book to cognition (well, "cognitive behaviour" actually). It may, therefore, be intended as a monograph arguing for a consistency principle as a major interpretative tool, in which case it is very disappointing. It has very little that is new to add to the available arguments for this viewpoint and although it is written extremely clearly and coherently its choice of topics and material is essentially eccentric. In addition it contains a number of errors of fact, a few misrepresentations and some other inaccuracies. For example, on p. 73 the fundamental postulate of Kelly's Personal Construct Theory is wrongly defined; on p. 94 Byrne is wrongly claimed to be a similarity theorist and although subsequent qualifications are added the inexpert reader is likely to be confused by the original misrepresentation; theories of attraction are divided into two types, neatly omitting about six major theories that do not fit either classification, particularly those of Murstein, Levinger, Lewis and Wright. Further detraction from the book's authority and credibility are instances of contradiction and naivety like the claim that the status of Cognitive Dissonance theory has been seriously affected by criticism. The point (and a very interesting one, surely, for someone interested in consistency) is that although Cognitive Dissonance theory has been most coruscatingly criticized its status is unaffected (witness the six pages of discussion given to it in the book under review).

On the positive side, the book is very clearly written and easy to read although on occasions it claims to be introducing something whilst it assumes a considerable degree of knowledge. Thus on p. 15 the statement is made that Kelly's theory "provides a method (the repertory grid) by which a person's construct system can be investigated". The method is not explained at this point or any other and the notion of a construct system has not been discussed either. Equally, work on ethnomethodology is early presented as an alternative to social psychological methods which are themselves not explained—or even described. What can an introductory reader get out of such material? Yet the style is intensely introductory.

In summary, this is a book that is stylishly written and easy to read but which never provides a satisfactory answer to the question of why it *should* be read. In view of the clarity and skilfulness of the writing I regretfully conclude that the author's evident talents have been wasted on a white elephant.

STEVE DUCK

K. PAWLIK (Ed.): *Multivariate Persönlichkeitsforschung*. Verlag Hans Huber, Bern (1982). 382 pp. DM 73.

Kurt Pawlik is a former student of Cattell's, and many aspects of this book bear the imprint of Cattell's views and attitudes. The book contains a number of quite separate contributions by eight co-workers or students of Pawlik's, as well as the editor himself. Some of these deal with discussions of the methods of multivariate analysis, while the majority apply these to a variety of problems, including learning and practice, dimensions of convergent, divergent and 'social' intelligence, the multivariate study of the relationship between intelligence, creativity and personality, an important chapter on the structure of intelligence, a chapter on the relationship between handwriting and temperament, factor analytic studies of the psychology of interests, and the dimensional study of psychological disorders, using Lorr's IMPS in its German translation.

The reviewer is less certain of the importance of multivariate analyses and the objectivity of the results thereby obtained than are Cattell and Pawlik, but clearly these studies are of considerable interest, and ought to be familiar to those concerned with individual differences. Of particular interest perhaps is the study of the hierarchical structure of intelligence, resulting in eight factors, which bear some close resemblance to the factors isolated previously by Royce. Curiously enough Pawlik does not go into the question that will occur to anyone familiar with the literature, namely what has happened to the general factor? Nor does Pawlik really answer the question of why his solution differs so much from Cattell's advocacy of fluid and crystallized intelligence.

There is a curious atheoretical air about the whole book; interest throughout seems to be in descriptive statistics rather than in causality, which is fair enough when only multivariate methods of analysis are being used. However there is little discussion of the limitations of such methods and the need for more directly experimental and causal analyses and experiments. This makes the book more acceptable to followers of Cattell than to experimental psychologists; nevertheless, it is clearly a considerable contribution to the various areas involved, and should be read by anyone interested in individual differences.

H. J. EYSENCK

R. B. CATTELL: *The Inheritance of Personality and Ability*. Academic Press, London (1982). xxi + 449 pp. $47.50.

If Raymond Bernard Cattell didn't actually exist, he probably couldn't be invented. R. B. Cattell, Distinguished Research Professor Emeritus of Psychology, University of Illinois, has been publishing psychological articles and books at a prodigious and unslackening rate for more than half a century. To those who are familiar with Cattell's work, he is a unique phenomenon, in the same sense that the individual creative character of every great composer is immediately recognizable in each of his works. Cattell's articles, and especially his books, it seems could not have been produced by anyone else. This can be said of only a handful of the 'greats' in the history of psychology. Whatever mixed and varied reactions may be expressed by readers who have delved studiously into Cattell's major works, none escapes the realization of exposure to an extraordinarily rich and complex intellect. But what may be a heady adventure to some psychologists may prove a travail to others. This seems inevitable for any kind of complexity on a grand scale. We are confronted here by nothing less than a whole conception of psychology—it might be added, a thoroughly 20th century, scientific conception. But also, Cattell's conception of psychology could almost be called Wagnerian—in its utter size, its heroically impracticable ambition, its grand design, its macrocosmic expanse and macrocosmic complexity, its recurring elaboration of numerous 'leitmotifs',

