PSYCHOLOGY REVIVALS

The Structure of Human Personality

Third Edition

H.J. Eysenck

ROUTLEDGE

Psychology Revivals

The Structure of Human Personality

Originally published in 1953, this third edition was first published in 1970. It was one of the early attempts at bringing together theories of personality organisation and finding empirical evidence to test their hypotheses. This third edition includes additional chapters and updated references to current research of the time. It is a particular feature of this book that a large number of figures are reproduced in the text; this is essentially a consequence of the writer's belief that diagrammatic representations are better suited to the transmitting and remembering of information than are words or numbers.

The first chapter outlines the theories and discusses some of their implications, the second and third look at methods of analysis and projective techniques, while the rest of the book is devoted to a critical presentation of the evidence, arranged according to the technique employed – rating, self-rating, objective testing, constitutional assessment, autonomic measurement, and so on. Today it can be read and enjoyed in its historical context.

This page intentionally left blank

The Structure of Human Personality

H.J. Eysenck

Routledge
Taylor & Francis Group
LONDON AND NEW YORK

First published in 1970
by Methuen & Co. Ltd

This edition first published in 2013 by Routledge
27 Church Road, Hove, BN3 2FA

Simultaneously published in the USA and Canada
by Routledge
711 Third Avenue, New York, NY 10017

Routledge is an imprint of the Taylor & Francis Group, an informa business

A Library of Congress record exists under ISBN: 0416180302

ISBN: 978-0-415-84441-3 (hbk)
ISBN: 978-0-203-75343-9 (ebk)

THE STRUCTURE OF HUMAN PERSONALITY

H. J. EYSENCK

Professor of Psychology, University of London;
Director. Psychological Dept., Institute of Psychiatry
Psychologist, Maudsley and Bethlem Royal Hospitals

METHUEN & CO. LTD.
11 NEW FETTER LANE, LONDON, E.C.4

First published August 27, 1953
Second Edition 1960
Third Edition 1970
SBN 416 16420 X

First published as a University Paperback 1970
SBN 416 18030 2

PRINTED IN GREAT BRITAIN BY
JOHN DICKENS AND CO LTD NORTHAMPTON

Distributed in the U.S.A.
by Barnes & Noble Inc.

To

L. L. THURSTONE

In Profound Admiration

If there are some subjects on which the results obtained have finally received the unanimous assent of all who have attended to the proof, and others on which mankind have not yet been equally successful; on which the most sagacious minds have occupied themselves from the earliest date, and have never succeeded in establishing any considerable body of truths, so as to be beyond denial or doubt; it is by generalizing the methods successfully followed in the former enquiries, and adapting them to the latter, that we may hope to remove this blot on the face of science.

J. S. MILL.

CONTENTS

This page intentionally left blank

INTRODUCTION

IN SPITE OF the great interest that psychologists, psychiatrists, and psycho-analysts have shown in the subject, no detailed account appears to exist of those natural phenomena on which our views of the organization or structure of personality are based. There are, it is true, many professions of faith; thus we are assured that "personality is an emergent 'gestalt' phenomenon whose organization cannot be accounted for in terms of atomistic concepts", and that "the unique totality of personality determines the very nature and meaning of the individual sub-wholes or 'parts'; it is not determined by them". Proof of such far-reaching asseverations is hardly ever attempted; occasionally recourse is had to philosophical deduction (which is irrelevant) or analogy with perceptual phenomena (which is improper), but, by and large, reliance is placed more on intuitive agreement than on scientific demonstration. Such demonstration, including the demand for proof which it implies, is indeed often declared to be superfluous; clinical experience and phenomenological observation are believed to be able to take its place. This view is maintained, even although it would appear to make impossible any rational choice between opposing experiences and observations, by explicitly denying the very principles of scientific method on the basis of which one expert's views might be shown to be correct and another's to be faulty.

It consequently appeared worth-while to bring together in one volume some of the major theories of personality organization, and to comb the literature for empirical studies either undertaken with the aim of testing deductions from these hypotheses or at least having some direct bearing on them. The first chapter represents an attempt to set forth these theories in broad outline, and to discuss some of their implications; the remainder of the book is devoted to a critical presentation of the evidence, arranged according to the technique employed—rating, self-rating, objective testing, constitutional assessment, autonomic measurement, and so forth. Also discussed in the first chapter is the problem of a proper statistical model for the investigation of personality organization, and an attempt is made to show that at the present moment the method of factor analysis alone

enables us to represent the known facts in terms of a strictly quantitative conceptual schema.

There are three main criticisms of such a conclusion which merit at least a brief answer. (Criticisms based essentially on a misunderstanding of factor analysis, either with respect to its aims or its methods, will not be dealt with here. They are deplorably prevalent in the literature, so much so that it might be wise to follow psychoanalytic practice and lay it down that criticism of factor analysis should be confined to those who had themselves been factor analysed!)

The first point often made is a denial of the importance of the problem which factor analysis attempts to tackle. Essentially, types, traits, syndromes, and the various other concepts which psychology and psychiatry have elaborated in their attempts to describe the organization of personality, all of which are in principle similar to factors by virtue of their derivation on the basis of consistencies of behaviour similarities, are said to be artefacts which are used to imprison the unique personality, a veritable bed of Procrustes which distorts the essence of the phenomena under observation. That these concepts are artefacts is of course undeniable, as by definition all concepts are artefacts; the concepts of extraversion, hysteria, or suggestibility are artefacts in precisely the same sense that an electron, magnetism, or an ohm are artefacts. Spearman's "*g*" (general intelligence) is in the same position as Newton's "*g*" (gravitational force). Science attempts to bring order into the multiplicity of phenomena; it does so by introducing concepts which have no counterpart in the world of reality. These concepts may be useful or worthless; their value must be decided on other grounds than that of their being "artefacts"

Granted, then, that such concepts are unavoidable for the solution of our problem, is the problem itself worth-while? In all sciences we find a division between "statics" and "dynamics", between attempts to create taxonomies, classifications, or nosologies, and attempts to derive causal laws and developmental sequences. The table of the elements of chemistry, the dimensional analysis of physics, the taxonomic principles of flora and fauna in biology—these are all examples of the "static" type of analysis. To deny the importance of either type of approach is an admission of prejudice, because the history of science shows over and over again the essential dependence of these two types of approach on each other, and the fact that advances in one may lead to important advances in the other. Both are vital to the development of science, and there appears to be no reason

why psychology should be a solitary exception to this general rule. We may conclude, therefore, that the "static" approach implied in the study of *organization* has its valid place beside the "dynamic" approach implied in the study of *development*.

It is here that the second objection comes in. How, it is maintained, can we study organization without detailed knowledge of development? Physical diseases are classified in terms of their causes, i.e. by reference to development; without such knowledge of causes we may be gravely mistaken in our principles of nosology. Must not the same apply in psychiatry, and with reference to mental disorders? Biological classification proceeds by way of evolutionary concepts, i.e. again in terms of development; should psychology be an exception?

There are two answers to this point. The short answer is that while principles of organization derived from firmly established principles of development are, of course, infinitely preferable to principles of organization established in any other way, psychology cannot be said to have a choice in the matter because these firmly established principles of development are completely missing. In the absence of any knowledge of the causes of mental disorders, we must needs fall back on some other method of classification; in the absence of any knowledge of the determinants of personality development in general, we clearly cannot base our system of organization on these non-existing principles. In other words, although no one would deny the advantages of being able to base one's principles of organization on principles of development, some other method is called for at the moment in view of the conspicuous lack of the latter. (There is, of course, no lack of theories; quite on the contrary, there is such a multiplicity of theories regarding development, none of them based on firm scientific ground of proof and verification, that any choice between them becomes a matter of temperament rather than of reason.)

A somewhat longer answer may be made in terms of historical considerations. If we take botany as our example, we find that taxonomy began with the Greeks; Theophrastus may be considered to have been the first to classify plants and to describe them accurately (300 B.C.). Like more recent attempts in psychology, "the writings of the Greeks embodied too few observations on what plants are and too much philosophizing as to how they might be expected to be" but, nevertheless, their attempts at classification were an indispensable preliminary to later work which superseded theirs. There is no need to trace the development of systems of taxonomy through the work of Bauhin, Ray, Linnæus, Jussieu, Candolle, Endlicher, and others

who preceded the publication of Darwin's *Origin of Species* in 1859; the important point to note is that the principle of evolution could not have been established, or even conceived, without these previous attempts at taxonomy, and that its establishment gave a new foundation to taxonomy but did not change its general outline as much as might have been anticipated. "After the first wave of excitement was over, systematists quite generally accepted the doctrine and began to revise their systems to fit the new principle of phylogenetic relationships. Fortunately, much that had already been done was usable, for all but the morphological systems had been pretty much eliminated from botanical taxonomy, and *morphology has proved the best single criterion of phylogeny*" (Swingle, D. B., *A Textbook of Systematic Botany*, 1946).

Thus, in botany, and in zoology as well, it has proved possible to work out natural groupings on the basis of morphology, i.e. on the basis of correlational clusters of surface characteristics, which adumbrated rather closely the "true" relationships as established on the basis of developmental (phylogenetic) principles. Morphology in botany corresponds to the study of behavioural acts in psychology, and the rather non-rigorous methods used by the earlier workers to establish "natural groupings" are based on precisely the same principle as is factor analysis. It is to be hoped that in due course principles of development will be found in psychology which correspond to that of evolution in biology; it seems reasonable to expect that here also such new discoveries may lead to an improvement in, but not a complete change of, the principles of organization already developed.

