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On the Madness in His Method: R. B. Cattell's Contributions to Structural Equation Modeling

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Overview

In the space available for this essay I can provide only a cursory review of the many methodological contributions of R. B. Cattell. I have chosen to look at these from the vantage point of contemporary issues in structural equation modeling. Cattell's factor analytic approach is compared with current modeling practices. A critical evaluation is offered which finds much of Cattell's work still innovative, still technically advanced, and still of great value to contemporary model builders.

Historical Introduction

Ninety-four articles, 7 chapters, and 5 books represents a formidable reading list for any short essay. This reading task is compounded by the advanced technical nature of the mathematical and statistical issues it entails. Furthermore, a review of practical applications of these techniques is needed to provide a substantive perspective. Such a reasonable reading requirement adds to our list with an additional 328 articles, 36 chapters, and 39 books. This immense reading task is dwarfed only by the incredible fact that the writing for it is being done by a single person.

Raymond B. Cattell's views on many scientific topics seem strongly related to influences apparent in his youth. He grew up along the coastline of Southern England and nautical methaphors persist in his writing. Cattell's academic education of the early 1900's emphasized philosophical study in the work of Frances Bacon and J. S. Mill, and

I primarily thank John Horn for providing critical ideas, information, encouragement, and support for this unusual task. Unlike some of the other authors in this volume, I have never studied with Cattell nor, until very recently, had I met him. Nevertheless, I am also grateful to Ray for much advice and support. I am also grateful to my many friends and colleagues who have offered suggestions—paticularly, John Nesselroade, Dick Lehman, Bill Rozeboom, and Bill Gardner. Funding has been provided by the National Institute on Aging through research grants numbered R01-AG02695 and R01-AG04704.

undergraduate training in Chemistry. His methodological outlook took definitive form during the 1920's in his doctoral studies in Psychology as a "midshipman" under Charles Spearman. Cattell expressed the unusual academic serendipity of his development when he wrote:

> "My emphasis is only to redress a balance; for here as elsewhere I have always argued for a two-handed use of factor analysis and analysis of variance. I trace this to student years in which I shuttled across a little plot of grass between the laboratory where Spearman was developing factor analysis and the Galton Laboratory where (R. A.) Fisher was shaping with equal brilliance the analysis of variance." (1978b, p. viii). "Like any two really creative persons they had no use for one another." (1977c).

This historical period in the parallel development of both analysis of variance and factor analysis is of continuing interest (as discussed by Cattell in this volume; but also see Box, 1978; Hearnshaw, 1979, p. 154–181). Although some use of analysis of variance may be found in his work, Cattell steered his own studies mainly with factor analysis.

Cattell's factor analysis interests were overshadowed in the early years of his career by his substantive concerns; these included extensive clinical practice and initial advances in behavior genetic methodology, (see Loehlin in this volume). Following a move to the U.S. just prior to World War II, Cattell produced some of his most important work on factor analysis. In 14 articles (notably 1943c, 1944a, 1947b, 1949d), and in an introductory textbook (1941a), methodological research innovations were offered to both the advanced specialist and the unexpecting novice. Cattell's travels during this time included frequent visits to the Chicago laboratory of Louis L. Thurstone (1947; see 1948g), an early exponent of modern "multiple-factor" techniques. The 1950's marked the growth of Cattell's own laboratory at Illinois, where he produced his well-known *Factor Analysis* textbook (1952a) and over 18 articles. His Illinois environment of this time included its own cast of factor analytic heavyweights, including Saunders, Humphreys, Tucker, Kaiser, and Wrigley.

Cattell's lab was most active during the 1960's with students such as Horn, Nesselroade, White, Gorsuch, and Schönemann, and visitors such as Radcliffe, Hundelby, and Pawlik. During this decade Cattell and his colleagues produced some 30 methodological articles and created *The Society of Multivariate Experimental Psychology*—a society based on principles enunciated in a methodological manifesto titled *The Handbook of Multivariate Experimental Psychology* (1966a). Although Cattell's lab was cast adrift by his retirement (in 1973), the 1970's list of his methodological contributions includes over 15 articles and two books focusing on factor analysis.