its peaks of inspiration, and yes, also like Wagner, its occasional stretches of tedium. In the opinions of most students of Cattell, the four grand opuses of the Cattellian 'Ring' would probably be *Abilities—Their Structure, Growth, and Action* (1971), Volumes I and II of *Personality and Learning Theory* (1979, 1980), and his latest book, *The Inheritance of Personality and Ability* (1982), the subject of this review. As in the case of Wagner's Ring, so too with Cattell's, there is the risk that many of the uninitiated (in factor analysis, quantitative genetics, and Cattellian terminology) confronting the present work will find it onerous to sustain the required level of concentration through thick and thin from beginning to end, and will lose patience. It is their loss, of course, because, in fact, careful study of Cattell is intellectually and scientifically rewarding. But one may also sympathize with those who do not already bring a considerable background of expertise to the task, for Cattell, unfortunately, makes few concessions to didactic simplicity. Mercifully, the book contains a good glossary of specialized terms, and Cattell tries, when feasible, to explain quantitative formulations verbally, in addition to the mathematical notation. Also, the excellent chapter summaries, being less discursive than the main body of the text, will provide a welcome consolidation for most readers. Hence the book's main source of difficulty stems more from its complex conceptual level, rather than from its technical aspects *per se*.

This is the only methodologically comprehensive book on behavioral genetics I know of which is addressed exclusively to the study of *human* variation, and hence it fills a conspicuous need in differential psychology. It is primarily concerned with explicating various methodologies for analyzing the relative contributions of genetic and environmental factors (and their covariance and interaction) to individual differences in mental abilities and personality traits. The results of the application of some (but not all) of these methods to measurements of ability and personality are reviewed in the last three of the book's ten chapters. The chapter headings provide a fair indication of the book's contents: Scientific and Social Issues in the Advance of Behavior Genetics; Methods and Models Available for Research in Behavior Genetics; The Twin Method with Illustrative Findings; Genesis of the MAVA Model and Its Solutions; Further Designs for Determining Genetic, Threptic, and Heritability Values; Models of Interaction of Learning and Genetic Processes; Evaluating Interactions: Path Coefficients and Diverse Heritabilities; The Inheritance of Abilities: Some Psychometric Requirements; The Heritability of Nine Primary and Five Secondary Source Traits in Questionnaire Data; Heritability and Conceptual Advances for Source Traits in Objective Test Data.

The detailed substance of the book is difficult to review. Nowhere else in the literature are the problems and complexities of behavior-genetic analysis so fully spelled out in a single volume. The methodological centerpiece is the MAVA model, which Cattell introduced in 1953. (It should not be overlooked that Cattell is one of the pioneers of human behavioral genetics.) Although Cattell has written about MAVA in a number of other sources, this is by far the fullest exposition of it. The acronym MAVA stands for *M*ultiple *A*bstract *V*ariance *A*nalysis—*multiple*, because a number of variance components are involved; *abstract*, because the variance components of primary interest are not directly measurable but are mathematically inferable from the raw variances obtained from various sets of kinship measurements. The MAVA method originated as an improvement over the classical twin method, i.e. the comparison of identical and fraternal twins, which allows only a quite limited analysis of the genetic, threptic and genothreptic components of variance. (In Chapter 3, Cattell offers the most thorough and trenchant critique of the classical twin method I have seen in the literature.) Cattell invented the useful term *threptic* to indicate that part of the total trait variance attributable to variations in the environment. The term environmental variance refers exclusively to measurable variation in the objective environment itself. This is a crucial distinction—between objective environmental variation, on the one hand, and the amount of phenotypic variance attributable to environmental influences, on the other. By use of the term *threptic* for the latter, Cattell has made explicit an understanding which has always been merely implicit in quantitative genetics. Cattell's term *genothreptic* refers to the covariance (or correlation) between genetic and threptic *deviations*, or what in quantitative genetics is given the potentially misleading term, genotype-environment covariance.