The third objection, which is often raised against factorial methods, refers to the subjectivity of choice between one system of factor axes and another, and to the apparent and often acrimonious disputes between factor analysts. Where practitioners of the same methodology arrive at contradictory findings, it is often said there must be some vital flaw in the method used. Let us consider these points in some detail. There is of course a certain subjectivity implied in all dimensional work; this is true in physics as much as in psychology. As the physicist Bridgman points out, "there is nothing absolute about dimensions . . . they may be anything consistent with a set of definitions which agree with the experimental facts". To object to such subjectivity is to misunderstand altogether the nature of science and its methodology. Where there are alternative solutions to the problem posed by a set of experimental facts, these solutions are

mathematically convertible into one another, or else give rise to different deductions which can be experimentally checked. Thurstone's disproof of Spearman's original contention regarding the sufficiency of "*g*" and "*s*" as the only factors to account for the intercorrelations between cognitive tests may be quoted here as an example of such experimental check.

What, then, about the alleged disagreements? These occur in all sciences, and are often much more fundamental than those which have arisen in the brief history of factor analysis. The fundamental opposition in mathematics between Kronecker and Weierstrass; the continuing difficulties implied in reconciling the theories of light advanced by Newton and Huyghens in physics; the antagonism between Engler and Prantl on the one hand and Bessey and Hallier on the other in botany; the differences between Fisher and Pearson in statistics; the heated disputes between Glover and Klein in psycho-analysis—should these lead us to discard mathematics, statistics, physics, botany, and psycho-analysis as unworthy of our attention? Indeed, factor analysis, at the moment, appears to have reached a point in its development where there are hardly any fundamental issues on which agreement is not fairly complete; Thurstone's brilliant development of his system to embrace oblique and second-order factors has led to a reconciliation of opposing viewpoints which leaves room only for very minor disagreements.

The history of this book itself may illustrate the influence of these developments. A collection of references was undertaken first in connection with the preparation of *Dimensions of Personality*, and then again in connection with *The Scientific Study of Personality*; but in view of what appeared to be marked contradictions between results reported by different writers, no attempt was made to work these references up into some connected whole. The development of Thurstone's system in terms of second-order factors, and an opportunity for the writer to discuss some of the resulting problems with Thurstone personally, changed the somewhat pessimistic outlook, and a reassessment was begun which resulted in the present book. The reader will be able to judge for himself to what extent the results summarized may be said to give a congruent picture; to the writer himself the amount of agreement found was a revelation.

It is the writer's hope, as it presumably is that of all the students whose work is reported here, that from these studies there should arise a system of taxonomy, or classification, or nosology, which may be regarded as firmly based on biological reality, and which will find

considerable support in principles of ontogenetic development when these are established, as well as aid in its turn in the discovery of these principles. Even if these hopes should be judged ill-conceived, however, and even if the reader be unwilling to admit the writer's contention regarding the importance and value of the factorial technique, nevertheless there is here a store of facts regarding human behaviour which any theory which attempts to cover the total human personality must account for. Most psychological theories only blossom in a climate of their author's devising, and die of inanition when exposed to the sharper winds of facts gathered outside the sanctified circle; here is a set of facts on which the theoretician may try out the inclusiveness of his hypotheses. The test is a very clear and simple one: does his theory help him predict the observed correlations? Is his picture of personality organization inclusive enough to permit of cross-checking with the data here recorded? If the answer be no, then the theory is clearly segmental only and does not cover the whole of personality. Nor would it be adequate to claim that the facts recorded are unimportant or uninteresting. In science we cannot pick and choose; the theory of gravitation must account for all the facts subsumed under it, and even such minor deviations as the slight eccentricity of Mercury's motion, or the minute bending of light rays when passing the sun, were sufficient to dethrone what used to be the most strongly established theory in the history of science.

One word regarding the method of presentation adopted. It appeared useless to present to the reader a simple summary of all relevant research, uncritical and undigested. Instead, the writer has adopted a highly critical attitude, and has not hesitated to point out flaws in experimental design, method of analysis, or even in calculation which reduce the value of a given paper. These are fairly objective matters, on which agreement would be reasonably high among competent judges. It is to be feared that other judgments are less objective; while the writer has made an attempt to keep the book clear of purely personal preferences, he has no illusions regarding the success of this undertaking, and can only hope that the reader will be a charitable judge of any deviations from this ideal. It is perhaps a matter of personality, but to the writer it appears better to err in the direction of excess of critical rigour than in the opposite direction of eclectic acceptance of good and bad indiscriminately—a tendency exemplified in so many of our text-books on personality.

While only primary sources were used for the preparation of this

book, acknowledgment should be made to several authors whose writings have exerted some influence on the presentation. The most important of these are probably G. W. Allport, whose book on *Personality* has been more influential perhaps than any other in the development of this branch of study; A. A. Roback, whose *Psychology of Character* is the most learned historical introduction, as well as the most lucid; P. E. Vernon, whose *Assessment of Psychological Qualities by Verbal Methods* is a masterpiece of condensation and critical evaluation; C. Spearman, whose *Abilities of Man* has a place secure in the history of psychology; and L. L. Thurstone, whose *Multiple Factor Analysis* is the foundation-stone on which all later workers may securely build. As without his brilliant unification of apparently antagonistic elements this book could not have been written, it is most fittingly dedicated to him as a small token of indebtedness.

INSTITUTE OF PSYCHIATRY,
Maudsley Hospital.
30*th Dec.*, 1951.

This page intentionally left blank

INTRODUCTION TO THE THIRD EDITION

THE SECOND EDITION of this book, coming seven years after the first, had been changed and amplified in many ways. Two new chapters were added, dealing with methods of analysis (albeit briefly and probably inadequately) and with projective techniques. Over a hundred new references were added and for the sake of simplicity the terminal bibliography was given up and instead lists of references printed at the end of each chapter. In spite of these additions, the main argument did not seem to require any change; if anything, work done in the intervening years had strengthened the foundations on which it rested.

The third edition, appearing another ten years later, has been changed primarily by adding a chapter dealing with casual theories of personality structure, and detailing some of the hypothetico-deductive chains of evidence linking these theories with experimental and observational evidence accruing over the years; an attempt was made in particular to give preference to articles and books which had appeared quite recently. The ever-increasing flow of literature in this field has made it even more necessary than before to be selective and critical in one's choice of experiments; a simple survey of the 5,000 or more articles in my files would be of very little use to the reader who rightly demands from an author that he should organise the field for him, and present him with certain guide-rails, however tenuous, to help him in traversing it. There is of course no gainsaying the fact that in making such provision the writer is departing from the ideal (if it be a proper ideal, rather than an "idol of the market place") of complete impartiality; he inevitably introduces value judgments both into his selection of what to include and what to exclude, and into his evaluation. Such value judgments, as Polanyi among others has so strongly emphasised in his writings, are in fact of the essence of science; the Psychological Abstracts are an invaluable tool for the practicing scientist, but they do not provide a proper example for writers of scientific books. The need for personal commitment in providing structure for a field of scientific endeavour is sometimes underplayed; readers who prefer a com-

pletely impersonal and non-commital account are accordingly warned that they are likely to be disappointed.

It is a particular feature of this book that a large number of figures are reproduced in the text; this is essentially a consequence of the writer's belief that diagrammatic representations are better suited to the transmitting and remembering of information than are words or numbers. There is little precise information on this point, unfortunately, and one has to go by experience; all the same, it would be surprising if this generalisation were to be disproved. The reader must of course be his own judge, and the possibility arises that different personality types prefer different information media. In any case, there are also many Tables, and a considerable body of Text, for those who do not share my preference for visual displays.

The study of personality is becoming more and more recognised as an essential complement of the experimental investigation of any particular human function, be it learning, conditioning, memory, perception, sensory thresholds, or any other of the hallowed title-headings of psychological textbooks. As the reader will discover, phenomena are not independent of the type of person who is being studied; verbal learning, to take but one example, shows reminiscence over time in introverts, forgetting in extraverts; these contrary trends cannot be reconciled by simple averaging. To resort to averaging simply sweeps the problem under the carpet; it does nothing to solve the difficulty of bloated error terms. Personality study in an integral part of experimental psychology, simply because the subject of the experiment is a person. This ineluctable truth is finally dawning on many experimentalists who had hoped to eliminate from their experiments just that feature of their field of expertise which is in fact central to it—man. It is in the interplay between experimental psychology and personality study that I see the greatest hope for the development of both into truly scientific disciplines, and it is in the hope of furthering this desirable development that this book has been written.

ACKNOWLEDGEMENTS

The author wishes to acknowledge his indebtedness to authors and publishers who permitted the reproduction of tables and figures. A detailed list of copyright holders who agreed to such reproduction follows below:

The McGraw-Hill Book Company; Harper Brothers; Paul B. Hoeber; Cornell University Press; The World Book Company; The Harvard University Press; The Macmillan Company; University of Chicago Press; Stanford University Press. *The Journal of Personality; The Journal of Mental Science; The British Journal of Social Medicine; The Journal of Genetic Psychology; Child Development; Educational and Psychological Measurement; The American Journal of Psychiatry; Psychologische Rundschau; Journal of Psychology; Personnel Journal; The Journal of Applied Psychology; The Journal of the Royal Statistical Society; The Canadian Psychiatric Journal*, and the journals of the American Psychological Association, particularly *The Journal of Experimental Psychology* and *The Journal of Abnormal and Social Psychology.*

This page intentionally left blank

CHAPTER I

THEORIES OF PERSONALITY ORGANIZATION

"EXPERIMENT WITHOUT theory is blind; theory without experiment is lame." There is perhaps no field in psychology where this saying of Kant's applies with greater force than in the study of the structure of personality. Observers have been struck again and again by the fact that what should be a unitary field of study is cleft in two; that instead of an harmonious co-operation between theory and experiment, we have, on the one hand, an experimental school which investigates in the minutest detail processes having only the most tangential relevance to personality or to any plausible theoretical orientation, and, on the other, theoretical schools of the "dynamic" type whose theorizing proceeds without any proper basis in ascertained fact and without any consciousness of the need for verification. Most psychologists would agree that this division of labour has been carried to such extremes that it is threatening the very conception of "personality" as a legitimate field of scientific study.

