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### Cattell and the Theory of Personality

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Among theorists of personality, Cattell occupies an outstanding position. Most handbooks and textbooks rightly select him as the representative of a line of research and theory which is identified with correlational methods, factor analysis, and multivariate approaches generally (e.g. Hall & Lindzey, 1970). Other writers, such as Guilford or the present author are also sometimes mentioned in this connection, but for most psychologists the multivariate, factor analytic approach is identified with R. B. Cattell. Such an identification of an author with a method, whether of analysis, design, or a particular type of apparatus or test, has advantages and disadvantages. In so far as the method. design or apparatus makes a genuine contribution to science, the scientist identified with it benefits. On the other hand, in so far as the method, design or apparatus has faults and imperfections, leads to partial rather than complete answers, and can be criticized on rational grounds, the scientist identified with it falls prey to these criticisms also. In a book like this it would not be desirable to praise Cattell for his major contribution to the factorial approach, without indicating also such weaknesses as this approach may hold; such weaknesses invariably will be seen also to act as criticisms of Cattell himself, but in reality are merely the other side of the coin.

All factor analytic theories share in common a belief in some form of generality of conduct; this is opposed to the theories of "specificity," as advocated by Thorndike around the turn of the century, or of "situationalism," as advocated nowadays by Mischel (1968). Thorndike (1903) put this view quite clearly when he announced his belief "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 behavior and stability of personality, but only independent and specific stimulus-response bonds or habits." The empirical evidence overwhelmingly contradicts this view (Cattell & Dreger. 1977; Eysenck, 1970; 1976), and demonstrates that broad, general traits of personality do exist, and are associated with general and consistent forms of conduct; the denial of consistency of behavior and stability of personality might have been permissible in Thorndike's time (when little empirical evidence was available), but it makes no sense at all nowadays when the evidence is literally overwhelming. Cattell's work has materially contributed to this evidential refutation

of the "specificity" hypothesis, and the associated "situationalism" of Mischel (Cattell, 1966a; Cattell, Eber, & Tatsuoka, 1970; see also Guilford et al., 1976).

Mischel's criticisms, of course, do have a proper target, and it is interesting to contrast this target with the factor analytic opus. Much modern work seems to verify Mischel's complaint that personality variables, in predicting conduct (either in the laboratory or in everyday life), contribute very little to the variance. This is undoubtedly true, but it cannot be used to discount personality research as such. We must look at the weaknesses of much of what is done in the name of personality research, in order to find an explanation of the general run of the findings. Let us note, then, that in most cases the investigator starts out with no proper theory or hypothesis, deduced from a large body of ascertained knowledge; if hypotheses are framed, these are either of the "null" variety (which is merely a device for pretending that an hypothesis is being tested when in reality no hypothesis exists), or they are ad hoc, common sense fabrications, often issuing from Freudian speculations or other "soft" backgrounds. The instruments for measuring personality are often multi-purpose, multi-barrel shotguns constructed on an arbitrary, subjective basis without benefit of factorial investigation; the MMPI and the CPI are only two examples of this kind of test. Having usually something like 20 scales, these instruments almost guarantee that the investigator will find at least one "significant" correlation-even though this will most likely have arisen by chance! In this wilderness of near-zero correlations, which should be expected when there are no rational predictions linking most of the scales with the criterion, the few "true" correlations easily get lost, and the conclusion is erroneously arrived at that the average predictive accuracy of personality variables is minimal.

Such almost random methods of testing non-existing hypotheses with arbitrarily derived scales should be contrasted sharply with the application of properly constructed factored scales, embodying replicable personality factors, to criteria where proper predictions can be made. Mischel fails to make this vital distinction, and seems to concentrate on the scientifically least valuable aspects of personality research, aspects which Cattell (1946a, 1950a) has always vigorously criticized, and would reject totally as unrepresentative of proper scientific methodology. Criticisms of personality constructs, such as Mischel attempts to present, should deal with the strongest, not the weakest examples of modern research in this field in order to be convincing; it is no excuse to say that most research in this field is poor and subject to many strictures. This is true but irrelevant; the faulty

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application of poor measures to non-existent theories is unfortunately an all-too-frequent fact of psychological life, but it does not serve to invalidate the proper application of factored measures to relevant psychological theories.