Another useful historical perspective is provided by tracing the computer support available to Cattell over these decades. During the period from 1920 to 1950 Cattell's complex research calculations were completed solely by hand(s) pushing pencils and pulling levers. Notably, Cattell and his colleagues considered many ways to make these tasks less tedious and more accurate (e.g., Cattell, 1952a, 1977c). In the period from 1950 to 1970 Cattell's laboratory used the most advanced computer techniques then available. In the 1950's he had access to the famous Illiac, the first of the first-generation of modern computers, and his lab was the proving ground for many popular calculation algorithms (Cattell, 1966a). But while Cattell sought out and found the most advanced state-of-the-art computing machinery available, the storage and speed capacities available for his major mathematical and statistical researches were about equal to those found on a contemporary personal computer. In any evaluation of Cattell's work it is important to recognize that modern day computing hardware and software simply were not available throughout most of his career (see Cattell, 1965g).

Cattell's methodological research has focused on the use and development of the techniques of "factor analysis." But in this essay I will describe Cattell's work using the contemporary language and concepts of *linear structural equation modeling* (e.g., Horn & McArdle, 1980; Jöreskog & Sorbom, 1978; McDonald, 1979). I will distinguish four broad areas of any structural equation modeling enterprise—Specification, Estimation, Comparison, and Substance. I will use further descriptors within each of these areas to highlight major issues and problems. Although this organizational scheme may at first seem to be distracting, it will enable me to lay out a brief but broad overview of the basic concepts used in contemporary structural modeling. In the long run it will also help the reader to avoid unnecessary concerns about the particulars of algebraic notation.

This use of structural equation modeling concepts to describe Cattell's work may surprise many current researchers in this area. Indeed, references to Cattell's work are largely nonexistent in much of the literature of this area (e.g. Bentler, 1980; Jöreskog & Sörbom, 1978). In this essay, however, these "revolutionary" techniques are overviewed to highlight a major point—R. B. Cattell has been using and developing structural equation modeling techniques for over five decades! Most important, there is still much that contemporary modelers can learn from Cattell's past and present structural adventures.

Although his thoughts on methodological issues represent only one aspect of Cattell's contributions, they are very important in a broad sense for understanding the most central features of all his ideas. The constructs of Cattellian factor analysis are the "architechtonic" foundation on which all of his other psychological ideas stand or fall. Indeed, these methodological efforts have most clearly separated R. B. Cattell from mainstream psychologists and, interestingly enough, from mainstream psychometricians and other methodologists (see Goldberger, 1971).

Other pervasive characteristics of Cattell's approach are found, perhaps not too surprisingly, in his frequent use of nautical metaphors. Cattell seems to suggest that we view the total personality system as a vast ocean of unknown depths, the psychometric laboratory as a sturdy ship and sail, the research community of scholars as members of a crew, and, most critically here, the factor analysis methodology as a guiding compass and navigation chart (see 1937b; compare 1980a). In these massive contributions we find all of the adventure, enthusiasm, and swashbuckling aspects of a seafaring odyssey.

It follows, then, that in any comparison with the standard mathematical or statistical treatment, where especially high premiums are placed on dry technical accuracy, Cattell's work is, at the very least, unusual. As will soon be obvious even to the casual observer, there is some madness in Cattell's method.

Specification—From Psychological Models to Mathematical Definitions

Contemporary structural equation modeling typically begins with the translation of verbal statements of a psychological hypothesis into a mathematical form. A few key principles used in model specification may be listed as:

(1) Global Distinction—'variables' (also termed 'terminals' or 'nodes') representing scores on objects will be separated from 'parameters' ('unifiers' or 'edges') representing relations among the variables.
(2) Unifying Distinction—'covariance' ('symmetric' or 'undirected') parameters will be separated from 'regression' ('asymmetric' or 'directed') parameters.
(3) Terminal Distinction—'manifest' ('observed' or 'obtained') variables will be separated from 'latent' ('unobserved' or 'hidden') variables.

As we now show, Cattell "navigates the same seas with a different compass."

The "specification equation" of Cattell's factor model (1952a, pp. 31, 76; 1978b, p. 34) is in the form of a 'linear regression equation': every manifest variable is equal to a weighted summation of several latent variables. (Cattell typically omits 'specific' factors to simplify his presentations.) The asymmetry of this model is fundamental—the manifest 'output' variable is determined by the latent 'input' variable(s). For Cattell, factors represent underlying "determiners," "functional unities," "source traits," "influences," or, with a given time sequence, they exist as "causes" (e.g., 1966a, p. 179). He proposes that factors, like other natural influences, exist in quantity and are likely to act as "multiple determiners" of manifest variables.