Corresponding to this division into "experimentalists" and "theoreticians", there are a number of other divisions among students of personality hardly less deep and hardly less acrimoniously debated. Yet to the onlooker it often appears that while both sides are right in their positive claims, they are wrong and one-sided in their condemnation of what other schools and other points of view have to contribute. Few would seriously argue that experiment could fruitfully be carried on without theory or theory lead to important advances without the check of experimentation. Similarly, most of the other disputes which appear so formidable in cold print seem amenable to compromise when each side's arguments are carried to their logical conclusion.

As an example, we may take the very definition of the term "personality" itself. Here we find immediately an apparently irreconcilable opposition between those who lay stress on *behavioural acts* and those who lay stress instead on *dynamic concepts.* As an example of the behavioural type of definition, we may quote Watson (1930), according to whom personality is "the sum of activities that can be

discovered by actual observation over a long enough period of time to give reliable information". As an example of the dynamic type of definition we may quote Prince (1924), according to whom "personality is the sum-total of all the biological innate dispositions, impulses, tendencies, appetites, and instincts of the individual, and the acquired dispositions and tendencies".

It is obvious that the concepts which enter into one kind of definition—observable behavioural acts—play no part in the other, which deals entirely with dynamic concepts—impulses, dispositions, instincts, and the like. Yet the opposition clearly cannot be as complete as it appears. We have no direct knowledge of instincts, dispositions, and impulses; they are abstract conceptions created to unify and make intelligible the observable behavioural acts from which they are abstracted. Without these behavioural acts the concepts would have no assignable meaning: all we can know about human behaviour must ultimately derive from observations of behaviour. Yet such observation of behaviour by itself is not enough. We must have concepts which denote aspects of behaviour common to a number of situations; science cannot exist without abstractions based on common properties. Both definitions therefore are one-sided; a proper definition must stress both the empirical source of our data and the theoretical nature of our unifying concepts.

For the purposes of this book, we shall adopt the following definitions: Personality is the more or less stable and enduring organization of a person's character, temperament, intellect, and physique, which determines his unique adjustment to the environment. Character denotes a person's more or less stable and enduring system of conative behaviour ("will"); Temperament, his more or less stable and enduring system of affective behaviour ("emotion"); Intellect, his more or less stable and enduring system of cognitive behaviour ("intelligence"); Physique, his more or less stable and enduring system of bodily configuration and neuro-endocrine endowment. It will be noted that this definition, which owes a great deal to Roback (1927), Allport (1937), and McKinnon (1944), stresses very much the concept of *system, structure*, or *organization*; in this it goes counter to the doctrine of *specificity of behaviour*, which held almost complete sway in American research from the early nineteen-twenties until quite recently. A few words may therefore be said regarding this issue of specificity versus generality, particularly as from one point of view all the experimental work reviewed in this book is intimately related to this problem.

Common-sense psychology unhesitatingly describes and explains behaviour in terms of traits, such as persistence, suggestibility, courage, punctuality, absent-mindedness, stage-struckness, "being one for the girls", stuck-upness, and queerness, or posits the existence of types, such as the dandy, the intellectual, the quiet, the sporty, or the sociable type. For the greater part, orthodox psychology has taken over these concepts, and has presented us with traits such as ascendance-submission, perseveration, security-insecurity, and with types such as extraversion-introversion, schizothymia-cyclothymia, or Spranger's *Lebenstypen.* This easy acceptance of these concepts has been challenged, however, by a number of critics, who hold that "there are no broad, general traits of personality, no general and consistent forms of conduct which, if they existed, would make for consistency of behaviour and stability of personality, but only independent and specific stimulus-response bonds or habits".

This theory of specificity has its roots deep in the experimental tradition, and its *à priori* improbability should not prevent us from glancing at the main sources from which it draws its strength. The first of these sources is the Thorndikian type of learning theory prevalent around the first decades of this century. Learning is conceived in terms of S-R (stimulus-response) bonds after the manner of the reflex or the conditioned reflex, and these bonds are, of course, conceived to be entirely specific. If the organization of personality is largely a matter of learning—and here the great majority of writers have favoured an anti-hereditarian view, without however basing themselves on any convincing experimental evidence—then the specificity of the learning process should be mirrored in the final product of learning, i.e. the adult personality. And while S-R theories in the field of learning have been challenged by S-S (sign-significate) theories which maintain that learning is part of a larger problem of organization, particularly perceptual organization, these non-specific theories came into the field more recently, have been somewhat less influential historically, and have not carried over into the field of personality description to the same extent as the specificity theories.[1]

A second source, not unrelated to the first, has been the vast volume of work done on the problem of "transfer of training". It used to be assumed that certain specific acts (learning verses by

[1] For a review of experimental studies of these theoretical issues, see Hilgard (1948), Hilgard and Marquis (1940), and the appropriate chapters in S. S. Stevens (1951).

heart, or doing problems in arithmetic, or writing out French irregular verbs) would in the course of time lead to improvement in general abilities or faculties (memory, will-power, logical ability, and so on). James and Thorndike showed in a number of investigations that this easy assumption had little empirical foundation. When two groups of subjects were equated for their ability in a given task, such as learning poetry by heart, for instance, and one group subjected to a period of training in memorizing material which might even be closely similar to that on which they had been tested, while the other group was not given any training, then the predicted superiority of the former group over the latter on a repetition of the original task was not observed. Learning, apparently, is relatively specific: there is no general effect on the hypothetical faculties which such training was supposed to improve. Any transfer effects which might be observed were considered to be due, not to the action of broad mental "faculties", but to the fact that the original and the practised activities had certain elements in common. This theory is known as the "theory of identical elements"; in Thorndike's (1903) own words, "a change in one function alters any other only in so far as the two functions have as factors common elements. . . . To take a concrete example, improvement in addition will alter one's ability in multiplication because addition is absolutely identical with a part of multiplication, and because certain other processes—e.g. eye movements and the inhibition of all save arithmetical impulses—are in part common to the two functions." Development of personality, no less than of linguistic or numerical skills, is therefore seen as specific training of individual association, never as generalized improvement of larger mental units or "faculties".[1]

A third source of the specificity theory of personality organization, equally influential as the other two, has been the direct experimental attack on the problem by Hartshorne and May (1928, 1929, 1930). These writers carried out a large-scale project, described in some detail on a later page, in which many hundreds of children were given the opportunity to behave in a dishonest, deceitful manner under conditions which apparently made discovery impossible, but which in reality were completely under experimental control. Other types of behaviour (persistent, moral, charitable, impulsive, and self-controlled behaviour, for instance) were also investigated by means of ingenious and largely novel techniques. The statistical treatment

[1] A recent review of the voluminous literature on "transfer of training" is given by Gagné, Foster and Crowley (1948).

of the data was beyond cavil, and in view of the brilliance of the design and the technical excellence of the execution, this study has rightly been regarded as crucial in respect to the theory of specificity. When therefore Hartshorne and May found very low intercorrelations between their tests, and discovered that children who were honest, or persistent, or co-operative, or charitable in one test-situation were not always honest, or persistent, or co-operative, or charitable in another, their conclusion that these alleged qualities were "groups of specific habits rather than general traits" was very widely accepted as finally settling the issue in favour of the theory of specificity.

This powerful and imposing theoretical structure was subject to a variety of damaging criticisms, however, and none of the three sources on which it bases itself has remained unscathed. We have already mentioned that S-R theories were opposed by writers whose outlook was formed or at least influenced by Gestalt notions; Köhler, Koffka, Tolman, Adams, Zener, and others have developed theories which account for the observed facts without invoking the specific connections posited by the followers of Thorndike, and, indeed, Thorndike himself has admitted concepts into his system which are incompatible with a completely specifist point of view. There is no sign of any decision in this battle of learning theories, but it is already clear that if one's theory of personality organization must be determined by one's learning theory, then there is still freedom of choice between a "specific" and a "general" type of learning theory. It would seem to follow that a direct attack on the problem of specificity in the field of personality itself would be more promising than a somewhat lengthy wait for a decision in the field of learning theory.

Much the same must be said about the conclusion to be drawn from investigations into the problem of "transfer of training" and of "identical elements". Allport's (1937) brilliant criticism of the specifist contention is probably too well known to need repetition. By showing that the very notion of an "element" is completely ambiguous in the writings of those who support the Thorndikian view, and that the alleged "identity" of these elements is merely an *a posteriori* justification of the observed phenomena, without any value in predicting and without any possibility of verification, he has succeeded in throwing great doubt on the tenability of this whole view. When his criticisms are seen in the light of experimental work, which fails to show the theoretically predicted correspondence between improvement after practice, and the similarity between origi-

nal task and practised task, we can only conclude that regardless of the eventual outcome of the argument regarding "transfer of training" and the theory of identical elements, our decision with regard to the question of specificity in the field of personality must rest on direct evidence from that field, rather than in deductions from principles of such uncertain validity.

