

PERSONALITY¹

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Personality is probably the most general and the least well defined term in use in psychology, and it is not surprising that the selection of topics to be dealt with, and papers to be included, in this chapter is even more arbitrary than is the case in other parts of this book. Inevitably, a review of this kind will serve the function of a projective technique as much as that of an objective appraisal, and the firm belief that this procedure is not contrary

for structuring this review around certain firmly held views regarding the nature and development of personality. These views are not altogether in line with much current thinking, and it may be worth while to mention them briefly so that the reader may be able to discount possible bias. In the first place, I regard the study of personality as a scientific discipline subject to all the customary dictates of scientific methodology. This, to my mind, excludes the clinical, idiopathic, and intuitive methods of approach, except as sources of hypotheses. Secondly, I would lay much greater stress on constitutional and hereditary factors in personality than is common; fortunately, there is evidence to be reported in this chapter which is very relevant to this problem. Thirdly, I believe in the importance of classificatory or taxonomic problems in the early stages of a science. Again, there is ample material in this review to illustrate this belief and put it into an experimental setting.

The discussion will be in four main parts. The structure; the second to personality development; the third to methodology and measurement, and the fourth to theory. Also included is a section on textbooks and summaries.

ORGANISATION

Probably the most important event during the past year has been the appearance of the twentieth edition of Kretschmer's *Körperbau und Charakter* (56). It would probably be true to say that very few authors have been more misrepresented by writers of textbooks and even by research workers in their own field than has Kretschmer. The reasons for this are not far to seek. An English translation of Kretschmer's work appeared early in the 1920's and has served as a source book for most English and American psychologists ever since. While this edition sets out Kretschmer's main hypothesis regarding the correlation between schizothymia and leptosomatic body build and between cyclothymia and pyknic body build, it does not even

¹ The survey of the literature to which this review pertains was completed in May, 1951.

adumbrate the vast amount of experimental work in many fields which has since been carried out in Kretschmer's laboratories in Marburg and then in Tübingen. This material has been incorporated in successive editions of Kretschmer's book—a custom common on the European continent but quite alien to the Anglo-American tradition. [Fisher's *Statistical Methods for Research Workers* (27) is the one exception that proves the rule.] Hence, the subtle changes that have taken place in Kretschmer's way of thinking about the problem of types and the vast structure of psychological testing which he has built up on the basis of his theories have been almost completely neglected by workers in the field of personality.

Essentially, Kretschmer's system is a typology, but we should be careful to understand the word "type" in the sense which Kretschmer himself gives to it and avoid knocking down again the man-of-straw built up by textbook writers and used for generations to scare away the young student from this important area of psychological thinking. To Kretschmer, a type is not an either-or system of absolute classification; he defines it essentially in terms of observed correlations:

Konstitutionsforschung ist Korrelationsforschung. . . . *Typenforschung*—dies wollen wir klar festhalten—*beginnt erst dort, wo empirische Zusammenhänge und Korrelationen zwischen biologischen Merkmalsgruppen nachgewiesen werden, die man vorher nicht kannte oder nicht beweisen konnte.*

While Kretschmer is not familiar with factor analysis, his methodology, as far as it is expressed in words, shows a striking resemblance to procedures advocated by factor analysts, and, indeed, his thinking can be translated almost directly by substituting the word "factor" for the word "type." For all those who take seriously the theoretical dilemma posed by the traditional discussion of type versus trait psychology, Kretschmer's chapter on "Der Konstitutionstypus als naturwissenschaftliches und erkenntnistheoretisches Problem" must surely be required reading.

When we turn from the general question of type to Kretschmer's experimental procedure, it is impossible not to be impressed by the ingenuity of his approach and the psychological acumen shown in his choice of concepts. In the absence of a translation of Kretschmer's chapters on "Beziehung zwischen Körperbau, Persönlichkeit und Psychose im Experiment" and "Konstitution und Leistung," the reader may wish to refer to a brief summary of some of these findings which recently appeared in the *Journal of Personality* (18). This summary also deals with the very ingenious experimental method which Kretschmer has used throughout to prove certain points important to his general theoretical system, such as, for instance, the essential similarity of nonpsychotic, normal individuals to the two groups which he regards as the prototypes of his typology, i.e. the manic-depressives and the schizophrenics. It will be noted that Kretschmer has anticipated a good deal of the current interest which is being taken in the relation between personality and perception and has also shown many important relations between personality and motor phenomena. Of great

interest, too, are his experimental studies on the relation between personality and autonomic functioning.

I do not wish to give the impression that Kretschmer's system can be regarded as definitely established. There are obvious weaknesses in his procedure although these do not necessarily coincide with those which appear obvious to most readers at first. The fact, for instance, that there is a complete absence of indices of statistical validity is not of any great importance; in going through 100 or so studies from Kretschmer's laboratory, I have routinely calculated these indices and found that, in nearly all cases, reported differences were significant at the 1 per cent level. It is easy to "make the dubious assumption that scientific wisdom increases by steps significant at the 5 per cent level" (7). There must be thousands of questionnaire studies, meticulously accurate in their statistical treatment, which, nevertheless, are completely barren of any psychological interest; Kretschmer's work is of the highest importance psychologically, and though I would be the last to advocate lack of rigorous statistical treatment, I feel that such treatment is only appropriate when it is used on worth-while data accumulated in an attempt to test a worth-while hypothesis.

A typology similar to Kretschmer's in some ways, but laying great importance on embryonic constitution, is that of Martiny (65), who writes in the same tradition as other Continental authors whose hypotheses preceded Sheldon's (89, 90, 91) and were taken up by him later. According to Martiny,

La biotypogenèse permet en s'appuyant sur l'embryogenèse, de considérer 4 biotypes génétiques, véritables constitutions de base, l'entoblastique, le mésoblastique, le chordoblastique, l'ectoblastique. . . . La constitution entoblastique, dans l'absolu et le relatif, se marque anatomiquement par sa rondeur tissulaire, par la grosseur plus grande des organes du tube digestif; physiologiquement par l'exagération fonctionnelle des glandes endocrines, appareils et systèmes de l'anabolisme anaérobie; psychologiquement par une prédisposition au statisme endesthésique.

La constitution mésoblastique ajoute l'épanouissement de ses organes, appareils et systèmes issus de son tissu primitif à ceux d'un entoblaste normalement développé. Le mésoblastique anatomiquement trapu et sanguin, physiologiquement anabolique aérobie, est psychiquement mû par un dynamisme amphiesthésique.