In Cattell's factor analytic model, manifest variables are specified by measurement operations, but the number and nature of the latent variables is a question open for exploration. This exploration is fully determined by invoking a few "reasonable scientific assumptions" about latent common factors. Most critically, he suggests that an individual factor, when acting as a real influence, ". . . is unlikely to operate on more than a small fraction of any truly comprehensively, representatively chosen set of (manifest) variables." (1962c, p. 685). Thus, Cattell closely follows Thurstone's (1947) principle of "simple structure" and presumes that a unique and sparse pattern of "behavioral weights" (i.e., factor loadings) will result from appropriate axes rotation. This kind of reasoning also leads Cattell to specify "oblique" models—like all other naturally occurring phenomena, factors operating as influences are correlated amongst themselves (e.g., 1952a, 1966a, 1978b, etc.).

Cattell (1952a) has clearly built on the "pioneering" work of Spearman and Thurstone, but he has added a few subtle and often overlooked scientific features to their earlier notions. For example, he introduced the idea of a factor pattern specification of "instrument" factors (i.e., "method" factors) to account for real influences of similar test forms, testing conditions, or specifics of sampling conditions (Cattell, 1957a; Cattell & Dreger, 1977). He also recognized the need to specify models including "artifactors" (i.e., "error" factors). More advanced concepts about "trends and cycles" (Cattell, Cattell, & Rhymer, 1947; Cattell, 1966a), and the separation of "dynamic trait" and "state" factors (e.g. 1957a, 1966a), were specified with latent variables representing the underlying, unitary, and invariant "processes" or "changes" among multiple factors (see also Horn, 1972; Nesselroade & Bartsch, 1977; compare Sternberg, 1977).

Cattell also proposed the "modern" idea of changes in the regression weights themselves—"modulation" effects, introduced to represent a factor which is temporarily raised to a new level by a particular experimental stimulus (1963e; 1966a). In other models, he characterized the notion of "non-linearity and interaction" among factors in terms of additional linear model factors (1960a, 1978b), although the patterning of relations among such factors, which is required for later factor identification, was not completely specified (but see McDonald, 1967; Rozeboom & McArdle, 1983). Cattell's introduction of such basic principles can be seen as a dramatic shift in the traditional factor conceptualization.

Following Thurstone (1944), Cattell advocated the specification of "higher-order" factors—models in which 'first-order' latent factors are themselves determined by the weighted sum of other 'second-order' latent factors. There is no doubt that Cattell considered these higher-order influence models in much the same way as the lower-order models (e.g., 1947d, 1956e, 1975d). But a decidedly different level of abstraction runs through the history of these discussions:

> "One of the unfortunate results of uncritical imitation of the physical sciences is the assumption by the social scientist that he 'knows' the direction of causation in any correlation and that he is entitled to use the terminology of dependent and independent variables when in fact this conceptualization does not strictly apply. . . . In general not only do we lack information about a specific direction of causation, but we also have given to us the general directive from the whole of social and biological research to the effect that 'most interaction will be circular.' The form of such interaction most commonly discussed today is what has been called feedback or servo mechanism." (1952a, p. 361–362).
>
> "The multivariate method is noncommittal and open minded about the dependent-independent variable relationship. It can use it or leave it." "Parenthetically, it may be asked why variables cannot also be both causes and, like factors, consequences. The answer would seem to be that they can be, but with lower probability." "The general reticular model, a network with unrestricted directions of influence, is the most generally acceptable solution to accommodate most scientific possibilities. The popular monarchic hierarchy is often a constantly recurring artifact from the statistical limits of any single factor analysis." (1966a, pp. 9,213; 1965j, p. 262).
>
> "However, the more important relations, and those with which we are concerned in the present primary, secondary, etc., discussion, are among factors, and here the ultimate extremes of possibility are between (1) orderly strata, as discussed, with one-way action and (2) completely free interaction in any kind of network, which we shall call the 'general reticular model.' In that model there can be direct action among factors along with positive and negative feedback in all directions" (1978b, p. 200).

Thus, while Cattell limits his mathematical specification to the fundamental factor form at both first and second orders, he suggests that the second-order models may be used to represent psychological theories of a broader class—namely, the 'latent variable path' or 'systems' models displayed in Figure [1]. Cattell does not typically promote the specification of path analytic models among latent variables until "relations become known" (1965j, p. 262).