We are thus led to a re-examination of the results of the Hartshorne-May study. While the detailed results are presented in a later chapter, we may here note certain doubts regarding the interpretation of their perfectly valid results made by these two authors. Let us examine first of all their finding that a child who behaves in a dishonest manner in one situation does not necessarily behave in a dishonest manner in another situation; their conclusion is that honesty is not a general trait but specific to the two situations. But this would assume that the two situations made equal demands on the hypothetical honesty of the child, a view for which there is no evidence at all. A child may fail a difficult item in an intelligence test and pass an easy one; because he passes one and fails on another, we do not argue that he is not behaving in a consistent manner! A child may tell what he considers a white lie, but balk at cheating; or he may cheat, but balk at stealing. To imagine that an advocate of the view that a general trait of honesty existed would necessarily deny the existence of degrees of temptation, or of degrees of immorality as between one act and another, is quite unrealistic, and there is no such implication in the "generality" theory. Related to the first point is a second, made by Hartshorne and May, and by many other writers since, namely that while some children do show the postulated trait, i.e. are always honest or persistent, and while others are consistent in never showing it, i.e. being always dishonest or lacking in persistence, the majority sometimes show the trait and sometimes not. Thus the trait is supposedly applicable only to a few cases, i.e. those who demonstrate it consistently, and not to others. By a similar argument it might be maintained that the concept of intelligence is applicable only to those who never fail an item or to those who fail every item! If we conceive of honesty as constituting a continuum, then the most honest should indeed never cheat and the least honest always; intermediate grades of honesty should be reflected in action by cheating when temptation is strong or when the immorality involved is rather slight, and by not cheating when temptation is weak or the immorality involved strong. For a given degree of temptation and immorality of the act, we would then be able to predict with as

much accuracy for the intermediate child as for the extreme, just as we can predict for the child of average intelligence as easily as for the genius or the dunce whether he will succeed or fail with any given problem.

As a third argument, Hartshorne and May advance the view that the very low intercorrelations between the different tests for each one of the various personality qualities measured—honesty, persistence, self-control, and so on—make the assumption of the existence of such qualities very unlikely. Yet on the specificity theory these correlations should be zero; in actual fact they are almost in every case positive. Thus it is reported that "the twenty-three tests used in securing our total character score, for example, intercorrelate + ·30 on the average". Such intercorrelations are admittedly lower than those found between intelligence tests, but we must be careful not to compare an intelligence test, composed of fifty to a hundred items, with a single test of honesty, or persistence, which in truth would correspond rather to an item in a much larger test battery for the measurement of honesty, or persistence, made up of fifty or a hundred such items. We shall see, in our discussion of the detailed results of this experiment, that reliabilities and validities approaching and sometimes even exceeding values of ·85 and ·90 are found in Hartshorne and May's own work for such batteries of "honesty" or "persistence" tests. Such results are inconceivable on any strict specifistic hypothesis, and must therefore be held to controvert that position.

In the fourth place, we must take into account the fact that Hartshorne and May used social and ethical concepts as the qualities whose specificity or generality was to be investigated. Now, even if the chosen qualities had been shown to be entirely specific, it would not follow that because certain socio-ethical qualities lacked generality, therefore more genuinely psychological qualities would also be found to be specific; as Watson (1933) points out, the experiment may beg the question by selecting the wrong type of quality to investigate. We may find consistency in the habits of frequenters of library by observing whether they choose books from the fiction, science, history, or poetry racks; our failure to observe such consistency when we direct our attention to the colour of the binding of the books selected does not prove the specificity of the choices!

In the fifth place, the preceding argument appears to apply with particular strength when children constitute the experimental population, as they did in these studies. Socio-ethical concepts are clearly

not innate; they are acquired through social learning. The young child has only had insufficient time to integrate the teaching he has received from a variety of sources into some kind of general *set*, some standard which he or she can apply to a variety of different situations; integration should hypothetically be incomplete in the young child and progress as the child advances in age. Such is indeed the fact, as demonstrated in Hartshorne and May's own data, and McKinnon's (1933) later work with adult subjects. This latter writer found considerable consistency in the honest and dishonest behaviour of his subjects, and even succeeded in predicting their reactions to the test on the basis of a five minutes' interview. We may therefore with some confidence assert that in part at least the lowness of the correlations found by Hartshorne and May was due to the youth of their subjects; if the investigations were to be repeated with older subjects, higher coefficients could confidently be expected.

It may be asked whether Hartshorne and May were not aware of some at least of these criticisms. The answer must be that they were, and that they recognized the difficulties raised for their hypothesis of specificity by the observed correlations, low as they were. Their reply was not to deny the observed correlations, but to explain them in terms of "identical elements". As we have seen, they speak of "groups of specific habits rather than general traits". But in doing so they have given up what is most significant about the theory they hold, namely the complete specificity of conduct, and have admitted at least the partial generality of behaviour. Their explanation, it is true, is in terms of specific S-R bonds, but, as we have seen above, little faith can at the moment be placed in any explanation in terms of learning theory, in view of the lack of agreement between different investigators. We are left, therefore, with a very clear admission of the existence of generality in behaviour. A child who is honest, or persistent, or co-operative in one situation does tend to be honest, or persistent, or co-operative in another situation, although prediction is very far from perfect. Our task, then, must be to enquire into the *degree* of generality manifested in human conduct, and to construct a theoretical model which will faithfully represent the facts in so far as they have been established by experimental enquiry. In doing so we will do well to bear in mind that although Hartshorne and May have failed to show that human conduct is completely specific, they have shown conclusively that it is far less general than we tend to imagine, and far more strongly determined by the specific situation in which it occurs than used to be thought at one time. There is truth

in the contentions of the adherent of the theory of specificity, as well as in those of the adherent of the theory of generality; the problem ceases to be a theoretical one, and becomes instead quantitative and empirical.

In looking for a model for our description of personality organization, we find two claimants in the field, two concepts which have for a long time been used by those who have theorized about the mechanics of consistent and congruent behaviour—the concepts, namely, of "trait" and "type". The former of these has found a particularly warm champion in Stern (1921), who writes: "We have the right and the obligation to develop a concept of trait as a definitive doctrine; for in all activity of the person, there is besides a variable portion, likewise a constant purposive portion, and this latter we isolate in the concept of trait." And how are these traits to be discovered? According to Allport (1937), who has done much to popularize this concept in the Anglo-Saxon countries, "traits . . . are discovered not by deductive reasoning, not by fiat, not by naming, and are themselves never directly observed. They are discovered in the individual life—the only place where they can be discovered—only through an inference (or interpretation) made necessary by the demonstrable consistency of the separate observable acts of behaviour". And again: "Traits are not directly observable; they are inferred (as any kind of determining tendency is inferred). Without such an inference the stability and consistency of personal behaviour could not possibly be explained. Any specific action is a product of innumerable determinants, not only of traits but of momentary pressures and specialized influences. But it is the repeated occurrence of actions having the *same significance* (equivalence of response) following upon a definable range of stimuli having the same personal significance (equivalence of stimuli) that makes necessary the postulation of traits as states of Being. Traits are not at all times active, but they are persistent even when latent, and are distinguished by low thresholds of arousal."

It will be clear from these quotations that the notion of *trait* is intimately connected with the notion of *correlation*. Stability, consistency, repeated occurrence of actions—all these terms, when translated into more rigorous and operationally definable language, refer to co-variation of a number of behavioural acts. Such co-variation, as we shall see, may refer to correlations between tests, correlations between persons, or even to correlations between different occasions of measurement within the same person. A trait may be defined as a

co-variant set of behavioural acts; it appears thus as an organizing principle which is deduced from the observed generality of human behaviour.

The concept of *type* has fared very badly at the hands of psychologists in the Anglo-Saxon countries; they mostly seem to share Stagner's (1948) belief that "the shift from type to trait conceptions

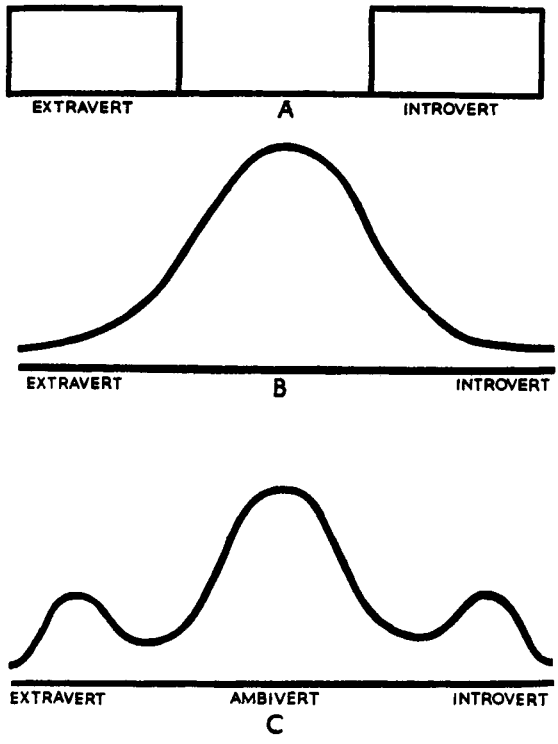

Fig. 1.—Three Conceptions of the Type Theory.

has generally paralleled the progress of psychology as science". As the same writer points out: "There are at least three different conceptions of psychological types, as they appear in the writings of various authors. These have been diagrammed in Fig. 1. Some writers still seem to think of types as pigeon-holes, mutually exclusive classifications with clear dividing lines, into which people can be segregated (Fig. 1, A). Others use the type concept as more or less equivalent to a trait, contrasting types defining the end of a continuum between which people are distributed according to the

normal curve (Fig. 1, B). A third usage proposes that true types differ from traits, in that the distribution is multimodal, with people clustering at certain points which approximate a pure type (Fig. 1, C)."