Whereas the classical twin method is perhaps the simplest of what Cattell terms the entire class of *conVarkin* methods (abstract variances deduced from *con*trasting *var*iances of *kin* groups), MAVA is the most complex. The *full* MAVA scheme, in fact, is aimed at estimating almost every plausibly (or even conceivably) hypothesizable source of genetic, threptic, and genothreptic variance in any quantitative trait. It would be impossible to describe this complex method within the confines of this review. Essentially it consists of solving overlapping sets of simultaneous equations in which the unknowns are the abstract *genetic*, *threptic* and *genothreptic* variances, both within and between families, and the known variables are the observable ('concrete') between- and within-family variances for various kinships (identical and fraternal twins, siblings, half-siblings, cousins, adopted children, each kinship reared together or apart). Necessarily implicit in the MAVA model is the well-established concept that different degrees of kinship indicate different degrees of genetic correlation. MAVA is distinguished from other methods of biometrical genetic analysis mainly by three features:

(1) its emphasis on *correlations* between genetic and threptic deviations;

(2) its systematically thorough distinction between *within*-family and *between*-family genetic, threptic and genothreptic variances and correlations; and

(3) the formal absence of the partitioning of the genetic variance into components attributable to *additive* (genic) effects, *assortive mating* (Cattell abjures the more common term, assortative), *dominance*, *epistasis*, and (the theoretically troublesome) *interaction* (as distinct from correlation) between genetic and environmental effects.

This particular partitioning of the genetic variance gained prominence in traditional quantitative genetics largely because of its development as a tool of agricultural and experimental genetics, in which a distinction between 'fixable' and 'non-fixable' genetic effects (to use Mather's terminology) is important in selective breeding. Selection can fix certain components of genetic variance, but not others, for transmission to subsequent generations. These can be classified in a 2×2 table as follows:

	Fixable	Non-fixable
Additive	Genic Variance Breeding Values	Assortive Mating
Non-additive		Dominance Epistasis

This type of analysis, which is of primary interest to the plant and animal breeders, although explicated by Cattell in Chapter

5, is merely adjunctive to the MAVA model. For example, the degree of assortive mating for the trait in question must be taken into account in order to partition the total genetic variance into between- and within-family components in the MAVA procedure. The estimation of heritability (proportion of variance attributable to genetic effects) separately for *between* families (H_B) and *within* families (H_W) is deemed important for the indication this division of heritability gives as to "where the forces that typically mold certain traits lie." MAVA's explicitness in ferreting out a variety of genothreptic covariances may yet prove to be the answer to our present puzzlement over the fact that the heritability *per se* of certain traits, such as intelligence, and the threptic variance accounted for by directly measurable environmental factors (or estimated indirectly from the weak correlation between genetically-unrelated children reared together) usually add up to considerably less than the total variance (excluding measurement error). The 'missing' variance may be eventually filled in by the various genothreptic covariances provided in the full MAVA model. On the other hand, the missing variance might consist of epistasis (genic interactions) and Genetic × Environment interactions and the MAVA procedure *per se* seems not to provide for these possible sources of variance.

But the analytic task appears inordinately demanding even without these complicating problems. As it is, the full MAVA model comprises 15 unknown abstract variance components, to be estimated from 19 concrete, empirically obtainable variances from a variety of kinships and rearing conditions (i.e. reared together or apart). This is what is so empirically daunting about the full MAVA, which at present is just a theoretical castle in the air. It has not yet been tried. As Cattell admits (p. 119), it will demand heroic qualities in the investigator. In MAVA, the problem of statistical significance, which has generally been ignored by the older classical methods of genetic analysis, demands very large samples of the various kinships, as compared with what we have been accustomed to in behavior-genetics research. Cattell (p. 387) claims that an N of 2000 to 3000 is at the lower limit of sample size required for acceptable standard errors of heritability and other variance estimates. The typical heritability studies of the past, with total Ns of 100 or less, are hardly adequate for reliably establishing much more than the fact that the heritability of a trait is greater than zero. Because there is already a considerable data base of kinship studies for some traits, such as intelligence, methods of meta-analysis would be applicable to results from a large number of different studies. The MAVA model, or some limited part of it, might well lend itself to meta-analysis of existing data, consisting of the mean or median correlations obtained in different studies of the various kinships. It still seems rather unlikely that any one investigator would be inclined to obtain all of the data required for the full MAVA with respect to any given trait. More likely, quite specific and limited genetic/threptic hypotheses will be tested which would not demand such vast data.