History shows that most efforts to map out the constellations seen in Figure (1) leave plenty of room for failure, and, in some senses, Cattell has failed. In an effort to provide a broad treatment Cattell maintained a formal precision that may have limited his consideration

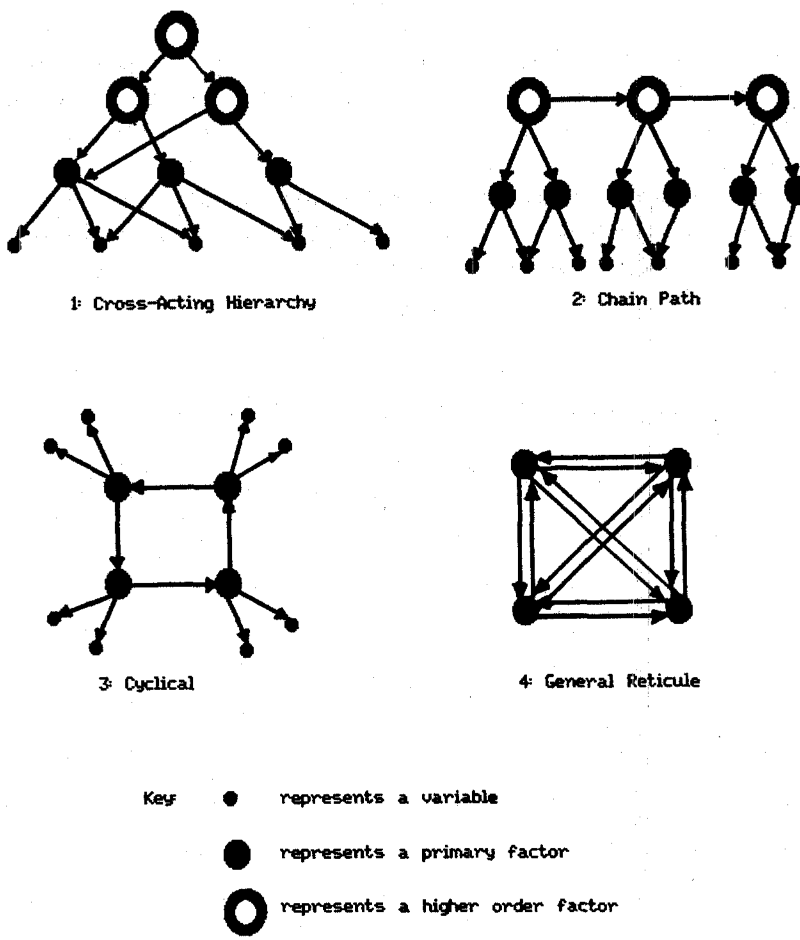

Figure 1: The "General Reticular Model of Factor Influences" (after Cattell, 1965j, p.238)

of alternative possibilities. For example, Cattell did not rigidly specify either the number of latent variables, or the precise non-zero pattern of their coefficients. This did not permit Cattell to consider, say, the path analytic pattern specification of Guttman's "simplex" (Jöreskog & Sörbom, 1979), and led to Cattell's claim that the simplex was a "mosaic without scientific meaning" (1966a, p. 212). Also, there are many models that Cattell strongly advocated but never fully developed. For example, models that mix both analysis-of-variance and factor analysis are highly praised but rarely specified (but see Loehlin in this volume).

The assumption that Cattell has failed is, however, also misleading: By any reasonable contemporary standard Cattell's modeling specifications remain highly advanced and widely diverse. Although he limited his formal mathematical specification to models based on the factor analytic tradition, Cattell goes well beyond this class of models in his multivariate thinking. Indeed, Cattell's historical conceptualizations remain several steps ahead of most current multivariate thinking about the rudiments of causal action (compare Wold, 1956).

Estimation—From Mathematical Models to Empirical Data

After specifying mathematical models one next logical step is to obtain values for the unknown parameters from a selected set of available data. A few key principles in model estimation may be listed as:

(4) Summary Matrices—Raw data are summarized in the form of empirical correlation, covariance, or moment matrices, and initial parameter values are chosen to yield model reproduced matrices of similar metric.

(5) Algorithmic Fitting—The 'badness-of-fit' between the empirical and reproduced matrices is indexed by a selected mathematical function whose value is 'minimized' by invoking iterative numerical algorithms.

(6) Generative Parameters—A set of values are accepted which minimize the badness-of-fit between model and data, have 'stabilized,' and have no logical, mathematical, or statistical 'identification' problems.

Again, Cattell "navigates the same seas with a different compass."