Stagner's discussion leaves out of account two points which may be important in coming to a conclusion regarding the value of the "type" concept as a model for personality organization. In the first place, as will be seen from Fig. 1, B and C, these two different concepts of type are related to the hypothetical distribution of the population with respect to the alleged type, i.e. either unimodal or multimodal. But no knowledge of the form of distribution of any kind of mental quality is possible without the prior determination of a scientifically meaningful *metric*. We can plot raw scores on tests or questionnaires, but not even the veriest tyro in statistical analysis will assume that such scores give us any knowledge regarding the distribution of the hypothetical underlying trait—particularly when, as is usually the case, our measuring instrument measures more error than true variance! Until a proper metric is proposed, no argument from the form of distribution can be regarded as relevant; as no such metric has hitherto even been suggested in the field of personality measurement, it does not appear possible to argue the merits of the "type" concept on this basis.

In the second place, the distinction drawn between continuous and discontinuous distributions (Fig. 1, A, as opposed to B and C) is perfectly valid, but it does not in any way reflect the theories and hypotheses of those whose concepts have been most influential in creating modern typology, namely Jung and Kretschmer. They do reflect a widespread misconception of the views held by these writers, and by many others who have worked in the same tradition. These misconceptions have become so widely accepted that a brief outline of the correct position is imperative.

If we consider Jung's (1921) position first, we note that in his view "every individual possesses both the mechanism of introversion and that of extraversion, and it is only the relative strength of the one as compared with the other which creates the type. . . . A rhythmic alternation of these two psychic functions characterizes the normal course of life. . . . External circumstances and inner dispositions frequently favour one mechanism and impede or restrict the other. This quite naturally leads to the dominance of one of the mechanisms. If this dominance should, for whatever reason, become chronic, then we would be faced with a *type*, i.e. the habitual dominance of one

mechanism. . . . Type never denotes more than the relative dominance of the one mechanism. . . . It follows that there can never be a pure type in the sense that the one mechanism is completely dominant to the exclusion of the other." These quotations could be multiplied many times, but they will suffice to show that Jung was very far from conceiving of all human beings as being either extraverted or introverted. Rather, he considered that most of them were characterized by a balance between the extravertive and the introvertive mechanisms; a relatively small number he considered to be unbalanced and characterized by the more or less marked dominance of one function or the other. Nothing is farther from his thoughts than the hypothesis of discontinuity; stress is laid again and again on the notion of complete continuity and balance. Admittedly his description is in terms of ideal types, i.e. of completely introverted or extraverted individuals, but he emphasises repeatedly that these are abstractions, in the same sense that Newton's laws of motion are idealized abstractions, not to be found in actual experiment.

What, then, is at the basis of his concept of type? We may answer this question by quoting a passage from Kretschmer (1948), who seems to hold a view of typology similar to that of Jung, and who has discussed this concept with admirable lucidity. According to him, "the concept of type is the most important fundamental concept of all biology. Nature . . . does not work with sharp contrasts and precise definitions, which derive from our own thought and our own need for comprehension. In nature, fluid transitions are the rule, but it would not be true to say that, in this infinite sea of fluid empirical forms, nothing clear and objective could be seen; quite on the contrary. In certain fields, groupings arise which we encounter again and again; when we study them objectively, we realize that we are dealing here with focal-points of frequently occurring groups of characteristics, concentrations of correlated traits. . . . What is essential in biology, as in clinical medicine, is not a single correlation but groups of correlations; only those lead to the innermost connections. It is daily experience in the field of typology, which can be deduced quite easily from the general theory, that in dealing with groups of characteristics one obtains higher correlations than with single characteristics. . . . What we call, mathematically, focal-points of statistical correlations, we call, in more descriptive prose, constitutional types. . . . A true type can be recognized by the fact that it leads to ever more connections of biological importance. Where there are many and ever-new correlations with fundamental bio-

logical factors . . . we are dealing with focal-points of the greatest importance."

A type is defined, then, as a group of correlated traits, just as a trait was defined as a group of correlated behavioural acts or action tendencies. According to this view, then, the difference between the concepts of *trait* and *type* lies not in the continuity or lack of continuity of the hypothesized variable, nor in its form of distribution, but in the greater inclusiveness of the type concept. The relationship between the two concepts is presented diagrammaticaly in Fig. 2,

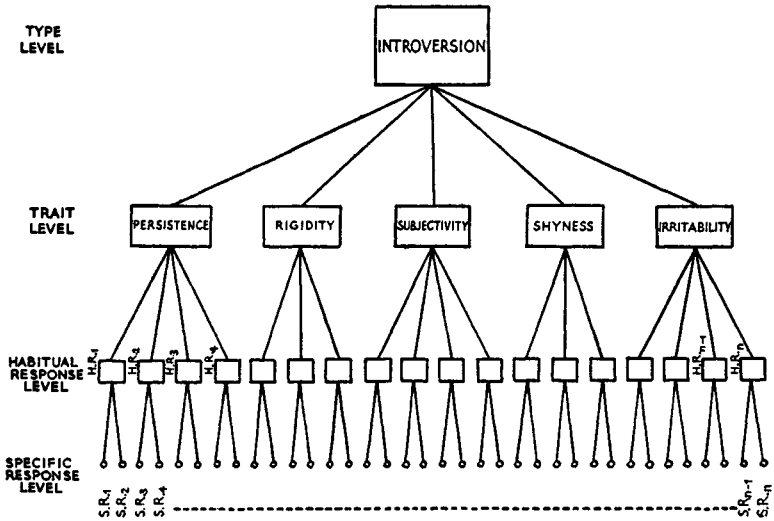

Fig. 2.—Diagrammatic Representation of Hierarchical Organization of Personality.

taken, as is the explanatory text, from Eysenck (1947). "We are dealing with four levels of behaviour organization. At the lowest level, we have specific responses, S.R.$_1$, S.R.$_2$, S.R.$_3$, . . . S.R.$_n$. These are acts, such as responses to an experimental test or to experiences of everyday life, which are observed once, and may or may not be characteristic of the individual. At the second level, we have what are called habitual responses, H.R.$_1$, H.R.$_2$, H.R.$_3$, . . . H.R.$_n$. These are specific responses which tend to recur under similar circumstances; i.e. if the test is repeated, a similar response is given, or if the life-situation recurs, the individual reacts in a similar fashion. This is the lowest level of organization; roughly speaking, the amount of organization present here can be measured in terms of reliability

coefficients, i.e. in terms of the probability that on repetition of a situation behaviour will be consistent.

"At the third level, we have organizations of habitual acts into traits T_1, T_2, T_3, . . . T_n. These traits—irritability, persistence, rigidity, etc. —are theoretical constructs, based on observed intercorrelations of a number of different habitual responses; in the language of the factor analyst, they may be conceived of as group factors.

"At the fourth level, we have organization of traits into a general type; in our example, the *introvert*. This organization also is based on observed correlations, this time on correlations between the various traits which between them make up the concept of the type under discussion. Thus in our example, persistence, rigidity, subjectivity, shyness, irritability, and various other traits would form a constellation of traits intercorrelating among themselves, thus giving rise to a higher-order construct, the type."

Type and trait are thus both defined in terms of a pattern of intercorrelations; the question of continuity or distribution is irrelevant to the distinction between them, which is merely in terms of inclusiveness. It should be noted that this model of personality organization derives directly from the writings of psychologists like Jung, Kretschmer, and Allport, none of whom can be said to be orientated very positively towards psychometric techniques in general or factor analysis in particular. Nevertheless, this hypothetical model of personality deduced from clinical experience and acute psychological insight fits in almost completely with the statistical model elaborated by factor analysts. Factorial theory distinguishes four types of factor: error factors, which are present only on one occasion, but not on others; specific factors, which are peculiar to a single test or trait whenever it occurs; group or primary factors, common to certain of the tests or traits, but absent in others; and general or second-order factors, common to all the tests or traits used in an investigation. "It will be noted that the four levels of personality organization correspond closely to the four types of factor. . . . An 'habitual response' is merely a 'specific response' divested of its error component and made into a specific factor; a 'trait' is a system of 'specific responses' divested of its error and specific variance; a 'type' is a system of 'specific responses' which has lost its error, specific, and group-factor variance." (Eysenck, 1947.)

The remainder of this chapter will deal with the outstanding type theories suggested in the literature; the remainder of the book will deal with empirical attempts to verify theories regarding the existence

and interrelationship of traits, and of the hypothesized types. But before dealing with these attempts to apply the methods of factor analysis to psychological theories of personality organization, it may be worth while to draw attention to a feature of the scheme illustrated in Fig. 2 which has caused a great deal of difficulty to many psychologists in their endeavours to follow the arguments presented by factor analysts. This difficulty results from the fact that we have two main alternative ways of approaching our problem, namely the ways identified respectively with the names of Spearman and Thurstone, and that superficially their two methods sometimes appear to give different or even contradictory results. This apparent contradiction has been most apparent in the field of intellectual ability, where the debate regarding the existence of Spearman's "*g*" has attracted a good deal of attention; it is equally noticeable, however, in the non-cognitive areas of personality research.

Let us assume that our main interest lies in the type-factor of introversion, and that we use Spearman's technique in order to test our hypothesis. We would choose one test of persistence, one test of rigidity, one test of subjectivity, one test of shyness, and one of irritability; we would then correlate these tests over our experimental population and discover the existence of a general factor ("introversion") which satisfied our statistical criteria, such as the vanishing of the tetrad differences. We might also be led to the (erroneous) conclusion that group factors, or traits, did not exist because they did not emerge in our analysis. If group factors did appear, we might be tempted to discount their existence on the plea that the tests which gave rise to them were "too similar" and consequently that these group factors were really only "overlapping specifics".