La constitution chordoblastique jouit d'un parfait équilibre par l'apport à égalité des éléments de l'ectoblaste à ceux de l'entomésoblaste. Le chordoblastique anatomiquement élancé et musclé, physiologiquement catabolique aérobie, est psychiquement en possession d'une harmonie cénesthésique.

La constitution ectoblastique se traduit par une carence des éléments entomésoblastiques, laissant une prévalence absolue et relative au système neuro-sensoriel. L'ectoblastique anatomiquement mince et chétif, physiologiquement catabolique anaérobie, est psychiquement prédisposé à un détachement exesthésique.

As with Kretschmer, it is impossible to summarize the wealth of information given in this book and the many suggestive correlations posited between physiology, anatomy, psychology, psychiatry, and other disciplines. There is hardly any parallel in the Anglo-Saxon countries to the stress laid on interdisciplinary constitutional work on the Continent, and in view of the lack

of suitable translations and the abandonment of language requirements by many universities, it is unfortunately likely that this gap will become more and more marked. Perhaps a closer perusal of Continental journals, such as *Biotypologie* and of summary articles such as that of Schreider (86) will do something to bridge this gap.

Two British articles on body build (79, 80) provide the transition to a typological system less wide in its impact than the French or German and more strictly controlled from the statistical and experimental point of view, namely that expounded by the writer in *Dimensions of Personality*, a book whose appearance in a French translation during the past year may serve as a point of unification for a number of studies (22). As will be remembered, an attempt was made in that book to discover certain major taxonomic variables in personality by means of factorial techniques applied to objective test scores. The main dimensions discovered outside the cognitive field were those of neuroticism and of extraversion-introversion. These psychological dimensions were found to be related to body build in men, and the two recent studies by Rees referred to above show that similar correlations and factors can be found for women also. As previously, an index was derived from the factorial study of body measurement to determine a person's "body type," so that now we have available such an index for women as well as for men. The fact that such reproducible correlations exist between body type and the measured dimensions of personality suggests strongly that these dimensions are constitutional in origin; a more direct proof for this proposition will be given later. On the more definitely psychological side, Himmelweit & Petrie (43) have applied the type of objective personality test made familiar in *Dimensions of Personality* (19) to groups of normal and neurotic children in an attempt to discover whether the same factor of neuroticism could be isolated there also. The experiment was entirely successful and the "discriminant function" analysis of the two groups showed a relatively low level of misclassification, particularly when the lack of reliability of the psychiatric criterion is taken into account.

The factor of neuroticism also appears in two studies which are of interest here for rather different reasons. The first of these is a statistical study of the Rorschach test in which 100 Indian students were given the Rorschach test and verbal and nonverbal group tests of intelligence, as well as Cattell's test of fluency (88). In addition, each subject was rated independently by two judges on a number of personality traits. (The average reliability of these ratings was .71.) Tetrachoric correlations were calculated between 36 Rorschach scoring categories, and three main factors were extracted. The first of these was one of associative fluency, which correlated with the fluency test and with the rating for imagination. The second factor was one of intelligence, correlating quite highly with the two intelligence tests and also with a rating for intelligence. It is the third factor which is interesting in this connection; a correlation of .68 was found between Sen's third factor and the rating for neurotic tendency, which is the highest of all correlations recorded between factors and external criteria. We thus find

strong evidence in this factorial study of the Rorschach, both of the existence of the factor of neuroticism and the value of the Rorschach test in predicting the strength of this factor in a given individual.

The other factorial study to which attention is drawn here is one carried out by Reyburn & Raath (81). This experiment involved a rating by 83 observers of two subjects each, with relatively high reliability of the ratings. These ratings were on a five-point scale covering 45 well-defined personality traits. The table of intercorrelations was factor analysed and an oblique solution is reported which gave rise to six factors which are not independent of each other. The correlations between these factors clearly give rise to higher order factors which were not derived by Reyburn & Raath, but were calculated for the purpose of this review because of their theoretical interest. Thirteen iterations were required before the communalities began to converge. The results of the analysis are very clear-cut: there are two factors, the first one clearly the neuroticism factor (with a saturation of .97 on "stability"), and the second factor clearly an extrovert-introvert factor, with high positive saturations for "assertiveness" and "spontaneity" and negative ones for "sensitivity" and "inferiority."

We must now turn to quite a different type of investigation which still, however, falls under the general heading of personality organisation, namely, a book by the Gluecks (31) on juvenile delinquency. This 10-year study involving almost three dozen investigators is a monument to patient and painstaking accumulation of details relevant to the characterization of a particular type of person, in this case the juvenile delinquent, and to the equally painstaking analysis of the data. Not all of the book is relevant to psychology, and if those parts that do bear witness to the aridity of such a collection of material when there is no guiding hypothesis underlying it, this may be due in part to the sociological and legal training of the investigators. Roughly speaking, 500 delinquent boys were matched with 500 non-delinquent boys on age, general intelligence, national origin, and residence in under-privileged neighbourhoods. Investigations of particular interest to psychologists were carried out by means of the Rorschach test and through the use of Sheldon's somatotyping technique, which again links this study with the constitutional work of Kretschmer, Martiny, and others. In addition to these data, psychiatric interviews were held with all the children. It is somewhat of a surprise to note that, although the two groups were matched with respect to intelligence, there is a significant difference ($CR=3.57$) on the Wechsler Verbal Scale between the two groups, a difference larger than most of those found with respect to "qualitative and dynamic aspects of intelligence" by means of the Rorschach. When we come to the more affective and conative aspects of personality, we learn from the Rorschach that the delinquents are significantly less suggestible and less neurotic than are the nondelinquents. From the psychiatrist we learn that the delinquents are much more suggestible and have far more emotional conflicts, as well as being less adequate with respect to their deep-rooted personal dynamics. The authors seem to feel that such contradictions may

make it difficult for the reader to find out precisely which of the two groups is more suggestible or which is more neurotic. They therefore point out that the psychiatrist and the Rorschach expert do not agree with respect to their definition of these terms. They say,

The psychiatrist defines emotional instability as "a conflict of feelings," while the Rorschach trait of emotional lability is defined as "lack of affective inhibition in a quantitative sense."

It is difficult to understand either of these definitions, and it still remains puzzling why emotional instability and emotional lability should show a high negative correlation. Here, indeed, is a clear example of the absolute necessity of operational definition if any worth-while results are to be had from investigations of this kind.