The lack of formal a priori specification of a restricted parameter set created an initial need to determine 'the appropriate number of factors.' One of Cattell's most popular inventions in this regard is the "scree" test which, ". . . suggested itself to the writer from experience of a hundred or more factor analyses carried out over thirty years" (Cattell, 1966g, p. 249). In this test the number of factors is determined by visual search for an inflection point in the sequential latent root plot—the point(s) that separates the mountain of useful variance "from the straight line of rubble and boulders which forms at the pitch of sliding stability at the foot of a mountain." (1966g, p. 244).

For Cattell, the process of parameter estimation was initiated by the data-based indication of a range of feasible numbers for a wide range of non-common factor models (Cattell & Vogelman, 1977; Cattell & Burdsal, 1975). But, in Cattell's view useful parameter estimates were obtained only after factor rotation, so the precise nature of the scree was not stated a priori as a formal hypothesis (see 1978b, p. 85).

Unlike many other modern factor analysts, Cattell strongly urged the inclusion of an extra "error factor" that, he said, would later be "rotated away."

Extensive numerical calculations were typically required in the extraction of a given number of factors. Given the available computing power of the early days of Cattell's research, it was somewhat surprising to find that Cattell quickly and uncategorically dismissed the non-iterative Principal Components extraction as a "closed model." Neither was Cattell generally in favor of Guttman's "Image" model—he noted that the estimation is rapid but he seemed more concerned that the specific variances were only a "lower bound, not the most likely value," so the "mirror is distorted" (1966a, p. 227; 1978b, pp. 389–390; compare Velicer, et al., 1982). At another extreme, Cattell consistently chose iterative least-squares methods of factor extraction (i.e., Centroid, Principal Axes) over the computationally inefficient but "most elegant and preferable" maximum-likelihood extraction (1952a, p. 146–148; 1978b, p. 378–380). In general, Cattell always considered computational problems, but he was outspoken about the required match between computational technique and scientific model.

Similar scientific and practical logic is applied to critical questions surrounding the problems of obtaining final estimates by 'rotation.' Here Cattell found computationally convenient 'clustering' techniques useful in the analysis of large item sets (Cattell, 1944b; Cattell & Burdsal, 1975), and for the separation of divergent sets of objects (TAXONOME, Cattell, 1966a; Cattell, Bolz, & Korth, 1973). But, for Cattell, the major purpose of factor analysis was in the discovery of "source traits"; he was an outspoken critic of the computationally convenient search for patterns of "surface traits" by clustering:

> "The view that traits are only to be discovered by empirical studies of covariance is strongly maintained and developed in all the following discussion. But it is equally strongly disputed that the definition of a trait merely as a mathematical factor or, still more inadequately, as a simple cluster or correlating elements, is defensible." (Cattell, 1943c, p. 573).
>
> "The finding of types and species is quite a different aim, pursued, for example, by cluster analysis . . . but factor analysis is by no means a taxonomic instrument and it is poor and misleading teaching to present it as such." (1966a, p. 179–180).
>
> "In short, to quote a familiar verse, clusters "are made by fools like me but only God can make" a hyperplane. It is because it is such an inherent expression of nature that we pursue it in seeking natural structure." (1978b, p. 135).

So, in stark contrast to simple cluster searches, Cattell operationally defined his simple structure rotational estimation as a search for natural hyperplanes with "maximum hyperplane count" to locate the factor as the "line created by the intersection of the remaining hyperplanes." (1952a, p. 218).

Cattell's rotational estimation was entirely done by visual adjustment in a 'single plane' (i.e., two factors at a time), was 'blind' to the specific manifest variable locations, usually permitted oblique latent variable solutions, and terminated on a stable "history of hyperplane count." The same processes were reapplied to the covariances among the factors for the further estimation of higher-order factors. In most of Cattell's work this visual adjustment process took many months to complete (1952a, p. 253–288). Given these task demands it is not surprising to find that Cattell directly influenced the development of computer based 'topological' procedures for visual rotation (e.g., MAXPLANE, Cattell & Muerle, 1960; Eber, 1966; ROTOPLOT, Cattell & Foster, 1963; compare Katz & Rohlf, 1975). Cattell also indirectly stimulated the development of a wide range of other analytic rotational procedures which now seem more popular than any of his own topological favorites (e.g., EQUIMAX and PROMAX).