On the other hand, let us assume that our main interest was in the traits, or group factors, and that we used Thurstone's technique in order to test our hypotheses regarding these factors. We would choose a number of tests of persistence, of rigidity, of subjectivity, and so on, intercorrelate them over our experimental population and discover the existence of a number of group factors, identified in in terms of our traits, which satisfied our statistical criteria, such as simple structure. By forcing these factors to remain orthogonal and uncorrelated, we would make it impossible to discover from our analysis the existence of any higher-order concepts, such as introversion, based on these proscribed inter-trait correlations. Thus the results reached by adherents of one school might be entirely different from those of the other, and apparently quite irreconcilable.

Recent years have brought a solution of this conflict. It became clearer and clearer that Spearman-type hypotheses could no longer be maintained, either in the cognitive or in the orectic field, because the statistical criteria were hardly ever satisfied when the number of cases studied was large enough to make sampling errors relatively unimportant. Similarly, it became clear that Thurstone-type hypotheses could no longer be maintained because the demands of simple structure and those of orthogonality of factor structure were found to be irreconcilable. As is well known, Thurstone finally achieved a solution which enabled a reconciliation to be effected in terms of oblique factors and second-order factors. Such a solution still emphasizes interest in the trait-level type of hypotheses, and extracts primary factors from the matrix of intercorrelations first of all, to go on to the investigation of type-level hypotheses only as a second step, by extracting second-order factors from the intercorrelations of the primary factors. Followers of Spearman still retain a prime interest in "general factors", as they call Thurstone's second-order factors, and extract these first of all; they no longer deny, however, the existence of "group factors", as they call Thurstone's primary factors, and proceed to deal with them after the extraction of the general factor. It is debatable whether one procedure is preferable to the other (the present writer has a distinct preference for Thurstone's method), but it can hardly be doubted that the differences which have remained are of detail only, and in no way preclude complete agreement on the general outline of the model offered.

This agreement on matters so fundamental is of course welcomed by all those who felt somewhat disturbed by what seemed to be the eternal differences of opinion between different schools of factor analysis. However, in a survey of the experimental literature such as the present, we shall be dealing with many articles and books written before reconciliation was effected, and, indeed, the great majority of our sources will be seen to fall into that period. This has necessitated a somewhat detailed discussion of several researches, as well as the reinterpretation of others. On the whole, however, the writer was surprised and delighted to see what large measure of agreement there was between writers who saw themselves as protagonists of opposed doctrines, and who in their more argumentative moments would have left their opponents hardly an intellectual shred to cover their nakedness. Ultimately the facts asserted themselves, and demanded that factors at all levels of complexity of organization be taken into account. Our discussion will throughout proceed on the

basis of such recognition of the hierarchical structure of personality, and will deal very fully with the experimental evidence in favour of this position.

So far we have discussed almost exclusively the *formal* properties of the hypotheses which are going to be discussed in these pages. We must now turn, at least briefly, to their *content*. In doing so, we shall not attempt to duplicate the admirable historical account of theories of personality and character given by Roback (1927), nor shall we follow Jung (1921) in tracing these concepts through the writings of poets and philosophers. We shall, instead, restrict ourselves to those theories which have influenced and in large measure determined the empirical studies described in the following chapter; without some brief acquaintance at least with the theories of Jordan, Gross, Heymans, Wiersma, Jung, Spearman, and Kretschmer no adequate understanding is possible of the factorial studies set up to test these theories.

Of mainly historical interest, but nevertheless still very instructive, is the four-type theory popularized by Galen, Kant and Wundt. According to this theory there are four temperaments classified according to reaction type, as shown below:

	Strong	Weak
Quick	Choleric	Sanguinic
Slow	Melancholic	Phlegmatic

These types were conceived of as being quite separate; as Kant says in his *Anthropologie*: "There are four and only four simple temperaments." However, we can redraw this scheme as in Fig. 3, and it will be obvious from simple inspection that we can shift our attention from the four *quadrants* which constitute the four types of these earlier authors to the two *co-ordinates* or dimensions, i.e. the quick/slow and the strong/weak continua. If we do that we immediately shift to a much more modern conception implying not four completely separate types but rather two orthogonal (independent) dimensions which are continuous and along which the position of any particular person can be plotted. Thus a person with strong emotional reactions could also be quick, in which case he would be called "choleric" by Galen; he could be slow, in which case he would be called melancholic; but he could also be intermediate in the speed of his reactions, in which case none of the four type concepts

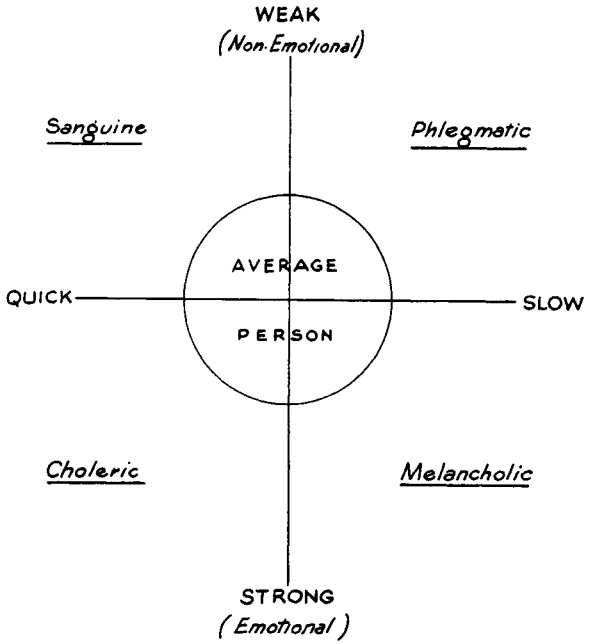

Fig. 3.—The Galen-Kant-Wundt system of personality description, illustrating both the *categorical* form (four discrete types in four quadrants) and the *continuous* or *dimensional* form (quick-slow and weak-strong dimensions).

would fit him. Similarly, a person who was quick might also have strong emotional reactions, weak emotional reactions or emotional reactions intermediate in strength. Indeed, it would appear quite likely that the majority of people would be intermediate on both continua, thus being placed somewhere near the point of intersection of our two dimensions; only relatively few would be high or low on two dimensions. We would expect therefore that not very many people would be easily characterized as cholerics, melancholics, sanguines, or phlegmatics, and, of course, this is precisely what we do in fact find.

This transfer of interest from the quadrants to the coordinates, or from a categorical to a continuous typology, or from qualitative to quantitative measurements, has been a lengthy and drawn-out affair and even nowadays remnants of the old view still linger, particularly in psychiatry, where diagnosis and classification are still categorical. Nevertheless, the evidence in favour of the modern treatment is overwhelming as we shall see throughout the remainder of this book. Granted the admissability of this transformation, it will be seen that modern experimental work gives some support to

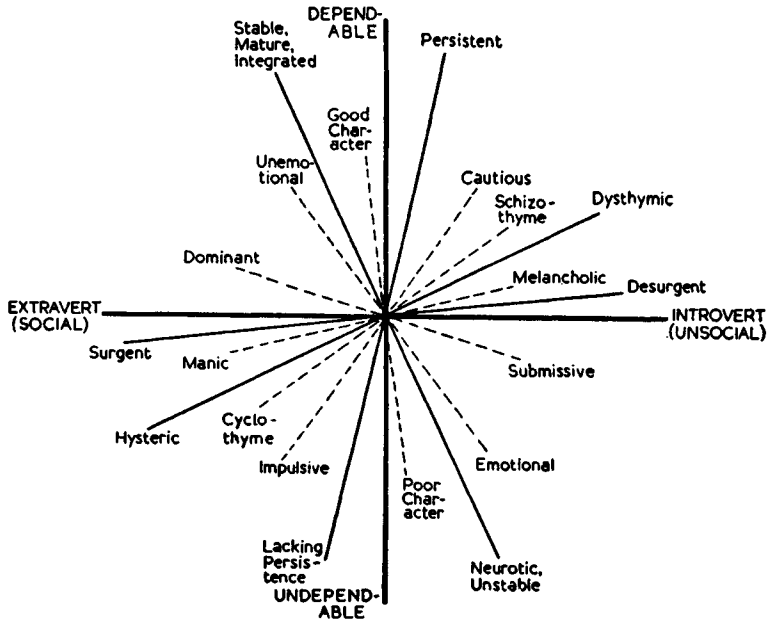

Fig. 4.—Diagram of relations between main personality dimensions according to Vernon (1953).

such a claim as that presented above. Fig. 4, which is taken from Vernon's review of modern personality theories (1953), shows a picture not essentially unlike that given in Fig. 3. If we can identify extravert and introvert respectively, with the quick and slow reactors of Galen, and the neurotic, unstable, undependable type of person with the strong, emotional reactor, as opposed to the stable, dependable kind of person, who would be the weak, unemotional reactor, then we might claim a certain continuity from the early Greek theories of temperament to the most modern. Such an identification is probably only warranted to a very limited degree; it is easy to read into historical writings what one wishes to see, and particularly to interpret ancient terms in line with modern connotations. Nevertheless, there do appear to be certain similarities providing a continuity between these early speculators and the more modern work summarized in this book.