One further point of interest relates to the Rorschach test. It is claimed that, in a blind analysis, a considerable number of cases were correctly diagnosed as delinquent or nondelinquent. Here, as elsewhere in the book, there is no recognition of the possibility, indeed the probability, that there are features in the protocol such as, for instance, the use of swear words, etc., which would make such recognition possible, quite apart from any formal Rorschach interpretation. Until such possibilities are ruled out, little attention can be paid to the results as reported.

Another set of data which is of interest is that related to bodily constitution. It is found that in gross bodily size, delinquents are superior to non-delinquents and that in delinquents there is a greater laterality of body-build. By and large, according to Sheldon's system, the delinquents tend to be mesomorphic, while the controls do not have any excess in that somatotype. This finding, indeed, we may regard as well established, and it is of particular interest in view of the force with which it points to the importance of constitutional factors in delinquency.

DEVELOPMENT

Hypotheses regarding the development of personality have usually been either in terms of environment, in terms of heredity, or, very much more rarely, in terms of both plus an interaction factor. A quick glance through recent publications in this field reveals an overwhelming bias in most psychological and psychiatric writers in favour of environmentalistic hypotheses and a complete disregard of the possibility that the figures given in support of this may find an equally easy interpretation in terms of a nativistic hypothesis. Slater (92) has drawn attention to this bias in a very forcible manner:

There has . . . been an increasing tendency among clinicians to minimize the effects attributable to genetical causes, and to teach a psychiatry in which they receive little or no mention. This tendency has been marked in Britain, but it has assumed formidable strength in the U.S.A. Instead of a harmonious development, in which the psychoses and neuroses, constitution and environment, psychogenesis and physiogenesis receive their due share of attention, interest among practical workers has been de-

voted more and more exclusively towards psychotherapy, psychoanalysis, social psychiatry, personnel selection, group therapy, and preoccupations with anthropology, sociology and political theory. In its one-sidedness, this development is not healthy.

It is a sign of bad omen that it is possible for text-books of clinical psychiatry to appear, with claims for comprehensiveness, in which no mention is made of the established facts of genetics and of the hereditary element in mental disorder. Their authors appear to feel, though in fact this view depends on a misapprehension, that recognition of a hereditary factor implies a therapeutic nihilism; and that an energetic and optimistic attitude towards treatment calls for a neglect of hereditary factors, just as a due appreciation of the patient as an individual demands forgetfulness of nosological entities. One suspects that the prime motivation is derived from the philosophy of Dewey, which is no doubt over-simplified in the notion that one should accept as true that which has convenient practical applications.

It would not perhaps be putting it too high to say that we are witnessing the manifestation of an anti-scientific tendency which is winning an increasing number of supporters. The customary canons of scientific reasoning are ignored by these schools. Uncomfortable facts are left unconsidered. Hypotheses are multiplied regardless of the principle of economy. Explanations which may be valid for certain members of a class of phenomena are regarded as true for the class as a whole. Interpretations which conform with theory, and which might be true, are regarded as established. Possible alternatives are not considered, and no attempt is made to seek for evidence of critical value which shall decide between them. Criticisms from outside are ignored, and only the initiate may be heard. Utterance is dogmatic and arrogant and lacks scientific humility and caution. These are the mental mechanisms which we associate with the growth of a religious orthodoxy, and not with the progress of science. The movement is of significance to genetics, because it is likely adversely to affect the personnel and facilities for research, and to lead to a psychiatry without biological foundation and divorced from contact with the other natural sciences.

The comparison of two groups, such as delinquents and nondelinquents as in Glueck's study, has often been used to extract information or test hypotheses regarding causes for differential development. An example of such a study may be found in a comparison by Warren (102) on 90 children suffering from conduct disorders and 70 children suffering from neurotic disorders. Some of the historical antecedents appear to have a close relation with eventual behaviour of these children. Some of the most interesting ones are related to maternal behaviour. Thus, oversolicitude on the part of the mother is present in 14 per cent of the conduct disorder group, but in 40 per cent of the neurotic disorder group. Undersolicitude, on the other hand, occurs in 40 per cent of the conduct disorder group and in only 7 per cent of the neurotic disorder group. Paternal over- and undersolicitude, whilst tending in the same direction, appears much less highly related to filial conduct. Overanxiety in the mother before the present disorder developed in the child was found in 39 per cent of the cases in the conduct disorder group, but in 71 per cent of the cases in the neurotic disorder group. Separation from the mother before the age of five was found in 46 per cent of the cases in the conduct group and in 16 per cent of the cases in the neurotic disorder group. Similar figures for the father are 69 per cent and 39 per cent,

respectively. All these figures are very highly significant and are discussed by Warren, under the heading of "Etiological Factors."

He also mentions certain constitutional factors which are interesting. Thus, for instance, epilepsy and psychopathy occur in 22 per cent of the family histories of the "conduct disorder" group and in only 11 per cent of the cases in the "neurotic disorder" group. Neuroses, on the other hand, are more frequent in the family histories of neurotic children (43 per cent) than in the family histories of conduct disorder children (32 per cent). These findings suggest an alternative conclusion to that of Warren. Instead of accounting for the observed correlation between solicitude or anxiety and disorder in terms of a direct causal relationship, we may prefer an interpretation in terms purely of constitutional factors, i.e., neurotic mothers have neurotic children and also show overanxiety, oversolicitude, etc. Epileptic and psychopathic mothers have children with conduct disorders; they also tend to show undersolicitude and little anxiety. The data given do not enable us to decide between these two hypotheses—indeed, it is possible that we may here be dealing with an interaction between heredity and environment. It is interesting, however, to note the ease with which the environmental hypothesis is accepted by most writers and readers in spite of the complete lack of direct evidence in its favour.

The fact that a mother's behaviour pattern (say, oversolicitude) is correlated with the child's behaviour pattern (say, neuroticism) may indeed be an expression of a direct causal relation. It is equally possible that both the mother's and the child's behaviour are genetically determined and that their correlation is due to the fact that both are correlated with this third variable. In nearly all the papers examined here, it will be found that there is no possibility of deciding between these two alternatives, yet nearly all the writers have explicitly accepted the environmentalistic hypothesis and implicitly rejected the hereditary hypothesis. In drawing attention in each case to the alternative possibility, I do not wish to be misunderstood as maintaining that this alternative hypothesis is more likely to be correct than the one presented by the writers concerned; in the absence of further evidence it is simply impossible to decide one way or the other. In such a situation, it is clearly unscientific to accept one hypothesis without even considering the possibility of an alternative hypothesis. Evidence strongly favouring the hereditary hypothesis with respect to at least one major dimension of personality will be discussed when we come to the last paper in this series.