The postulation of "broad, general traits of personality" implies some sort of statistical, factor analytic approach; broad generality can only be demonstrated by correlational analysis of logically separate but empirically related behaviours, whether shown in life conduct, laboratory investigation, or questionnaire response (Dreger, 1972). The notion of correlation has become anathema to many experimental psychologists who feel that, as Thurstone once said, "a correlation coefficient is a confession of ignorance." This disregards the fact that while a correlation does not immediately reveal to us the secrets of causation, it nevertheless sets us on the way to overcome our ignorance, and suggests alternative causal theories which might then be tested. It is not often realized by psychologists that physics too relies heavily on correlations, as does astronomy. Hubble's work formed a vital beginning to modern cosmological theory, yet it was based on the discovery of a simple (rather low!) correlation (Hoyle, 1975). As long as correlations are not overinterpreted they are a vital tool for the discovery of generality in a given field, particularly when it is difficult or impossible to control all disturbing variables in that field.

Cattell has often been criticized for accepting rather low correlations, e.g. in determining the internal reliability of his scales; he has ingeniously turned this criticism around, and uses it to attack his critics. Internal reliability, he remarks, is merely evidence of redundancy (Cattell & Tsujioka, 1964); ideally we should look for items in a questionnaire which have maximum validity and minimum inter-item correlation! As an example, he constructs two "scales" made up of two items, both having validities (factor loadings) of .707. In one scale the items correlate perfectly, i.e. the scale has a reliability of 1.00. In the other case, the items are completely uncorrelated, i.e. the reliability of the scale is zero. Yet it is the latter scale which has perfect validity, i.e. correlates perfectly with the factor, while the validity of the other scale is only half of a perfect correlation, i.e. 707. The reason of course is that in the less valid scale one item is entirely redundant, while in the more valid scale both items contribute independently to the validity of the whole scale.

This demonstration constitutes a vital criticism of modern test theory; yet typically it has not been accepted into that theory, and few text books take the trouble to examine the argument. It might be thought that the example is an artificial one, and that no actual

demonstrations could be found. This is not so. Take a hypothetical factor of "ball playing ability." A questionnaire might be constructed having two items: "Do you play football (soccer)", and: "Do you play rugby?" A high scorer on the factor should perhaps be expected to answer both questions in the affirmative, but English schools usually only play one game or the other, and even when both are played the average boy has to choose because the games are played at the same time. This would seem to produce a negative correlation between the two items; it does not do so because (a) the worst ball players play neither, in fact, but do some running, or jumping, and (b) the best ball players play outside the school, in local teams. The overall correlation is thus just about zero, but the two items contribute independent variance to the total score. Another example. Anger and anxiety are two alternative manifestations of neuroticism (the factor), shown by extraverts and introverts respectively (Siprelle et al., 1977); thus the two show a low correlation, but have high loadings on N! These psychological possibilities have been discussed by Frenkel-Brunswick (1942) under the heading of "alternative manifestations;" they constitute an important but much neglected aspect of personality.

Does all this justify Cattell's tolerance for very low internal reliabilities in his personality scales? Here one may be more doubtful. A low correlation between items may be due to the existence of alternative manifestations of the trait being measured, but this should not be assumed—it must be demonstrated. Low item intercorrelations may simply mean what they say, namely that there is very little in common to the items, and that they do not cohere together sufficiently to produce a viable factor concept. Cattell's argument bears witness to his originality, his mastery of psychometrics, and his fertile imagination, but it does not necessarily settle the question of low internal reliability of his scales (Saville & Blinkhorn, 1976).