The picture becomes further complicated when MAVA methods are extended to the study of maturation and learning, as Cattell proposes. Briefly, this is accomplished by applying MAVA to longitudinal data or its cross-sectional age-groups data on a given trait. Such analysis would reveal the changing contributions of genetic and environmental learning factors to a given trait throughout the course of human development, from infancy to later maturity. With such ambitious goals, Cattell emphasizes the importance of careful selection of the variables to be subjected to such costly study. They should be highly reliably measurable traits represented by repeatedly replicable factors, at least within a particular culture. Cattell also points out the theoretical possibilities (and in some cases presents empirical demonstrations) that genetic, threptic and genothreptic covariance components may differ markedly between first-order (or primary) and second-order factors. For example, a second-order factor may be mainly genetic, but through interaction with differential environment opportunities would give rise to a number of first-order factors with fairly large threptic and genothreptic components. (The second-order g factor of fluid intelligence appears to be an example.) Or, a number of highly genetic primary factors could become phenotypically correlated through common genothreptic components, giving rise to a second-order factor which has relatively low heritability. (Cattell claims that the second-order factor of exvia [extraversion] is an example.) Thus the causal interpretation of the results of factor analysis can be aided by genetic analysis. Cattell's work is the first I have seen to spell this out fully.

If anything is needed immediately in the fields of quantitative and behavioral genetics, it is a uniform nomenclature and symbolic notation. No two textbooks are the same in this respect, to the dismay of both students and professors. Cattell has adopted the most comprehensive, detailed, and consistent notation to be found in the field. Whether or not it is the one that should be universally adopted will be arguable, but it appears more logical and systematic than any others I have seen. Too bad for those students who are intimidated by the superficial impression of difficulty created by the detailed subscripts to every σ and r in the MAVA notation! But one would be hard put to come up with anything simpler that would not create confusion.

I will not attempt here to review all the substantive findings which Cattell reports on the heritability of mental abilities and personality traits. In the abilities domain, Cattell's review of the evidence is wholly consistent with the picture of substantial genetic variance that has become so well established by numerous studies in the past decade or so. In the personality domain, the picture is more mixed, but it is clear that the estimated heritabilities of a number of factor-analytically established personality traits absolutely precludes a strictly Watsonian–Skinnerian learning theory view of the genesis of individual differences in personality. But various personality factors also differ markedly in heritability, ranging from 0.12 to 0.65, details of which are treated in Chapters 9 and 10. Surgency (high sociability and talkativeness) has about the highest heritability, and superego strength the lowest, among 12 traits.

Any serious and original book on the inheritance of socially-important human traits is destined for criticisms. We may anticipate some of these for the present work. A scientifically trivial, but practically considerable criticism is what I imagine will be most college instructors' estimate of the book's unusual level of difficulty for the majority of psychology students, even graduate students. Although Cattell claims it as a textbook, Cattell's highly discursive style does not seem to keep the unsophisticated student in mind. (The book's list price might also deter many of the student audience.) Minimum prerequisites for students using this book would be a course in analysis of variance and mastery of the contents of a more simple introductory text in behavioral genetics, such as *Behavior Genetics: A Primer*, by Robert Plomin and John DeFries. Cattell's book will be most rewarding to readers who already have a good background in quantitative genetics and are already familiar with the typical problems and standard methodology of human behavioral genetics. The book is an absolute "must" for anyone who teaches a course or does research in this field.

I have found a couple of technical errors that might cause trouble to students: on page 58, the N should be omitted from the numerator of the formula for the within-twins variance, and also from Equation 3.2 (between-families variance). The midparent–midoffspring theoretical genetic correlation (Table 5.8, p. 144), under the assumptions of no dominance and random mating, is not the same as the midparent–one offspring correlation (i.e. $\sqrt{1/2}$), but is $\sqrt{n/(n+1)}$, where n is the (average) number of offspring per family. Also, footnote b to Table 5.8 is in error: although the midparent–midoffspring

covariance is indeed the same regardless of the number of offspring (as Cattell correctly cites Falconer's statement on this), the *correlation* (i.e. the covariance divided by the geometric mean of the standard deviations for midparent and midoffspring) increases with the number of offspring, its asymptotic value being the narrow heritability. (This error is repeated in the last paragraph of p. 145, in footnote *b* of Table 5.10 [p. 145], and in the last paragraph on p. 300.)