Another research of much interest in connection with the development of personality is reported in two papers by Goldman-Eisler (32, 33), which take their cue from the Freudian hypothesis of oral types. Her procedure is of considerable interest from the methodological point of view. Taking the description of the orally gratified and the orally ungratified type from psychoanalytic literature, she has constructed a set of 19 questionnaire scales, each containing on the average eight items to measure these 19 traits. It was possible, from Freud's theory, to predict the way in which the scales should hang together and the kind of factor structure which should

emerge from their intercorrelations. Having shown that the factor emerging from the matrix of intercorrelations is indeed similar to the one posited by Freudian theory, the author goes on to the second part of her task which is to test the hypothesis that time of weaning is causally related to oral gratification or lack of oral gratification. Using factor scores for almost 100 adult subjects on the oral gratification factor and information obtained from the mothers of these subjects as to the age at which they were taken off the breast, an analysis of variance was carried out on the scores of the early and late weaners, defining early weaning as weaning before the age of four months, and late weaning as weaning at five months of age, or more. A correlation of .3, fully significant at the 1 per cent level, was found between oral optimism and late weaning.

This study, exemplary as it is of its kind, also suffers from its failure to consider the hereditary hypothesis. It is quite conceivable that an innate factor may cause both early weaning and the personality trait denoted as "oral pessimism"; the correlation between these two may be entirely fortuitous. It may be objected to such an interpretation that seemingly the author has gone through all the steps recommended by writers on scientific methodology, i.e., she has taken a hypothesis, made a deduction from this hypothesis, and verified the deduction. Such an objection, however, would hardly be valid because, as so often in psychology, it would take the empty shell of scientific methodology for the real thing. The hypothesis itself, in the case of Freud, was, on his own showing, the result of the observation that early weaning and "oral pessimism" coincide; if the hypothesis is based on such an observation, then clearly the verification of the observation cannot be used to verify the hypothesis. It is only when deductions are made from hypotheses, such that they lead to new observations, that we can really talk about verification in the scientific sense. This should not be taken to detract from the value of Goldman-Eisler's study, which has added two important facts to our knowledge of personality structure and development; it should rather act as a spur to devise experiments which take into account alternative hypotheses which must be considered before we can accept the environmentalist's hypothesis.

Rather similar to Warren's study, but more rigorous in its criteria of selection, is an article by McKeown (62) on the behaviour of parents of schizophrenic, behaviour problem, and normal children. Among the schizophrenics, the parents of the same sex showed a marked incidence of dominant, antagonistic behaviour; the same type of behaviour is shown by both parents among the behaviour problem children. Encouraging behaviour is rare among parents of schizophrenic and behaviour problem children, but is predominant among the parents of normals. It is hardly necessary to repeat our general criticism of conclusions drawn from such statements; the results are quite as explicable in terms of heredity as of environmental influences.

More cautious in its interpretations is a review of intrafamily resemblances in personality characteristics (82). (a) The evidence goes to show

that the resemblances of both sons and daughters to mothers may be higher than that of fathers when the children are immature; this difference, however, tends to disappear at maturity. (b) When a test is decidedly more appropriate to one sex than the other, greater similarity is usually found for the more adequately measured pairs. (c) On measures of attitude and information, daughters tend to resemble both parents more than do sons. (d) There is no indication on any variable that mother-son or father-daughter resemblance is greater than that for other parents. These results, while they throw a limited amount of light on the Freudian hypothesis, are, of course, neutral with respect to the environmentalist-nativistic discussion.

Another study with a somewhat ambitious title "The Influence of Constitution and Environment on the Development of Adopted Children" (72) is much too sketchy to make any appraisal possible, but as far as can be seen, it has avoided none of the pitfalls to which such studies are exposed. Another writer (57), who bases his conclusions regarding the greater influence of environmental factors on personality on a study of three pairs of identical twins, uses a somewhat anecdotal method, which can hardly be regarded as likely to impress the scientific investigator.

We must now turn to a strictly experimental study in which an attempt was made to give numerical expression to the extent to which hereditary influences can be said to account for the total variance of a particular operationally defined personality trait (26). This study, which begins with a review of the work done with monozygotic and dizygotic twins in the field of personality, makes a fundamental criticism which applies not only to the studies reviewed but also to twin studies in the cognitive field. The argument runs roughly like this: The typical experiment in this field attempts to find an answer to a rather general question, such as, for instance, the relative contribution of heredity and environment to individual differences in intelligence. The experiment is set up by giving a particular test, such as the Binet, to groups of monozygotic and dizygotic twins and using a particular statistical technique, such as, for instance, Holzinger's h^2 (71) to give a numerical answer to the original question. It is pointed out that this procedure is quite fallacious because it equates "intelligence" with "Binet score." This objection is made more explicit by reference to the factorial equation of a given test. Studies by McNemar (64) and Burt & John (10) have shown that only about 40 per cent of the total variance of the Binet test is accountable for in terms of a general factor of intelligence, leaving the remainder, i.e., over half of the total variance, to be accounted for in terms of verbal, numerical, memory, and other group factors, as well as, of course, a specific factor. Now a finding that the Binet test score is inherited to the extent of 80 per cent throws no direct light on the inheritance of intelligence. It may be that 40 per cent of the total Binet variance, which is a measure of "g", is inherited 100 per cent and that the group and specific factors are inherited to only a very limited degree. Alternately, it is possible that "g" is inherited to a very limited extent indeed, while the group and specific factors are inherited 100 per cent. In other words, no conclusion can be

drawn from a specific test to the inheritance of a trait measured imperfectly by that test. It follows from this argument that a completely different methodology is called for, and this methodology will become apparent in the description of the experiment itself.

A battery of tests which had been previously shown to be good measures of the personality trait of neuroticism were given to 25 pairs of monozygotic and 25 pairs of dizygotic twins, aged between 11 and 13. Scores on these tests for the total group of 100 children were intercorrelated, and a factor analysis carried out using the method of criterion analysis recently proposed for experimental studies in the field of personality (23). (As a control group, 21 neurotic children were used in order to find a unique and invariant solution to the problem of rotation.) Intraclass correlations were computed for monozygotic and dizygotic twins separately on each of the 17 tests used as well as on the factor scores of the children derived for the general factor of neuroticism. Also calculated for each test and for the factor score was Holzinger's h^2 statistic, purporting to give an estimate of the percentage of the total variance contributed by heredity. The intraclass correlation of the neuroticism score was .85 for monozygotic twins and .22 for dizygotic twins, giving an h^2 of .81, thus indicating, if we can indeed make the assumptions on which Holzinger's h^2 is based, that 81 per cent of the total variance of this trait is due to hereditary influences. This result is not subject to the objection mentioned previously as now we are not dealing with an individual test but with a factor score.