It should now be clear that, for Cattell, factor estimation was not done in a "little jiffy" (i.e., compare Kaiser & Rice, 1974). Instead, Cattell searched for a set of "exactly identified" but stable parameter estimates which optimally matched model to data. That is, Cattell searched for, but did not a priori assume, the validity of 'fixed zones' in complex data patterns. For Cattell, "over-identification" conditions such as these were a potential outcome of the factor analytic resolution of the structure inherent in the data.

In a broader sense, then, Cattell's estimation philosophy is very similar to contemporary techniques—with one major exception: mathematical goals are uniformly considered subservient to scientific goals. This philosophy of data analysis leads to familiar estimation problems and controversies because a priori scientific restraints often do not rigidly translate into a priori mathematically desirable conditions. On the other hand, this data analytic philosophy neatly sidesteps other major modeling potholes. Cattell steadfastly refuses to align the term "factor" with opportunistically selected variable groupings, forced by a priori definition to have completely zero hyperplanes, and made to fit the data by the inclusion of "specific" covariance parameters (e.g., "correlated errors"). This Cattellian perspective is distinctly at odds with the set of simplistic cluster-based models that make up, and indeed seem to be the goals of, much contemporary structural modeling.

Another major contribution to structural estimation comes in Cattell's simple but general statement about the available data that can be analyzed. In the typical factor analysis ("R-technique") is 'subjects-by-variables' data matrix is computed over subjects (rows) and summarized as the covariances among the variables (columns). But Cattell, through the now well-known "covariation chart" (see Cattell, 1946a, p. 97), proposes that meaningful estimation can also be obtained from other "faces," "facets," and "grids" of the available pychological database (for a detailed treatment see Cronbach in this volume). As an example, Cattell claims that meaningful analyses can be obtained by computing over variables and factoring the covariances among subjects ("Q-technique"; compare Stephenson, 1936). He provokes further controversy by applying factor analysis to the covariances among variables calculated as the difference between the same variable obtained twice ("dR-technique"; see Nesselroade, 1972). But perhaps the most ignored product of this way of thinking is Cattell's factor analysis of the data from a single subject—where covariances among variables are computed over occasions ("P-technique") and in another case where the covariances represent lagged occasion differ-

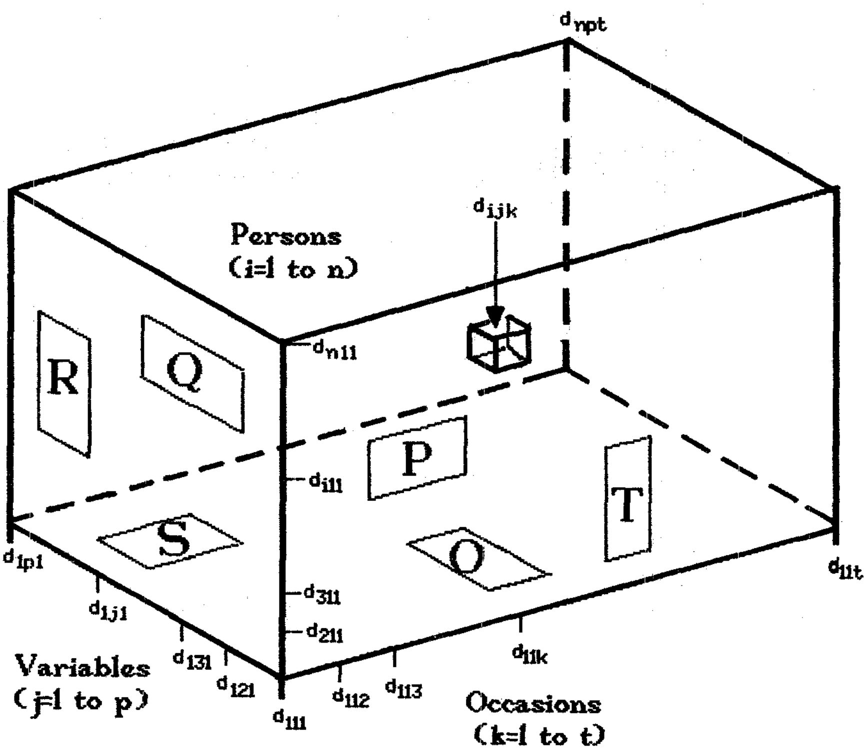

Figure 2: The "Covariation Chart" or "Data Box" (after Cattell, 1952e, p.501)

ences ("dP-technique"; e.g., Cattell, Cattell, & Rhymer, 1966; Cattell & Birkett, 1978; see Nesselroade in this volume; Wohlwill, 1973; McArdle, 1983).

