

	<i>Experiment 1, artificial data, 10 variables</i>	<i>Experiment 2 (44 variables) 16 P.F. data (Japanese sample)</i>
	<i>per cent</i> 60	<i>per cent</i> 50
1 Primary factors rotated to simple structure on variables	50	36
2 Secondary factors rotated to simple structure on primaries	35	38
3 Secondary factors projected on variables after being rotated to simple structure on primaries	45	47
4 Secondary factors rotated directly to maximum simple structure on variables	35	35
5 Pseudo secondary or 'mongrel' factors rotated to maximum simple structure on variables		

personality, the studies by Norman<sup>34</sup> and Tupes<sup>40</sup> which take out only five or six factors (still naming them as primaries, however) where, from similar ratings, Burt<sup>5</sup> and the present writer have preferred to take out a dozen or more.

These theoretical assertions, especially those of Becker and Eysenck, are the more misleading because there is, in fact, a general resemblance between the second order pattern and the first and second primaries when rotated in inadequate space. The overlap of these *space-deformed* factors with the true second orders is due to the fact that both explain the variance in, say, three or four factors, where perhaps fifteen may be necessary.

Even so, when closely examined (Fig. 12), the imitation, in loading pattern, of the true second orders by these pseudo-second orders is poor. This is because in one case the missing variance is a series of centroid factors, each a mixture of everything, while in the other it is the specific factor variance of the primaries, i.e. that part of the primaries which does not come into the second order common space. The hyperplanes to which one is most likely to rotate in the foreshortened, under-extracted space are those of the primary factors, which, however, are likely to be considerably blurred by being projected on the reduced space (see Diagram II, in Cattell<sup>6</sup>). The effect would be that of a primary confounded with and inflated by an approximate secondary.

Enough has been said, perhaps, to show that the guesswork involved in deciding how many second orders exist *before* one has taken out the primaries, and the inelegance of seeking a solution in short, deformed space, combine to make this 'pseudo-second order' approach scientifically indefensible. The cost is a wrong concept of the factors and a structure which, being composed as it were of rubble rather than fitted stone, is incapable of carrying us higher toward any dependable superstructure, e.g. of third order factors. It also prevents precise separation of such concepts as anxiety, introversion and neuroticism. Moreover, the hyperplanes are noticeably poorer than for the true primaries or the secondaries. This, and the illegitimate manner of reaching such resolutions, should suffice to warn factorists to avoid such mongrel concepts which are neither one thing nor the other.

We therefore tentatively conclude that: 1. The simple structure true second order factors on primaries is a little poorer than that of primaries upon variables; 2. the projection of the second orders, from their simple structure position found on primaries to their loadings on

FIG. 12

*Effect of extracting at first order the same number of factors as exist at second order*

*Projection of true second order factors directly on variables (from Fig. 9)*

	1''	2''
1	0	30
2	0	0
3	36	-30
4	25	0
5	-25	0
6	60	-25
7	-60	25
8	36	-66
9	0	30
10	0	36

Correlation between factors ·60

Hyperplane count  
35 per cent

*Projection of pseudo-second order factors (oblimax rotation) directly on variables*

	1''	2''
1	-01	+51
2	-10	+10
3	50	-23
4	21	24
5	-43	-05
6	83	+08
7	-83	-08
8	40	-58
9	-01	+51
10	-01	+59

Correlation between factors ·00

Hyperplane count  
(accepting  $\pm \cdot 10$ )  
40 per cent

variables, gives a distinctly poorer simple structure than either of the regular projections on an immediately adjacent stratum; 3. a better simple structure of secondaries on variables can be found, but it is still not good and the position is a poor approximation to the best second order rotation on primaries; 4. pseudo-second order factors have hyperplanes directly on variables about as good as those of true second orders. But the hyperplane count is poorer than for primaries on variables or second orders on primaries. They are neither good primaries nor good secondaries.

## SUMMARY

1. Simple structure and confactor principles offer the most meaningful resolution of correlations, if factors are regarded as influences.

2. Many models are possible, both for (a) patterns of factor loadings on variables, and (b) loadings of higher order on lower order factors. The general reticular model, a network with unrestricted directions of influence, is the most generally acceptable solution to accommodate most scientific possibilities. The popular monarchic hierarchy is often a constantly recurring artefact arising from the statistical limits of any single factor analysis.

3. As there is so far no way of inferring the complex causal connections in the reticule directly from factor analytic evidence alone, discussion has been restricted to the simpler *one-way stratoplex* model.

4. The notion of strata belongs to a model, and that of orders to an operation. To find strata most efficiently, operations should begin with variables sampled evenly from a sphere of equal density. A succession of higher order factor analyses properly coordinated, suffices, however, to locate strata. Factors initially containing representatives from different levels become sorted out, and a factor lying 'between two strata' can be recognized as such.

5. Formulae can be developed to express the variance shared between two factors of different 'order' (i.e., not immediately contiguous in the reticule or strata model) and this can be viewed as the contribution of one to the variance of the other. The path coefficient is available for general reticular calculations *when relations become known*, and the Schmid-Leiman and Cattell-White formulae handle the problem in the one-way hierarchical or strata models. These latter are particularly concerned with finding the loadings of higher order factors directly on the variables. Assuming the one-way stratoplex model, formulae are developed here for the complete relations among (a) the correla-



tions between variables, (b) the scores on the variables, (c) the loadings of variables, in terms of various higher order factors, (d) the loadings of higher on lower order factors, (e) factor scores, and (f) the intercorrelations among factors.

6. The Cattell-White formula expresses loadings of higher order factors on variables in the general setting described, whereas the Schmid-Leiman expresses loadings for factors first set as orthogonal in a hierarchy. The two formulae have different statistical properties and are useful for different purposes.

7. Attention is called to misleading inferences and constructions in personality theory based on what have been called pseudo-second order factors. When only as many first order factors are extracted as one believes second order factors to exist, the resulting rotation commonly produces imitations of second order factors, inflated by overlap with primaries. These can be shown theoretically to be inadequate and unstable, and in practice fall short of the hyperplane count of true primaries. They are unresolved, space-distorted representations of factors.

8. The main purpose of this paper has been (i) to survey theoretical models and to present the advantages and the practical solutions for the stratoplex model, and (ii) to demonstrate that, at the practical level, the Cattell-White formula enables one to evaluate the percentage loss of criterion prediction through resorting to the economy of a battery measuring a few second order factors, instead of more first order factors. This loss can be appreciable and suggests that keeping to the first order battery is generally to be preferred.

### *Acknowledgement*

This investigation was supported in part by Public Health Research Grant MH 1733 from the National Institute of Mental Health, Washington D.C., U.S.A.

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#### ADDENDUM

##### Notes to Fig. 8 (p. 251)

*Note 1:* The example used here is the same as in Fig. 9, where the single steps of obtaining higher order loadings by the Cattell-White formula are shown in detail.

*Note 2:* It will be seen that squaring the row values gives the  $h^2$  (communality) directly in the Schmid-Leiman case but only after further calculation in the Cattell-White case.

*Note 3:* Strictly the Cattell-White resolution should offer *no* values for the third order factor because it cannot be rotated to simple structure, in the absence of enough hyperplane stuff. To show a complete series comparison, however, the third order factor is accepted as it comes from the centroid, unrotated, and with an arbitrary choice of correlations to give a single factor.

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