From these data another important conclusion can be drawn. It is found that the h^2 for any one of the 17 tests used is considerably smaller than the h^2 for the neuroticism factor. This shows conclusively that this factor is not a statistical artifact but has very definite biological reality, a demonstration of particular value in view of the frequent criticisms of factor analysis made on this score. Appended to the article is a note dealing with the assumptions underlying Holzinger's h^2 statistic (66) which makes it appear very doubtful, to say the least, that the value of 81 per cent given above can be regarded as more than a very rough approximation to the true value. However, it does appear clear from this study that heredity plays a very strong part in the determination of personality differences and that psychology cannot go on making the easy and implicit assumption, which is inherent in both psychoanalysis and behaviourism, that environment is the only major variable determining individual differences. This belief, which has acquired a mystical and almost religious fervour in the U.S.S.R. is also widespread in democratic countries, for reasons which, as Pastore (73) has pointed out, may not be entirely apolitical. It seems a great pity that scientific issue should be clouded in this fashion.

METHODOLOGY AND MEASUREMENT

In a sense, it may be said that personality research stands and falls with the adequacy, or otherwise, of its methodology. The many contradictory findings which are published in the literature are almost entirely due to differ-

ences in methodology, and it would be a great boon to science if editors of psychological journals would firmly refuse to print papers containing obvious methodological shortcomings. An example of the differences in results which may be found when methodological safeguards are neglected is apparent in two papers reporting work on the influence of glutamic acid on human intelligence. One of these (35) arrived at the conclusion that "glutamic acid has beneficial effects upon mental age, personality, and school achievement" on the basis of a research design so chaotic and lacking in controls as to make any conclusion impossible. The other (69), using a proper experimental design with control groups, came to the conclusion that "the results of the cognitive tests provided no evidence in favour of the hypothesis that glutamic acid improves cognitive functioning." When, even in a case where proper tests are available and research design can follow the models in any elementary textbook, differences such as these can still arise, it is not to be wondered at that in the more complex fields, where no outside criteria are available and tests of much lower reliability and no known validity are used, there should be contradictions and differences of opinion (20). Ultimately, a solution to problems in the field of personality depends more on advances in methodology than on almost any other developments.

The most impressive experiment under this heading is the Michigan study of the prediction of success in the Veterans' Administration training programme in clinical psychology (47, 48, 49). The large number of subjects studied, the very large number of techniques of measurement and assessment used, and the excellence of the experimental design would have made the findings from this programme outstanding, even if they had been less revolutionary in their import. From every point of view, the final results are a devastating comment on and criticism of the clinical methods of interviewing and projective testing current in most applied personality work. It was found that the most efficient clinical predictions in terms of both validity and economy of data are those based only on the matters contained in the credentials file and in the objective test profiles. The addition of autobiographical and projective test data appears to have contributed little or nothing to the validities of the assessment ratings. Neither the initial nor the intensive interviews made any apparent contribution. In fact, the predictions based on the credentials and objective tests are better than those made at the end of the programme on the basis of test procedures and observations! This consistent trend would seem to be all the more significant in view of the fact that assessment staff members tended to be uniformly of the opinion that the interview contributed most to their "understanding of the case," followed by either the projective test or an autobiography.

There are many other interesting and important findings [Kelly & Fiske (49)]:

The pooling of clinical judgments and the staff conference does not seem to increase the validity of predictions.

Predictions based on individual projective tests as well as those based on an integration of data from all four projective tests yielded relatively low correlations with

the rated criteria. Scores from a single objective test obtainable by mail at little cost predicted each of several criteria as well as all the clinical judgments made in the entire assessment program.

Some of the comments made by the authors of the papers quoted draw attention to the main findings. They point out that

many who have seen our results have been disturbed by the findings regarding the validity for this selection problem of specific techniques, which are felt by many professional psychologists to have a high degree of face validity (or is it faith validity)? Thus, it was the firm conviction of the staff of the OSS Assessment Program (98) that the global evaluation of a person permits much more accurate predictions of his future performance than can possibly be achieved by a more segmental approach. . . . Our own findings to date serve to raise doubts concerning the validity of this general proposition. . . . Although the unstructured interview is one of the most widely used tools in personnel selection, the writers know of no evidence in the literature to suggest that such interviews have other than extremely low validity, which hardly justifies the degree of confidence and esteem with which they are held by users of the interview.

One aspect of our findings is most disconcerting to us: the inverse relationship between the confidence of staff members at the time of making a prediction and the measured validity of that prediction.

Kelly & Fiske (49) advance what seems the most reasonable hypothesis to account for these findings.

The essence of clinical evaluation and integration of data involves permitting the clinician to assign to each item of opinion "beta weights," which vary from case to case according to the clinician's perceived patterning of the data. Our findings suggest that this technique may result in increasing the ratio of error variance to true variance with successive ratings based on increments of information. This may lead to a subjective feeling of increased knowledge about the assessee without a parallel awareness of the fact that many of the additional items of information are not actually correlated with the criteria, and hence should not be weighted in arriving at a prediction about the assessee.

These words, which would at the same time explain the singular finding of the OSS staff that a one-day assessment programme gives better predictions than a three-day programme (98), should be engraved over the portals of every psychiatric institution and psychological department that attempts to apply scientific methods to problems of personality!

Signs are not missing that the factual results of Kelly & Fiske and the frequent strictures of the more experimentally minded are succeeding in making users of projective tests and other clinical methods more conscious of weaknesses in their methodology. The number of those who believe with Harrison (36), Lersch (58) and Heiss (38, 39, 40) "that clinical validation is ample evidence for the present with respect to the validity of projective tests" is getting smaller, and the number of those who, like Schneider (85), insist that

Rorschach validating procedures can be most fruitfully treated as problems in relating Rorschach variables to independent measures of component personality processes

is increasing, as shown not only in Schneider's summary of Rorschach validation methods, but also in Mensh's paper on statistical techniques in present day psychodiagnostics (68). Other examples are the application of factor analysis to Rorschach diagnostic signs by Hughes (46) and Wittenborn (103), the attempts by Blum (4) and Goldman-Eisler (32, 33), mentioned in the previous section, to submit psychoanalytic concepts to experimental verification, and the attempts by Rosenzweig (84) and Luft (59) to make the process of hypothesis formation and verification in clinical work more explicit.