In much of Cattell's work mathematical models were based on mixed observation modes, and statistical parameter estimation required a rigorous psychometric evaluation of scoring systems at both the manifest variable and latent variable level (1964a, 1970b, 1970c). Models were proposed and developed which required varying data collection modes (i.e. Behavior Rating, Questionaire, and Objective test data), extensions with "n-way" data partitioning (e.g. 1966a, p. 226; 1978b, p. 361–365), and empirical mixtures of multiple sampling units and database sources under different experimental conditions (1972b; 1978b).

It follows that Cattell has not added much to traditional estimation techniques from a statistical perspective—in fact, he could hardly be classified as a statistician. But that which was, and that which continues to be innovative about Cattell's work is that traditional views on factor analytic parameter estimation were matched with novel views on psychometric measurement and data selection design. The scree test, for example, is a visual illustration of Cattell's unique mixture of scientific creativity, mathematical expertise, practical experience, and lack of interest in the algebraic formality of parameter estimation. With his deceptively simple data-box innovations, Cattell has lucidly sliced-up and turned-around traditional data analytic thinking, and opened up previously overlooked portholes to view the psychological sea floor.

COMPARISON—From Empirical Models to Quantitative Evaluation

After obtaining parameter estimates one next logical step is to compare proposed models and empirical data on mathematical and statistical grounds. A few key principles used in these structural comparisons may be listed as:

(7) Fig Indices—Parameter salience is indexed by theoretical or empirical 'standard errors', and the overall model adequacy is indexed by 'parsimony' or 'chi-square' measures of the difference between empirical and reproduced matrices.

(8) Alternative Models—Any proposed model is evaluated by contrasting its fit with meaningful substantive alternatives, and arbitrary "null" standards, which have also been fit to the same data.

(9) Different Methods—Results for the same basic model fit to the same basic data are compared using different data compositions, data summaries, fitting criteria, and computational techniques.

Here again, Cattell "navigates the same seas with a different compass."

Cattell was acutely aware that many of his criteria for factor analytic resolution were grounded on logical bases and theory about natural phenomena rather than on formal mathematics or statistics. Knowing this, he attempted to illustrate that natural influences operated in specific ways and that one could recover this natural structure using factor analysis. In this "plasmode" work Cattell used his factor analysis methods to recover the structures of known form ("plasma") identified by known measurement operations ("mode"), under conditions where "(1) the data are physical, (2) the number of source factors is known independently . . . and (3) much analysis of properties has already taken place" (Cattell & Gorsuch, 1963, p. 57). Cattell and his colleagues cleverly studied the underlying structure of, for example, the "Movement of Balls" (Cattell & Dickman, 1962) and "Cups of Coffee" (Cattell & Sullivan, 1962). In contrast to, say, the nonlinearity of Thurstone's more well-known "boxes" problem, Cattell emphasized the importance of naturally occurring error and other "real-life" variation in these demonstrations (see Sokal, et al., 1980). More recent plasmodes include a variety of purely mathematical statements useful for Monte Carlo design work (Cattell & Vogelman, 1977).

In the early decades of his work Cattell answered questions about the number of factors with a variety of mathematical indices based on the residual matrix, including the use of the chi-square statistic (e.g., 1952a, p. 296–302, 1958d). It appears, however, that the statistical comparison indices did not satisfy him because Cattell: (a) did not view the number of factors primarily as a statistical question (see also Kaiser, 1976), (b) did not wish to make arbitrary cutoff conventions for complex mathematical issues, (c) did not answer the number of factors question in the absence of theory guided rotation, (d) did not answer the number of factors question in a single experiment.

The visual illusions and non-quantitative flavor of the scree logic did not immediately suggest associated mathematical and statistical tests. So, as in many aspects of his methodology, Cattell never came to depend on formal "solutions" for important problems. According to Cattell, the scree test merely represented a useful indicator of a range of useful factor extractions that needs to be re-checked after rotation and by multiple experiments. However, recent work shows that the scree test is related to a broad set of root comparison techniques, such as chi-square (Horn & Engstrom, 1979), and compares favorably with other popular mathematical and statistical criteria for model comparison (Hakstian, Rogers, & Cattell, 1982). Furthermore, it may also be true that Cattell postured too far over in this anti-statistical stance. For example, Cattell may have overlooked many opportunities to develop useful indices of sampling 'cross-validation' (see Cudeck & Browne, 1983).