Factor analysis is an essential tool in the discovery of the descriptive variables we employ in research on personality (traits, attitudes, aptitudes); this leaves open two further questions. Does Cattell, in the way he uses this tool, optimize its possibilities? Is factor analysis sufficient to answer the kinds of question which personality theory must answer? On both issues it is possible to raise objections to the manner in which Cattell has proceeded, or to argue that possibly other methods and approaches may be more fruitful. To say this is merely to state that all scientific methodologies have strengths and weaknesses, and that merely to look at the one or the other gives a one-sided impression. To take but one example, consider the multivariate use of laboratory investigations which has formed such an interesting aspect

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of Cattell's work (Cattell & Warburton, 1967). In looking at such usage, we are faced with two alternative strategies; both have their advantages and their disadvantages. Cattell typically administers dozens of different laboratory tests (conditioning, suggestibility, psychophysiological), intercorrelates them, and then extracts factors which he then tries to associate with the original factors obtained from questionnaires or life situations. This is one approach, but it clearly has many problems associated with it.

The most intimidating difficulty is that for any laboratory test there are many different parameter choices that have to be made, and the outcome depends crucially on the correct choice. To take one or two examples from our own work, we have predicted (and shown) that in eye-blink conditioning the strength of the UCS is absolutely crucial in determining the correlation between personality (introversion) and effectiveness of conditioning (Eysenck, 1976). With weak UCS the correlation is positive, with strong UCS it is negative. Similarly, CS-UCS interval length, degree of partial reinforcement, and other variables influence the outcome, as does the degree of anxiety aroused by the presentation of the test, and the amount of reassurance given. Unless these variables are controlled, the correlations discovered with personality variables can be almost anything; the parameters in question determine crucially the relationships that emerge with personality.

This dependence of correlations with personality on suitable parameter values is not confined to conditioning, but is a universal phenomenon; it emerges in relation to EEG alpha rhythm frequency and amplitude, cortical augmenting-reducing, salivary response to lemon juice, sensory thresholds as a function of sensory stimulation, drug effects, verbal learning, memory and retrieval, perceptual phenomena, and many more. It is not merely a question of obtaining correlations that are a little higher or lower; the very direction of the correlation is involved. Thus Howarth and Eysenck (1968) have shown in an experiment involving paired associate learning that retrieval correlated *positively* with extraversion if the period elapsing between learning and retrieval was short, *negatively* if it was long. All these phenomena are lawful, and can be (and have been) predicted on the basis of classical arousal theory; they also illustrate the fact that curvilinear regressions of response on stimulus are the rule, rather than the exception, a fact which makes attention to parameter values all the more important. Some fifty studies, illustrating these points, have been brought together elsewhere (Eysenck, 1976); they would seem to make the point quite strongly that parameter values must be

chosen carefully, and preferably in terms of an existing theory, and that correlations obtained under conditions where parameter values are chosen arbitrarily are essentially uninterpretable.

Accompanying this advantage is of course a complementary disadvantage. Laboratory experiments of this kind are essentially unsuitable for multivariate investigations, at least until all the problems mentioned above have been clarified. But these problems can only be clarified by working along theoretical lines, using a particular personality variable (extraversion-introversion in the examples given). That means essentially a univariate approach, at least in the initial stages of research; using the information provided by these univariate studies, and when sufficient evidence is available, we may mount a proper multivariate study with a reasonable hope that our choice of parameter values may enable us to construct a set of experimental designs which will give meaningful and congruent results. But of course by that time the major problem has already been solved, and the relevant theory substantiated; what remains is merely a mopping-up operation.

The disadvantage of this univariate approach of course is that it is essentially very time-consuming; that it proceeds in a step-wise manner in which we cannot anticipate the later stages by a flash of insight; and that if our theoretical expectations are falsified we are left with nothing. The multivariate approach, by using many different concepts and designs, is much more likely to throw up interesting findings, even if only by lucky chance, and later work may be able to build on these foundations. My own reading of the experimental literature would suggest that on the whole the outcome of the multivariate approach has not been entirely successful, but that replicable results, indicating close relationship between variable and personality, have been few and far between, and that little theoretical clarification has rewarded the experimenters for the very onerous outlay of money, time and energy required in all such multivariate studies. However, clearly it represents an alternative to the univariate approach I am personally advocating, and as such the advantages and disadvantages of both approaches should be more widely discussed than they are at the moment (Eysenck, 1977).