Of particular importance in this field of methodology and measurement is the use of factorial techniques and their development along novel lines. Among those following the traditional methods of Thurstone, none perhaps has so continuously and assiduously applied himself to the description of the personality sphere in factorial terms than has Cattell. The recent publication of two of his textbooks (11, 13) makes it possible to follow in detail, not only his own contribution to this field, but also the way in which he believes it to fit into a general theoretical framework. Among modifications of traditional techniques, we may perhaps mention Stephenson's "Q" technique (97) and Eysenck's method of criterion analysis (23).

Cattell's presentation is organized in terms of his factorial studies of ratings, and he brings forward much evidence from independent studies, again largely of a verbal kind, to support his contentions. From this point of view, his book is an excellent summary of the important contributions he has made to personality measurement and duplicates much of what has already appeared in his book on the *Description and Measurement of Personality* (12). He does not, however, effectively answer certain objections to his approach which have been made in the past. Thus, it has appeared to many that all descriptions of personality which are based largely on ratings, made by relatively untrained subjects, must inevitably partake of the unreliability and lack of certainty which attaches to such ratings. If it be true that a rating is almost as diagnostic of the rater as of the ratee, then what we are dealing with in our analysis is a conglomeration of rater-, ratee-, interaction-, error-, halo-, and other variances, so that it is very difficult to see how any rigid scientific structure can be built on such shifting foundations. Cattell, of course, is aware of these difficulties and has tried in a recent paper (14) to link up his rating studies with objective behaviour tests; little success, however, appears to have attached to this endeavour. It is too early yet to come to any final decision on the value of Cattell's results; what is particularly welcome in his approach, however, is the constant striving after taxonomic principles and his endeavour in doing so to use rigorous statistical methods. If the material on which he uses these methods is not always of a very high order, perhaps blame for that attaches to psychology as a whole rather than to him as an individual.

It is when he says that

observations on the development of dynamic structures in personality necessarily remain to-day still largely at a clinical level, but they are in major outlines sufficiently well established to permit us to go forward to a systematisation

that the present writer must part company with him. It is disappointing to see standards of rigour and of scientific exactitude, which are stressed in the first part, thrown away so easily and nonchalantly in the second part, and if, indeed, we are to agree with Cattell that observations at a clinical level are sufficient for the type of systematisation on which he embarks, then we may perhaps justifiably ask why we should undergo the long and difficult discipline of step-by-step scientific analysis in the demand for which he is so imperative earlier on. Nor does Cattell's actual systematisation convince that this particular solution is on any but a verbal level. While not denying the difficulty of integrating this type of material with scientific methodology, I cannot feel that Cattell's approach helps to solve this difficulty. Indeed, the systematisation of insufficient data may very easily lead the unwary to an unjustified belief in the excellence of the whole structure.

Stephenson's "Q" technique, which was introduced by him about 15 years ago under the title of "Inverted Factor Technique" (94, 95, 96) is difficult to assess for various reasons. In the first place, the practice of correlating persons instead of tests is one which was familiar to psychologists for many years already when Stephenson's articles appeared; the novelty of the procedure, therefore, can hardly reside in this particular aspect of it. Yet, when we look for other aspects of this technique, it is difficult to find them stated unambiguously and unequivocally; to be told that the "Q technique . . . is a system of devices which . . . serves to affirm conclusions already reached but requiring proof" seems neither particularly enlightening, nor, when we look at the examples provided by Stephenson, particularly true. Another difficulty lies in Stephenson's failure to deal consistently and stringently with a number of questions which require to be answered with regard to his system, such as, for instance, questions relating to sampling (both of persons and of traits) and questions relating to rotation. Presumably, the factors arrived at by means of this technique are as meaningless and lacking in invariance without rotation as are factors extracted by any other technique. Another difficulty faced by Stephenson, which he has not dealt with convincingly, is the question of a metric. Correlations can only be run over a metric continuum; can the type of data to which Stephenson applies his method be regarded as giving such a metric continuum when the summation is over people rather than over tests? It is impossible to anticipate Stephenson's replies to these questions from the few small, illustrative *ad hoc* experiments he has published; these may whet one's appetite but do very little to assuage one's doubts regarding claims such as that this technique "neatly represents the self even if in a statistical fashion," or that "it provides . . . by far the best framework into which to fit typology in general." We must be reassured on these important points, and we must also have some evidence that the factors arrived at by means of "Q" technique are unique and invariant. Finally, I think we would require evidence, which has hitherto not been forthcoming, that the factors discovered in this way differ essentially from those which would appear in an orthodox type of factor analysis, because Burt has shown rather convincingly (9) that there is

no essential difference between the two solutions and that factors from one can easily be translated into factors from the other.

The third application of factorial design to personality study is Eysenck's use of criterion analysis in an attempt to isolate the fundamental "dimensions of personality." A recent paper (21) has set forth the results of using this method in order to solve a number of theoretical and practical problems. Applications include the prediction of employability of mental defectives (successful at a remarkably high level) (100, 101), the prediction of working efficiency of nursing trainees (78), and the prediction of examination success of medical and other students (41, 44, 77). In all of these areas, the addition of objective personality tests of the kind used to define operationally the concepts of "neuroticism" and "introversion" to the more usual measures of ability improved prediction to a considerable extent.

Of more theoretical importance is the work of Petrie (74, 75, 76) on the aftereffects of lobotomy. Using as her starting-point the hypothesis that interference in neurotic patients with the frontal lobes would lead to changes along both these dimensions (towards a decrease in neuroticism and an increase in extraversion), this investigator used objective personality tests previously validated as measures of these factors. Changes of an extrachance character were observed along the lines of the hypothesis, which was therefore considered substantiated. These important studies show clearly the superiority of investigations based on cogent hypotheses over the more widely used "buck-shot approach," which consists of the unsystematic use of many varied but unrelated tests, selected without the benefit of any guiding hypothesis.