Jordan (1890) may perhaps be considered with some justification the first of the modern theorists. He posits two antithetical types, without of course losing sight of the fact that intermediate gradations

also exist. "There are numberless varieties of character . . . many divisions, conspicuous types, intervening gradations, equal or unequal developments, varying combinations. In domestic and social life, intermediate characters produce perhaps the most useful and the happiest results, but the progress of the world at large is mainly due to the combined efforts of the supremely impassioned and reflective, and the supremely active and unimpassioned temperaments." The main principle of division, then, according to Jordan, is one which opposes the *reflective* to the *active* type of person; he goes on to point out that the reflective type tends to be more emotional ("impassioned") than the active type. Jung identifies the "less impassioned and more active type" with his extravert, and the "more impassioned and less active type" with his introvert, although he is not altogether happy with many details of Jordan's descriptive hypothesis.[1]

A brief quotation may give the reader an idea of the flavour of Jordan's writing. "In the matter of character men and women may be put into three classes. One class (frequently called in these pages, because of its leading characteristics, the active and unimpassioned class) includes those who tend to be more or less ready, or even, in some instances, restless, busy, and quick; who tend, in their extreme varieties, wittingly or unwittingly, to imitation, affectation, and love of notice; who may also be fitful, or uncertain in mood, manner, greeting, and conduct; and who, while self-conscious, self-asserting, and self-approving, are given, so far as others are concerned, to discontent, disparagement, and candid criticism or censorious comment. . . . The men and women of this class, in addition it may be to other high qualities, have, not rarely, generous sympathies, emotions, and affections. These sympathies, emotions, affections are not usually deep, but when they are associated with high mental gifts and are helped out by strong reasoning powers, the resulting character is often altogether admirable. Sometimes the emotions and affections appear to be almost, if not entirely, absent; and if, at the same time, the mental gifts are but poor, the resulting

[1] The implied opposition between visceral and motor outlet has been verified by Jones (1930), who showed that even in infants disturbing stimuli produced *either* striped-muscle behaviour *or* visceral (autonomic) responses, the one tending to preclude the other. MacFarlane (1939) has drawn attention to a similar opposition between "internalizing" and "externalizing" children. Cf. also Himmelweit's (1952) analysis of Ackerson's data on p. 164, and Freeman and Pathman (1942). Jordan's observation cannot therefore be dismissed too easily as "armchair theorizing".

character is not pleasing. The men and women of quite another class (called here, from its leading characteristic, the more reflective and more impassioned class) are those who tend to repose, tranquillity, gentleness, and who, under a placid demeanour, possess deep—if sometimes sleeping—sympathies, affections, and passions. These passions are sometimes worthy, and sometimes marked by turbulence or indolence, or sensuality, or moroseness, or cruelty." Jordan devotes a whole chapter to the "Bodily Characteristics of Temperament", pointing out that, in addition to many other features, "the more impassioned women and men also have, on the whole, a greater tendency to be lean". This tendency for introversion and leptomorphic body-build to go together we shall encounter again in several experimental studies summarized later on.

Far more influential than Jordan, whose work has been almost completely neglected until Jung devoted a whole chapter of his *Psychologische Typen* to it, was the Austrian psychiatrist Otto Gross, whose two books on *Die Zerebrale Sekundärfunktion* (1902) and *Über psychopathologische Minderwertigkeiten* (1909) introduced the concepts of "primary" and "secondary" function. These concepts are basically physiological, and refer respectively to the activity of the brain cells during the production of any form of mental content, and to the hypothetical perseveration of the nervous processes involved in this production. Thus a nervous process which succeeded in arousing an idea in the mind was supposed to perseverate, although not at a conscious level, and to determine the subsequent associations formed by the mind. Gross also postulated a correlation between the intensity of any experience and the tendency for that experience to persist secondarily and to determine the subsequent course of mental associations. Most intense and energy consuming, in his view, were highly affective and emotional experiences and ideas, and these would therefore be followed by a long secondary function, during which the mental content would still be influenced and in part determined by the perseverative effects of the primary function. (There is an obvious similarity between the concept of "secondary function" and that of "refractory period".)

According to the liability of a person to develop strong emotions, Gross then distinguishes two types—the deep-narrow and the shallow-broad. In the deep-narrow type we find characteristically a primary function which is highly charged with emotion and loaded with affect, involving the expenditure of great nervous energy, and requiring a lengthened period of restitution during which the ideas

involved in the primary function go on reverberating and perseverating (long secondary function). In the shallow-broad type, on the other hand, a much less intense primary function, necessitating the expenditure of comparatively little energy, is followed by a short period of restitution (short secondary function).

Certain personality characteristics follow from the type hypothesis briefly described above. In the broad-shallow person, the short secondary function enables a much greater frequency of primary functions to take place within a given time; this constant readiness for brief actions and reactions suggests a certain superficiality, a distractibility, as well as a prompt reaction to external events (Jordan's "activity"). In the deep-narrow person, the long perseverative secondary function makes the integration of different sets of what Gross calls "themas" (sets of emotions, associations, determining tendencies, complexes, and sentiments centred around one idea which is the object of a primary function) more difficult, and leads to a sejunctive (dissociated) type of personality. (This concept of dissociation will be taken up in more detail in connection with Kretschmer's account.) Dissociation leads to a damming up of the available libido, to inhibition, and on the behavioural level to absorption in thought and social shyness.

Jung readily identifies the broad-shallow type with the extravert, the deep-narrow type with the introvert; his main difference from Gross lies in the stress he lays on the intensity of the primary function, whereas Gross stresses the length of the secondary function. "Introversion is characterized by general tension, an intensive primary function and a correspondingly long secondary function." Jung summarizes Gross's contribution by saying: "Gross deserves considerable praise for being the first to put forward a simple and unified hypothesis concerning the origin of these types."

One great advance of the formulation given by Gross over that given by Jordan appears to be that it lends itself extremely well to experimental verification. The first investigators to attempt such a verification were two Dutch psychiatrists, G. Heymans and E. Wiersma, who based a rather more complex system of typology on the notion of primary and secondary function, and who attempted to use objective tests of perseveration as measuring devices. They may be said to have anticipated the two main lines along which modern attempts at the verification of "type" hypotheses have proceeded. As will be shown in Chapter II, they used the method of intercorrelation of traits to demonstrate those sets of correlated

qualities we have agreed to call types, and, as shown in Chapter IX, they used the method of objective test construction according to the dictates of hypothesis for the measurement of the functions allegedly underlying the sets of observed correlations. In their work they have slightly expanded and altered the descriptive account given by Gross; as Roback points out, what characterizes the "shallow-broad" type for these writers "is *change*, lightness, lack of endurance and ready susceptibility to objective stimulation", while the concept of the "deep-narrow" type "entails the qualities of seriousness, solidity, endurance, and great susceptibility to ideational stimulation". As a much more detailed account of the work of Heymans and Wiersma will be given below in connection with their experimental studies, no more need be said here.

Spearman (1927) took up the concept of perseveration and made it into a fundamental law, his famous "Law of Inertia": *Cognitive processes always both begin and cease more gradually than their (apparent) causes.* In this law he tried to combine the theoretical contributions of writers such as Gross (1902) and Jung (1921), and the experimental studies of perseveration by Müller (1900), Wiersma (1906), Heymans and Brugman (1913), and others. As his treatment cannot be divorced from the experimental studies on which his law is based, we must postpone consideration of his contribution to a later page, where the empirical studies of "perseveration" will be reviewed in detail.

We must now turn our attention to Jung (1921), whose views have already been alluded to many times. Basing himself on the work of Jordan and Gross, Jung sees the main cause of typological differences in the extraverted or introverted tendency of the libido, i.e. in the tendency of the individual's instinctual energies to be directed mainly towards the outer world (objects), or towards his own inner mental states (subject). "When we consider a person's life-history, we see that sometimes his fate is determined more by the objects which attract his interest, while sometimes it is influenced rather by his own inner, subjective states. . . . Quite generally one might characterize the introverted point of view by pointing to the constant subjection of the object and objective reality to the ego and the subjective psychological processes. . . . According to the extraverted point of view, the subject is considered as inferior to the object; the importance of the subjective aspect is only secondary."

Jung gives very extensive descriptions of the personality traits which characterize the introvert and the extravert respectively; these

descriptions agree to a considerable extent with those of Jordan and Gross. The extravert emerges as a person who values the outer world, both in its material and in its immaterial aspects (possessions, riches, power, prestige); he seeks for social approval and tends to conform to the mores of his society; he is sociable, makes friends easily, and trusts other people. He shows outward, physical activity, while the introvert's activity is mainly in the mental, intellectual sphere. He is changeable, likes new things, new people, new impressions. His emotions are easily aroused, but never very deeply; he is relatively insensitive, impersonal, experimental, materialistic, and tough-minded. He tends to be free from inhibitions, carefree and ascendant. This brief description makes no pretence at being anything more than the most superficial summary of some of the characteristics of the extravert; they form a bare statement of a list of those traits which in Jung's opinion correlate together to define the extravert type. Our main interest will be in the empirical verification of Jung's conception, rather than in its detailed statement.

Jung links his description of extraversion-introversion with the distinction between the main neurotic disorders as given by Janet (1894, 1903). As is well known, Jung believes that the extravert in cases of neurotic breakdown is predisposed to *hysteria*, the introvert to *psychasthenia*. "It appears to me that much the most frequent neurotic disorder of the extraverted type is hysteria. . . ." On the other hand, speaking of the introvert, he maintains that "his typical neurotic disorder is psychasthenia, a disorder which is characterized on the one hand by marked sensitivity, on the other by great exhaustion and constant tiredness." Nowadays we would probably refer to "anxiety state", or "reactive depression" rather than to the obsolescent term "psychasthenia", which also held overtones of obsessional and compulsive tendencies. On the basis of a factorial study of 700 neurotics, referred to in the next chapter, Eysenck (1944) suggested the term "dysthymic" as a more modern equivalent to cover this syndrome of correlated affective disorders. (In literal translation, this term means mood-disorders, and appears to single out the hypothetical underlying emotional dysfunction or hyperfunction posited by Gross, Jordan, and Jung.)