It is indeed one of the sadder facts of life that handbooks and textbooks of personality seem quite unconcerned with the serious discussion of vitally important problems of research design, such as the one under discussion. Different "systems" are presented in different chapters, rather like a history of art might have a chapter on the pointillistes, followed by one on the cubists; there is little concern with the vital question of proof and disproof, verification and falsification, and none with the different methodologies available for testing these systems. Cattell is one of the few exceptions to this rule; in his writings he has always emphasized the importance of methodological problems and solutions (Cattell, 1966a), and it is for this reason that this point has been discussed here at some length. Clearly problems of this kind have no ideal solution; experience, conditions, facilities, training and the promptings of the Zeitgeist will suggest to each investigator what at a given time may be, for him, the best procedure. There are no right or wrong answers to questions of this kind; however, it is desirable that they should be aired in public debate, and that choices should be made knowingly, rather than in ignorance.

Much the same could be said with regard to another choice on which also Cattell and I have disagreed. By and large, Cattell has been opposed to the current theories emerging from experimental laboratories, and has openly criticised "brass-instruments" psychology. As a consequence his own work has little relation to the concepts which concern the classical experimentalists, or the psychophysiologist. In spite of the many differences in outlook, he seems to prefer even the speculations of the psychoanalysts to the concepts of the learning theorists (Cattell, 1980h). It is of course not uncommon for personality theorists to proceed in this manner, and Cronbach's (1957) famous Presidential Address to the A.P.A. on the two disciplines of scientific psychology drew attention to this separation of the correlational study of individual differences and the experimental study of controlled effects. Cronbach strongly emphasized the point, which I believe cannot be stressed too strongly, that one without the other cannot flourish, and that both are dependent on the other. This general orientation would seem to suggest that the concepts of the personality theorists should be the same as those which emerge from the experimental study, or the psychophysiological study, of laboratory phenomena. The success of lining up the personality dimension of introversionextraversion with the experimental concept of "arousal" has shown the possibilities inherent in this type of integration (Eysenck, 1967; 1981a).

This whole problem has a long history, which is often misunderstood. Boring (1950), in his *History of Experimental Psychology*, tells the story of James McKeen Cattell, who brought his own research problem of "individual differences" to Wundt, who was accustomed to assigning research problems to students arbitrarily. Wundt is quoted as having said: "Ganz Amerikanisch" on this occasion, and Boring adds: "—as indeed the problem has turned out to be." (p. 324.) This remark has been the source of much misinterpretation. It has given

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rise to the completely erroneous idea that prior to Cattell psychology was concerned only with general laws, and that he introduced the notion of individual differences. Nothing could be further from the truth. Experimental psychology started from observations of individual differences in reaction times, and the whole German tradition of Wundt, Kraepelin, Müller and others insisted on the integration of individual differences and general laws, very much along the lines I have advocated above. This insistence on seeing both sides of the coin is also very clear in Pavlov; throughout his book he is concerned with individual dogs, hardly ever with averages, and he constantly refers to the "personality" of his dogs, and their daily moods, as explaining aberrant results. This approach is typical of the German school also; Eysenck & Frith (1977) have documented this point in some detail. What might be considered "ganz Amerikanisch" is therefore not the notion of taking individual differences seriously, or studying them scientifically; it was perhaps rather the notion of studying them as something separate from the general thrust of experimental work. This breaking up the symbiotic relationship between the two "scientific disciplines of psychology" Cronbach talks about has indeed been the contribution of American psychology; it is not surprising that Wundt was less than enchanted with the prospect! We now have to undo the evil that this separation has caused, and attempt to reunify a science so needlessly divided into two.

This combined approach would seem to have many advantages. In the first place, it supplies our experimental investigations with an invaluable background of factual knowledge and theoretical sophistication. Work on conditioning, memory, learning, EEG contingent negative variation, cortical augmenting-reducing, and many other topics could not have proceeded along proper parametric study lines had the necessary knowledge not already been available from previous laboratory studies of arousal and its effects on these variables. In the second place it ensures that personality theorists and the experimentalists speak the same language—an essential preliminary if we are ever to get together and try to construct a single discipline of scientific psychology. And in the third place this procedure allows us to do more than be parasitic on the experimentalists; we can contribute greatly to their endeavours, and help solve some of their problems (Eysenck, 1981a). A single example will be cited to make clear what is involved, and why the liaison between personality study and experimental laboratory work must be so close.