The much more general criticism we may anticipate, not only in connection with Cattell's present heroic effort but in relation to current theoretical developments in behavioral genetics generally, is that, in this field, theoretical formulation and mathematical refinement run far ahead of empirical demonstration. This unavoidably raises the cost/benefit question of the empirical feasibility of executing all of the theoretically possible analyses to estimate every conceivable source of variance. Contemplation of all the theoretical possibilities may surely give us pause for the complexity inherent in the genetic-environmental causation of human behavioral variation. But, considering the size of the empirical task implied by such refined mathematical and statistical analysis, one may also begin to wonder if the answers provided by such Herculian efforts actually tell us what we want most to know. On reading certain of the more theoretical parts of Cattell's book, I can almost see it coming in behavioral genetics, as it did in factor analysis—a new breed of purely mathematical types, who have little or no substantive interests in either behavior or genetics, but who would work exclusively on the mathematical and statistical problems posed by quantitative genetic analysis, providing ever more elegant and esoteric refinements of methodology. Such activity may prove scientifically fruitful, up to a point. The point of diminishing returns is passed when the mathematical refinements greatly exceed the quality of feasible data. A probably more attractive approach, for most investigators, would be to test only quite limited hypotheses, for which rather particular limited data sets would be appropriate, rather than to attempt in one study to estimate all conceivable genetic and threptic variance and covariance components in any given trait.

So far, human behavioral genetics, for the most part, has attempted to demonstrate an innate biological basis for inter-individual variations in behavioral traits. But once this fact is demonstrated reliably for any given trait by rather simple means, albeit imprecise, where next do we go? Will further refinements of quantitative-genetic analysis then add up scientifically to more than merely a purely methodological *tour de force*? How much can it advance us toward answering the more ultimate questions of causal mechanisms? Perhaps our rather simple present methods of heritability analysis will adequately serve the useful purpose of a 'Geiger counter', merely to locate the potentially most fruitful pay dirt in the behavioral realm for more direct experimental investigations of the biological causal mechanisms involved in various dimensions of individual differences. Greater mathematical or statistical precision in the estimates of the genetic and threptic variances would then be of comparatively little interest or importance. There are even those who would argue that we should simply take the heritability of all important human traits for granted, and proceed directly to the investigation of the physical basis of behavior. These critics would question how more elaborate methods of analysis of genetic/threptic variance components can offer much aid in this direct approach to learning about the physical basis of individual differences, or would improve our understanding about how individual deviations might be beneficially influenced by specific environmental manipulations. These are just some of the kinds of broader philosophic questions which behavioral geneticists will have to ponder for the future development of their science. No other book that I have come across in this field provokes profounder thinking about the future of behavior genetics than does Cattell's book. Readers who are adequately prepared for the task and who wish to understand human behavioral genetics in a broader and deeper perspective will be amply rewarded for the time they spend with this book.

ARTHUR R. JENSEN

N. WIKLUND: *The Icarus Complex*. Tryckbaren, Lund, Sweden (1978). 272 pp.

This is an interesting and exemplary study indicating how research on Freudian hypotheses in the personality field should be carried out. The Icarus Complex is usually understood to contain a combination of enuresis, fire setting (pyromania) and ascensionism. H. A. Murray added a number of other traits, but retained the Freudian notion of a fixation at the urethral–phallic stage of development. The theory is certainly susceptible to empirical study, at least in its descriptive form, and Wiklund gives an excellent summary of the available evidence, and describes original work of his own tending to substantiate at least a relationship between enuresis and pyromania. He goes on to consider the Freudian theory, but rejects it in favour of an alternative theory, namely Maier's frustration theory. The argument is clearly developed, the evidence is interesting (and much of it would not normally be known to readers of this journal), and altogether the book demonstrates what can be done with the type of Freudian theory that has usually been dismissed as entirely speculative. There are faults in the empirical contribution, thus the factor analysis is left with an orthogonal rotation, when clearly an oblique rotation should have been carried out; this might have affected the interpretation of the results to a marked extent. Nevertheless this is a seminal work that carries the empirical investigation of Freudian theories in the field of personality to a much higher level than one has been accustomed to in the past. It is to be hoped that future researchers will follow Wiklund's example.

H. J. EYSENCK