Although Jung never formally elaborated this part of his hypothesis, it can be seen quite clearly that implicit in his scheme is a second factor additional to, and independent of, that of extraversion-introversion. This factor we may provisionally call "abnormality" or "neuroticism"; it is identified as that particular quality which

hysterics and psychasthenics have in common as compared with normal persons. The independence of introversion and neuroticism is especially stressed by Jung: "it is a mistake to believe that introversion is more or less the same as neurosis. As concepts, the two have not the slightest connection with each other". If we wish to represent Jung's complete scheme, then, we must have recourse to two orthogonal factors or axes, one of which represents the extravert-introvert continuum, the other the normal-neurotic continuum. This additional factor of abnormality is also implicit in both Jordan and Gross; it is explicitly mentioned by Heymans and Wiersma, as will be shown later. Consequently there is considerable agreement here also between the authors so far considered.

Like Jung, Kretschmer (1948) took his prototypes from the psychiatric field, but unlike Jung he turned to the psychotic forms of disorder rather than the neurotic. Following Kraepelin and Bleuler, he distinguished two main psychotic syndromes or groups of symptoms: the schizophrenic on the one hand, the manic-depressive or cyclic type of psychotic on the other. Unlike most other psychiatrists, however, Kretschmer considered these disorders not as in any way qualitatively different from normal mental states, but merely as the extremes of a continuum, as exaggerated forms of behaviour patterns characteristic of normal persons. This hypothesis may per-

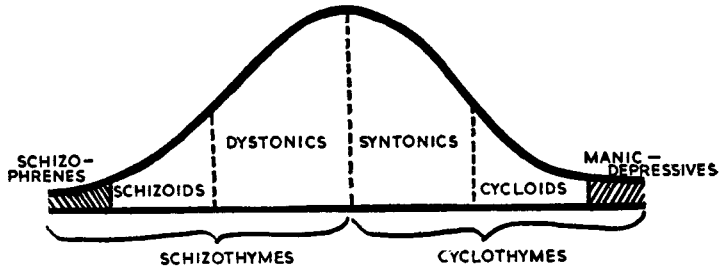

Fig. 5.—Diagrammatic Representation of Kretschmer's Theory.

haps be illustrated in terms of Fig. 5, showing the hypothetical distribution of the whole population in terms of a normal curve of distribution, ranging from one extreme (schizophrenia) to the other (manic-depressive insanity). All persons left of the mean would be *schizothymics*, meaning by that merely that their personality make-up had in common certain elements which are grotesquely exaggerated in those psychotic patients whom we label schizophrenics, whereas all those to the right of the mean would by *cyclothymics*, meaning

by that that their personality make-up had in common certain elements which are grotesquely exaggerated in manic-depressive patients. Persons who are definitely abnormal but not yet psychotic Kretschmer would call schizoid or cycloid respectively, according to the side on which they fell, whereas the large number of persons in the centre of the distribution he would call syntonic if they were on the cyclothymic side, and dystonic if they were on the schizothymic side. (It is possible that Kretschmer would object to the use of a normal curve to depict the relation between schizothymes and cyclothymes, but little importance can in any case be attributed to the form of distribution when the underlying metric is unknown.)

Kretschmer's description of the cyclothyme has certain similarities to the types already considered. Like the extravert, the active-unimpassioned, and the broad-shallow type, the cyclothyme is objective, realistic, sociable, optimistic, hedonistic, trustful, co-operative, and frank; he is also subject to mood-changes without apparent cause. The schizothyme, like the introvert, the passive-impassioned, and the narrow-deep types, shows the opposite qualities to these. Kretschmer is suggesting, then, a definite dimension of personality similar to, but probably not identical with, those considered above. It would also seem to follow from his writings that another dimension is also implied, ranging from normality to psychotic disorder, and orthogonal to the cyclothymia-schizothymia dimension. Thus, as in the case of Jung, Kretschmer's theory would best be represented in terms of two orthogonal factors or axes, one measuring cyclothymia-schizothymia, the other normality-psychotic abnormality, or "psychoticism". (Indeed, if we were to follow him faithfully, we would have to add another two dimensions, namely the diathetic and the psychasthetic scales. In his view, cyclothymes vary among themselves on a scale ranging from humorous, vivacious, quick-witted, to the quiet, calm, serious—the so-called diathetic scale; whereas schizothymes vary from shy, nervous, sensitive, to dull, stupid, torpid—the so-called psychasthetic scale. As, however, there is no experimental evidence in Kretschmer's work regarding these scales, and as he makes little use of them and does not define their relation to each other in any way, we shall not deal with these scales in detail. In a similar manner, and for similar reasons, we have not discussed Jung's amplification of his theories in terms of the four functions of feeling, thinking, sensation, and intuition. Little is gained by a discussion of refinements when the major structures themselves are in doubt.)

Kretschmer's approach is more experimental than that of any of his predecessors, and a review of some of the empirical studies carried out by him and his students has been presented by Eysenck (1950). As his attempted proofs do not make use of the correlational or factorial methods, they will not here be considered in any detail; such factorial studies as have been carried out by psychologists outside his immediate circle will be mentioned in later chapters. Two points, however, call for notice. In the first place, Kretschmer attempts to anchor his typology on the firm facts of biological constitution, relating personality types and psychotic syndromes to types of body build. The schizophrenic, and accordingly the schizoid and dystonic person also, is believed to show in the majority of cases an asthenic, leptosomatic type of body-build; the manic-depressive, and accordingly the cycloid and the syntonic person also, is believed in most cases to show a pyknic, thick-set type of body-build. A review of factorial studies of body-build, and an evaluation of this claim, will be found in a later chapter; the hypothesis of this body-mind correlation had to be mentioned at this point because no account of Kretschmer's work could be complete without what he would consider his main contribution.

The second point to be considered relates to Kretschmer's psychological theories regarding the dynamic causes underlying the typology advanced by him. Kretschmer holds that the concept of *dissociation* (Spaltung) is of fundamental importance in understanding the mentality of the schizothyme, just as its opposite, integration, is important for the understanding of the cyclothyme mentality. By *Spaltung* he means "the ability to form separate and partial groupings within a single act of consciousness; from this results the ability to dissect complex material into its constituent parts". This concept of dissociation is very reminiscent of Gross's long secondary function leading to dissociation of "themas", and giving rise to the "sejunctive" type of personality. Kretschmer has added, and this is a most important contribution, a whole series of experimental tests for the measurement of this hypothetical trait of "dissociative ability". Unfortunately these tests have not yet been subjected to factorial study, and consequently their functional unity must remain unestablished; the extensive evidence reviewed by Eysenck (1950) suggests strongly, however, that the majority of these tests succeed in differentiating the cyclothyme from the schizothyme and the leptosomatic from the pyknic type of person.

The apparent similarity between the concepts of schizothymia and

introversion, and between cyclothymia and extraversion, has led many psychologists and psychiatrists to identify the respective schemes of Jung and Kretschmer. In doing so, they have often overlooked certain consequences which are implicit in such an identification, but which are seldom if ever brought out in the purely semantic process employed in carrying out such identification. Fig. 6 gives a diagrammatic picture of some of these implications. It will be seen, in the first place, that in accepting such a view we are committed to

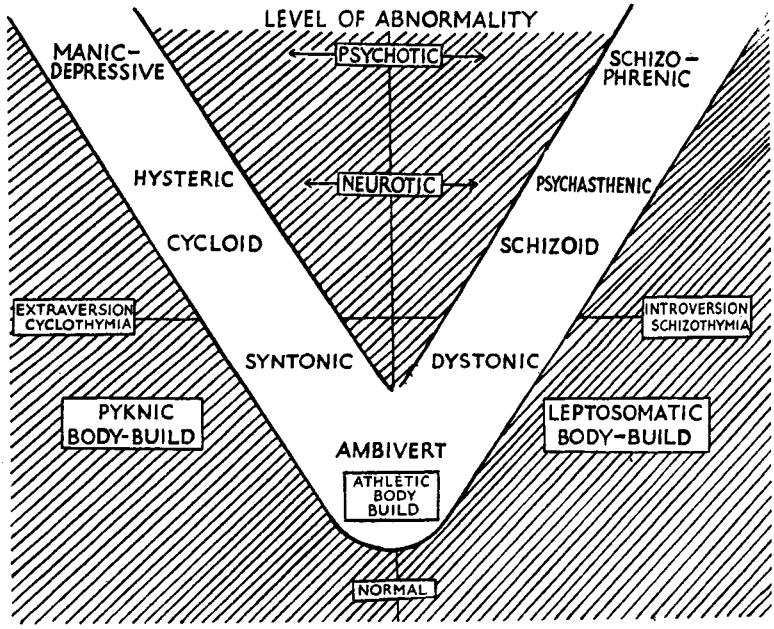

Fig. 6.—Diagrammatic Representation of Combined Jung-Kretschmer Theory.

the proposition that psychotic and neurotic disorders lie along one and the same continuum of "abnormality", a view explicitly held, among others, by Freud, who advances his hypothesis of "psychosexual regression" to account for differences in psychiatric patterns. In the normal person, there is hardly any regression to infantile patterns of psychosexual adjustment. In the conversion-hysteric, and even more so in the anxiety-hysteric, there is a considerable amount of such regression, while in the manic-depressive, and particularly in the schizophrenic, regression is almost complete. This hypothetical axis of "regression" would correspond quite well to the ordinate in our diagram. A similar assumption to Freud's is made by Kretschmer,