According to arousal theory, the effect of stimulation of one sensory system on the measurement of sensory thresholds in another

system should be to lower these thresholds; such stimulation should increase arousal, and increased arousal should serve to lower thresholds. The experimental literature, as so often, is contradictory-some authors find the predicted phenomenon, some find no effect, and some find exactly the opposite of the predicted phenomenon; Shigehisa and Symons (1973) have suggested that the reason for this unhappy state of affairs may be twofold. In the first place, the relation may be curvilinear, embodying the usual inverted-U relationship between stimulus and response. And secondly, they suggested that the reversal in this relationship should occur earlier (i.e. at lower stimulus values) in the introvert than in the ambivert, and earlier in the ambivert than in the extravert. Measuring auditory thresholds, and varying the illumination along a ten-step scale, they succeeded in verifying both hypotheses; replication gave identical results, as did a variation in which visual thresholds were measured under changing auditory stimulation (Shigehisa et al., 1973). Thus reference to personality variables like introversion-extraversion may clear up problems in the experimental analysis of perceptual and other phenomena which threatened to hold back seriously the development of a proper theoretical understanding of the field in question. In this way does personality study repay the debt it owes experimental psychology for furnishing it with concepts and paradigms for its investigations and theorizing.

Again, it should be said that this question of relying on concepts from experimental and academic psychology to furnish us with theories to use in our empirical work on the foundations of personality cannnot in the nature of things receive an unambiguous positive or negative answer. Those who believe with Cattell that it is futile to take our concepts from another discipline, and that we should elaborate our own when dealing with individual differences, may reasonably point to the changes that are taking place in the concepts most favoured by experimentalists, the divisions between them with respect to the concepts to be adopted, and the criticisms advanced against most concepts at all widely used. How, it may be asked, can we reasonably be expected to make a choice when the very group which has elaborated the concepts in question cannot seem to agree on that choice? How can we be guaranteed that once we have made such a choice, experimentalists will not turn around and throw out lock, stock and barrel the very concepts we have borrowed from them in the first place-as happened so recently with the Hullian set of theories!

The obvious answer is that no guarantees are possible in science; all is movement, change, and (hopefully) advance. Earlier notions are improved and their positive aspects taken over into new ones. There is no safety, no secure anchorage, no everlasting resting place here; a satisfactory choice depends on our knowledge, our training, and our serendipity. Many will agree with Cattell that it is more satisfactory to leave experimentalists alone to fight their own battles, and to work out one's own concepts oneself. For the reasons given by Cronbach, and for those outlined above, I would not agree to such a course, but again the important thing is that those who wish to work in the personality field should not only be free to make their choice, but should be aware of the arguments and alternatives involved. It is unfortunate that such arguments are seldom found in the current literature, and that the alternatives are hardly ever set out properly for the investigator to make a proper choice.

Taking together the points made on the last few pages, one might argue that essentially Cattell believes much more strongly than I do that multivariate methods, and particularly factor analysis, are in a position to give us reasonably definitive answers to our questions. My own position would be, rather, that factor analysis can be a useful tool, but that it requires theoretical models, concepts, and experimental techniques quite outside its circle in order to integrate its findings with general psychology, make them universally acceptable, and in particular get us out of the infinite regress involved in purely correlational, factor analytic work. Each factor "discovered" in the personality field is positioned slightly differently from every other in the multidimensional universe constituted by the interrelations of all possible questionnaire items, life history events, or whatever may be our data base. No two factors are ever identical, and choices are ultimately subjective. This subjectivity can only be abolished by stepping outside the circle of correlations and factors, and aligning the items and factors with some outside criterion, of theoretical importance, along the lines of bivariate experimentation. There cannot, in the nature of things, be any statistical criteria for factor rotation which might guarantee us a psychologically meaningful and "optimal" solution; in its absence integration of our findings with general psychology seems the only meaningful alternative.