THEORY

Attention has already been drawn to certain theoretical battlefields where issues are being fought over which, in spite of their great antiquity, will presumably engage psychologists for a long time to come (1). Some of these, like the environmentalist-hereditarian dispute, are perhaps incapable of being solved in terms of those concepts which still determine our thinking. Others, such as the organismic versus analytic approach may have become appreciably nearer solution through work such as that of Kelly and his assistants at Michigan (49). In this section, however, I shall deal not so much with these very large and complex fields, but rather with the less extensive type of theory which can be handled experimentally so much better and which, in the long run, will presumably be rather more fruitful.

One of the most impressive areas of advance, during the past year has been that linking perception with personality. In part, of course, this interest in perception is simply a fashion. Where 30 years ago interest centred on autonomic measures, particularly the psychogalvanic reflex, and where later on it switched to expressive movements and the Luria type of experiment, we now have perception in its various aspects. [That these older methods have not lost all interest to psychologists is indicated by papers by Duffy (17), drawing attention to the value of the psychogalvanic reflex as

a measure of energy mobilization, and McCurdy (61), drawing attention to the correlation between "consciousness and the galvanometer."]

Concentration on one of these three major approaches to personality may have its advantages, but the student of personality should not let himself be carried away by enthusiasm for any one method of approach. This having been said, we must acknowledge that many of the ideas, hypotheses, and advances in recent years by writers whose main interest has been in perception have been ingenious and fruitful. They have led to new experiments and to the discovery of new facts, which in turn have led to new hypotheses. The whole movement certainly promises to make a very real and lasting contribution to the study of personality.

Among the publications which mark this interest, pride of place must go to the appearance in book form of the Clinical Psychology Symposium held in 1949 to 1950 at the University of Texas (3). Edited by Blake & Ramsey, this book, on *Perception: An Approach to Personality*, contains much valuable information and novel material as well as a good deal of material which is obscure, philosophical, and nonscientific. Of main interest in this context are papers by Bruner (8), Klein (50), and Frenkel-Brunswick (29), dealing respectively with "Personality Dynamics and the Process of Perceiving," "The Personal World Through Perception," and "Personality Theory and Perception."

Bruner analyses perception in terms of hypotheses: "An operational definition of *hypothesis* can be stated by reference to the specific selectivity of a given perception at a given time." The basic property of hypothesis is what he calls "strength." Three theorems are developed to deal with this concept:

- (1) The stronger a hypothesis the greater its likelihood of arousal in a given situation.
- (2) The greater the strength of a hypothesis, the less the amount of appropriate information necessary to confirm it.
- (3) The greater the strength of a hypothesis, the more the amount of inappropriate or contradictory information necessary to infirm it.

I cannot follow through the brilliant development of this theme by Bruner, which serves to integrate a large amount of experimental material; I can only say that it seems to contain the seeds for really important advances in this field.

Klein will probably be found in general agreement with Bruner's general approach, although where Bruner's stress is on general laws, Klein's is more on individual differences. This is expressed in his key concept of *Anschauung* or "attitude,"

A perceptual attitude is a personal outlook on the world, embodying in perception one of the ego's adaptive requirements. A style of reality-testing is expressed through it. It expresses a broader control principle which makes comparable demands upon other systems besides perception.

Of particular interest are the experiments which follow from this theoretical approach and which are reported in terms of what purports to be a dimen-

sional analysis, although one sadly misses the statistics appropriate to this type of analysis. However, the experiments themselves and the hypotheses expressed in them are brilliantly conceived and carried out, although there is no space here to describe them in any detail.

Frenkel-Brunswik's paper has a more psychoanalytic bias, but again the emphasis is on experimental testing and on perceptual manifestations, and this author, too, reports new methods of approach which should be of great interest to the experimentalist. From her point of view, she would perhaps regard the whole book as an indication of the tendency to shift interest from the id and the super ego towards the ego, a point also emphasised by Bronfenbrenner in a more general, but very valuable chapter, "Toward An Integrated Theory of Personality" (7).

Another area in which current interest is considerable is that of the application of learning theory to the concept of personality. Here again, instead of discussing in detail the somewhat bewildering mass of papers, we will discuss in greater detail one important publication, namely, Mowrer's book on *Learning Theory and Personality Dynamics* (70). This is, in essence, a collection of papers published over the years, but, as Gestalt psychologists might have pointed out, the whole is often less than the sum of the parts, and what may appear brilliant and suggestive in a single paper, may appear much more questionable and subject to doubt when forming part of a larger structure. Mowrer's main points are the following. In the first place, he insists that there are two basic learning processes: one of these which he calls "problem solving" occurs when a primary or secondary drive is reduced, the other which he calls "conditioning" occurs on the basis of contiguity, or double stimulation, and accounts for the acquisition of the secondary drive. In the second place, fear ("anxiety") is not conceived of as having primarily an inhibitory function but as a drive, and as such provides reinforcement through drive reduction. These conceptions are exemplified in a number of experimental papers and are applied to a large number of putative psycho-analytic processes.

In spite of the great interest which must obviously be attached to such a long sustained attempt to integrate learning theory and personality dynamics, there are certain obvious criticisms which must be made of this work. In the first place, it is interesting to see that although the first eight papers contain a monistic theory of learning, whereas the later ones adopt a dualistic approach, yet the author apparently finds it quite easy to reconcile his data in retrospect with the new theory. If that is possible, it appears somewhat difficult to take either of these two approaches very seriously. If it does not matter very much which approach is used, then presumably there is a lack of rigour somewhere which is somewhat disturbing. This lack of rigour comes out again in Mowrer's treatment of latent learning where secondary drives, in their hypothetical reduction in latent learning, are said to provide rewards which are no less potent than those provided, for example, by hunger reduction. In the absence of any independent measurement of such secondary drives, this is a typical *ad hoc* hypothesis which can do

very little to shore up this particular theoretical edifice. [Contrast with Mowrer's position, for instance, the conclusion of Thistlethwaite's recent review of latent learning (99).]

In the second place, Mowrer's explanation of the neurotic paradox appears to leave out considerations which are quite vital. (He defines the "neurotic paradox" in terms of the fact that in neurotics, actions which have predominantly unfavourable consequences persist over a period of months or years, although, according to ordinary learning theory, such non-adaptive behaviour should be dropped.) Mowrer, in setting up experiments to illustrate this paradox, falls into an error common to many animal psychologists and pointed out many years ago by Köhler (51); he structures the test situation in terms of his own perceptual and conceptual field rather than in terms of the rat's. As an example, we may quote an experiment in which the rat is taught to run from an electrified cage to a nonelectrified shelter; Mowrer shows that when the part of the cage where the rat is put down is not electrified, after a period of training, the rat will, nevertheless, run across an electrified part of the cage in order to get to the shelter. This is given as an example of paradoxical behaviour because a rat could have adjusted better by just staying in the nonelectrified part of the cage. While this is, of course, true in terms of the experimenter's perception, who knows the exact set-up of the experiment, the behaviour of the rat in terms of his knowledge and perception seems quite reasonable and nonparadoxical; indeed, a human subject put in the same position might reasonably argue, "I always get shocked in this part of the cage but I'm safe over there, so let's get the hell out of here, even if there is a momentary shock in getting to the shelter." This would be a cognitive explanation of the rat's behaviour in contradistinction to Mowrer's psychoanalytic one, and the fact that when he perceptually disrupted the perceptual field of the rat by introducing a dividing line between the electrified and the nonelectrified part of the cage, he found that this profoundly influenced the rat's behaviour, strongly supports this rival hypothesis. I have no space to discuss this criticism further, but when seen in the light of the perceptual theories discussed above, it will be seen that Mowrer has left out of account a most important hypothesis which could account for his data in many cases better than the hypothesis he offers.

I have been critical with Mowrer's book, not because it can be regarded as anything but an important contribution, but because he attempts to objectify and investigate experimental concepts which are of the utmost importance in personality theory. The highest standards of criticism should be applied to this type of work precisely because of its fundamental importance. It may be possible to link learning theory with personality and such a feat would certainly unify psychology more than almost any other advance, but Mowrer's attempt to do so suffers from weaknesses which must be expunged before it can be regarded as likely to lead in the right direction. What is true of his book is *a fortiori* true of other writers who fall short of his own ingenuity and high standard of experimental accuracy. How much

more could this book have advanced psychological insight, if, instead of republishing old papers, Mowrer could have sat down to think out the whole problem afresh and have written a new and critical exposition!

Among other interesting developments in the theory of personality is the publication by Gilbert (30) of a summary of German theories of stratification of personality. There is probably little value in attempting to abstract here the very concise version that Gilbert gives of this particular movement. Its importance is difficult to evaluate, but it does not seem to have given rise to any worth-while experimental studies, and its terms and concepts appear to be subject to the same objections that have so often been advanced against the concept of "instinct." Highly sophisticated theorisation at the semantic level seems to be designed to cover the failure to arrive at operational concepts or to submit theories to rigorous experimentation. In the 25 years that this type of theorising has been current in Germany, one would surely have expected it to produce or give rise to some modicum of experimentation, but instead, the same old concepts and words are tumbling around in the same old empty drum to the resounding echo of excessive verbalisation.

Much more in touch with current experimental work is Ross Stagner's attempt to use homeostasis as a unifying concept in personality theory (93). Taking his cue from Cannon's investigations on physiological constancy, he goes on to extend the same concept to perceptual constancy and even ego constancy. His approach can easily be integrated with that of the writers mentioned previously in connection with research on perception and personality. This type of theorising, which is in close touch with experimentation and serves to unify concepts from different fields, is of undoubted use in psychology, although the exact value of Stagner's concept can only be determined when attempts have been made to use it as a guide to further experimentation.

Neurological theorising is well represented during the past year by Krech's three papers dealing with dynamic systems as hypothetical constructs (53, 54, 55). Krech objects to current tendencies either to neglect neurology in creating hypothetical constructs, which, following MacCorquodale & Meehl (60), he contrasts with intervening variables, or to become subservient to and limited by the present unsatisfactory state of neurological knowledge. He quotes Bertalanffy (2) and Brillouin (5) to show that physics is not necessarily inimical to his attempt to deal with dynamic systems as open neurological systems, and he devotes considerable space to discussing the second law of thermodynamics. He then lays down three general specifications of dynamic systems which are concerned with the pattern of activities, the open system characteristics, and the locus of dynamic systems. These rules are too general to have any direct specific application, nor does it seem possible to derive deductions from them which could be tested directly so as to support or contradict them; however, they may serve to suggest methods of approach which are somewhat different from those currently explored by physiological psychologists, and, no doubt, future

papers will make this system more relevant to the direct experimental attack. All that can be said at the moment is that Krech's attempt, coinciding as it does with the work of Köhler & Wallach (52) and Hebb (37), may serve to rejuvenate this part of psychology which had begun to show signs of arteriosclerosis, if not senile dementia.

It may perhaps be said in summary of this section that theoretical work is valuable in so far as it pulls together experimental researches and points out similarities and correlations that might otherwise be overlooked. When it is a question of rather grandiose analogies, *ad hoc* hypotheses, and intuition, theorising can be harmful by distracting attention from soluble problems to philosophical speculation. Current publications contain examples of both and attention has been drawn to examples which illustrate either approach.

SUMMARIES AND TEXTBOOKS

There are many chapters relevant to the psychology of personality in *Recent Progress in Psychiatry* (28), particularly those on psychiatric genetics (92), physiological psychology (34), intelligence testing (6), and personality tests (24). Cattell's textbooks, already mentioned, summarize a good deal of the literature (11, 12, 13). Much of what is printed in *Current Trends in the Relation between Psychology and Medicine* is also relevant in this context (16). Summaries on particular problems in the field of personality appear in Blake & Ramsey's book on *Perception: An Approach to Personality* (3) already discussed. With respect to the problem of anxiety, there is a book of that title by Hoch & Zubin (45), as well as a more historically oriented one by May (67). A summary of the much neglected theories of Janet is given in a fairly clear exposition by Schwartz (87).

Certain aspects of personality, such as, for instance, those associated with aggressiveness, are dealt with in a recent symposium on *The Psychological Factors of Peace and War* (25, 42), while other social aspects of personality are touched on in a publication devoted to papers read at the University of Oklahoma Conference on *Social Psychology at the Crossroads* (83). In a more limited field, there is an excellent review by Crown on the aftereffects of prefrontal lobotomy (15), which lays particular stress on methodology in its discussion of published work.

Much interesting material on psychology and personality along lines radically different from ours is given by Wortis (104) and by McLeish (63) in their writings on Soviet psychology and psychiatry. The book by Wortis is particularly valuable, not so much for its comments, but for the translations of reports, directives, and criticisms emanating from Russian sources. These paint a somewhat horrifying picture of state interference in scientific matters, deliberate misrepresentation of Western points of view, complete ignorance of advances made during the past 30 years, and an intermingling of politics and science, which, while it is no doubt intentional and in line with Marxist thought, does not seem to be favourable to the production of original work in this field.

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