Some of the issues discussed have direct relevance to problems within factor analysis. The most important of these problems is perhaps the choice between primary factors and superfactors (secondorder factors). American authors have laid most emphasis (and in the case of Guilford all emphasis—Guilford et al., 1976) on so-called *primary* factors, often using varimax rotation in order to force factors into independence (and Procrustes to force them into agreement with theory!). English authors have preferred oblique solutions (e.g. Promax rotation), with most emphasis laid on the higher order factors extracted. Cattell of course was one of the pioneers of oblique factor rotation, and he does extract higher-order factors, but the main thrust of his work relates to the 16 PF, i.e. primary factors. His argument is that these primary factors contain far more of the criterion-relevant variance than do the super factors; Eysenck (1971, 1972) has shown, by reanalysing some of Cattell's own work, that this is not in fact so, and that the super factors contain practically all the "true" variance contributed by the primaries. There has been very little debate on this issue in the technical literature; most people seem to assume that their solution is the correct one, without exposing themselves to rational rebuttal.

At first sight it might seem that a set of primaries, containing say n scales, must have superior predictive accuracy for any criterion as compared with a set of *m* superfactors extracted from the intercorrelations between the n primaries, where m < n. Each primary contains, in addition to variance it holds in common with other primaries, and which goes to define the superfactor, specific variance not shared with any other primary; these n specific variances, in so far as they are correlated with the criterion, must add to the shared variance incorporated in the superfactors. In theory this is no doubt true, but in practice we must write a prediction formula which gives specific weights to each primary; unless these weights are very stable from situation to situation, and from time to time, the contribution made by the specific variances of the *n* primaries may be negative as often as positive. Usually there is not sufficient psychometric evidence available to write very accurate weights, and the weights are determined by common sense, experience, or some other subjective consideration. Under these conditions also it must be very doubtful whether the contribution of primaries is really likely to make prediction more accurate than it would be if we concentrated on that made on the basis of shared variance, i.e. by reliance on superfactors only.

In spite of the importance of the issue, from both the theoretical and the practical points of view, there has been very little work done to throw light on the problem. A recent study by Reynolds and Nichols (1977) will demonstrate the essential validity of these criticisms of primaries as contributing much to the predictive accuracy of superfactors; the study was done on such a large scale that its conclusions are particularly impressive. The authors used 861 male and female National Merit Scholarship participants who were administered the CPI scales, 16 of which were included in the statistical analysis. "In particular, the study was designed to discover whether the common

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factor variance of the CPI scales carries the valid scale variance or whether the unique scale variance contains the information critical to scale validity." (p. 907). The common factor variance, as Nichols and Schnell (1963) have shown, is represented by two major superfactors which Reynolds and Nichols identify as *Adjustment* and *Extraversion*; the similarity to Cattell's superfactors of *Anxiety* and *Exvia*, or to my own *Neuroticism* and *Extraversion*, is obvious.

The authors go on to ask how well the two factor scales represent the information in the CPI profile. As they point out, the variance in the original CPI scales that is predictable from a linear combination of the two factor scales ranges from about 10% to 79%; the question they ask is, "is the useful information in the scale contained mainly in this common factor portion of the variance, or is it contained mainly in the unique portion of the variance?" (p. 908). To investigate this question, 178 criterion variables were selected from the files, and correlated with raw, predicted, and deviation CPI scale scores. The result of the study was that "for the most part, the factor scales do seem to capture the valid variance in the CPI scales. . . . In many instances the common factor portion of a scale's variance was actually more predictive of relevant criteria than was the total scale variance. These findings would suggest that users of the CPI might be better off to measure and interpret the two principal factors rather than attempt to derive meaning from a complex profile of scores." (p. 914).

The final conclusion, namely that "very little information is lost in the course of substituting the parsimonious measures of the principal factors for the many individual CPI scores" (p. 914), is exactly the point raised in connection with the common American practice of using multifactorial scales. Admittedly the scales of the CPI are not factored scales, like Cattell's 16 PF, but they are essentially similar in content and derivation to factored scales in the literature, and there is no reason to assume that what is true of the CPI is not true also of the 16 PF. As Reynolds and Nichols say, "similar studies of other multiphasic tests should now be undertaken to determine the generalizability of these findings to test batteries in general." (p. 114). Our point here is not to prejudge the issue, but to suggest again that the common custom of ignoring the issue may be mistaken, and that rational discussion and further empirical work are needed to come to any sensible and scientifically valid conclusion.

We see that there are many different lines along which research into personality can be carried on, even when researchers are agreed on such major issues as the existence and importance of broad and general traits of personality, the relevance of multivariate studies,

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primarily through some form of factor analysis, and the importance of objectivity in the ascertainment of data. Thus Cattell starts from a reasonably random sample of traits, proceeds to use factor analysis for the generation of hypotheses about the major factors involved, stays largely within the universe of factor analysis in his elaboration of his model, and assigns only relatively low importance to the fact that his primary factors are intercorrelated, and give rise to superfactors, such as anxiety and exvia. At each stage I followed exactly the opposite line. Starting out with a theoretical model which goes back at least as far as Wundt, Gross, and Heymans (and in some form as far back as Galen), I used factor analysis to test theories rather than to originate them. I tried to use theoretical concepts from general psychology and physiology to link the factors in question with causal hypotheses which led outside factor analysis altogether. And last but not least, the factors in question are not primaries in Cattell's sense, but rather superfactors. (I prefer the term to "second order factors", because the position of a factor in a hierarchy is determined by many influences, notably the selection of items or tests correlated; in some matrices a "second order factor" may indeed emerge from the intercorrelations of primaries, but in another matrix it might emerge as a primary, or a third order factor, etc.) I have suggested in this paper that the reasons for each of these divergences are not arbitrary and subjective, but are based on background reasoning which can be made explicit, and which deserves to be debated seriously by research workers in this field. It is not suggested that Cattell's choices are wrong, and mine right; it is suggested that both sets of choices have advantages and disadvantages, and that serious discussion of these respective advantages and disadvantages might be extremely useful for the further development of the field.

In view of these many differences in design and analysis, it might be thought that the results obtained by Cattell and myself would be very different. In fact of course it has often been pointed out that the results are surprisingly similar; indeed, when the proper comparisons are made this similarity becomes all the more apparent. On the "pure" personality side Cattell's superfactors of anxiety and exvia, as has already been said, duplicate very closely my own factors of neuroticism and extraversion. Cattell's factor "superego strength" seems in many ways the opposite of my psychoticism, and indeed correlates negatively with it, to quite a marked degree (Eysenck & Eysenck, 1976). Cattell's conservatism corresponds quite reasonably to the similarly named factors I found in my study of "The Psychology of Politics" (Eysenck, 1954), and even my "tendermindedness" may correspond to one of Cattell's 16 PF. Cattell and I both acknowledge the importance of

#### H. J. Eysenck

general intelligence, although Cattell includes it in his 16 PF, while I do not include it in my personality scales. In other words, the major conclusions are surprisingly alike; the only remaining difference is that Cattell attaches more importance than I do to his primary factors (more specifically, to the unique variance contributed by these primaries, over and above the common variance which they contribute to the superfactors). This is not an important difference, and it really becomes an empirical question whether very much is gained by scoring 16 PF rather than a much smaller number of superfactors. This is a surprising conclusion, in many ways. Given the almost universal impression that personality theorists and researchers come to conclusions so widely varied and so irreconcilable that no science of personality is possible, an impression furthered to a considerable extent by the habit, started by Hall and Lindzey, of giving separate chapters to different authors, without indicating the scientific value attaching to their contributions, and without indicating the decisive contrary evidence available in so many cases, it is unusual to discover such close correspondence between authors so distinct in their methods, procedures, evaluations and premises.

I feel that this agreement, over differential approaches, is of considerable importance, and bodes well for the future study of personality. Critics have often emphasized the differences in approach between Cattell and myself, and have argued that the existence of such differences invalidates the conclusions. This is clearly not so. There are differences in approach in all sciences; these relate often more to individual preferences and choices of method than to differential conclusions. When conclusions are so similar, in spite of differential approaches, much more confidence may be reposed in their (approximate) correctness than would be the case if they depended crucially on a particular approach. The Cattell and Eysenck constructs and theories should be seen, not as mutually contradictory, but as complementary and mutually supportive. When we take this point of view, we are more likely to evaluate correctly the contribution which Cattell has made to the scientific study of personality.