Within any particular experiment Cattell's stated goal was to achieve a rotational position with maximum simple structure—the largest count and narrowest hyperplane width possible for a given number of variables and factors. In all of his work Cattell advocated a variety of tests for checking the simple structure achieved. In the work starting about 1960, comparisons among alternative rotational counts are routinely available in his publications, and checking the distribution of final results against chance occurrences was formally accomplished using Bargmann's permutation based test (1978b, pp. 175, 568–575). To provide more direct rotational checking, Cattell and his colleagues developed a simple mathematical scheme for rotating the empirical factor pattern to an hypothesized target pattern—PROCRUSTES rotation, named after the innkeeper who provided custom-(er) fits (Hurley & Cattell, 1962).

Cattell now seems to consider these factor rotation methods as the precursor of contemporary 'confirmatory' modeling (see Cattell in this volume; 1978b). These rotational schemes, however, do not: (a) force any factor pattern coefficient to be zero, (b) permit reevaluation of communalities to achieve better fit, (c) solve for a partially specified target, (d) provide statistical features of the model estimates, or (e) deal with capitalization on chance in rotation (Horn & Knapp, 1974; Meredith, 1977). These particular features were not directly solved within Cattell's research but are now considered to be of great importance to structural equation modeling (e.g., Jöreskog & Sörbom, 1979).

One basic thread that runs throughout Cattell's model comparisons is the need for indices representing the goodness-of-fit across multiple experiments, and with different samples of subjects and variables. In his earliest data analytic work on "factor matching" Cattell provided a detailed development of r_p, a coefficient which attends to both the overall "level," and the overall "shape" of the factor pattern weights across experiments (1949e; 1949b). In later work Cattell and his colleagues recognized the importance of "configural pattern invariance" (see Thurstone, 1947; Horn, et al., 1983), and developed a simplified version of these indices termed the "salient variable similarity index" (Cattell & Baggaley, 1960; Cattell, Bakar, & Horn, 1969). Following a similar theme, Cattell suggests that 'factor efficacy' can be improved by the direct comparison of factors resolved from different faces of the data box (1966a; see Horn, 1972).

In his basic work on "factor invariance" Cattell makes a fundamental and often overlooked contribution to factor analytic thinking. L. L. Thurstone's (1947) requirements for the "principle of simple structure" were statements about zeros in the rows and columns factor pattern matrix (or reference vector structure matrix). He required, for example, that every row should have at least one zero given the "elimination of tests of complexity" (p. 335). In Thurstone's work the principle of simple structure seems to have led to statements about "metric invariance" over changes in test battery (p. 363) and "configural invariance" over changes in populations (pp. 360–365). But Cattell says:

> "the principle of parsimony, it seems, should not demand "Which are the simplest set of factors for reproducing this particular correlation matrix?" but rather "Which set of factors will be most parsimonious at once with respect to this and other matrices considered together?" ". . . The criterion is then no longer that the rotation shall offer the fewest factor loadings for any one matrix; but that it shall offer fewest dissimilar (and therefore fewest total) loadings in all the matrices together." "The special and novel required condition is that any two matrices should contain the same factors but 'that in the second matrix each factor should be accentuated or reduced in influence by the experimental or situational design,' so that all its loadings are proportionately changed, thereby producing, from the beginning, an actual correlation different from the first." (1944a, pp. 273–274)
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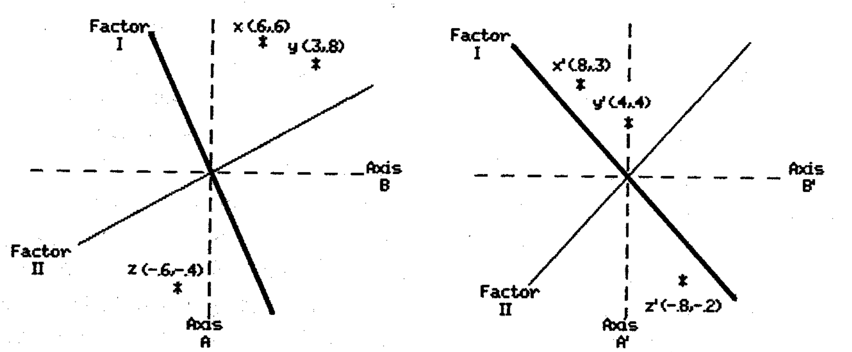
> "It is necessary that the two experiments have the same variables and yield the same factors 'but the variance of each factor in one experiment shall be different, through accidents of sampling or through deliberate manipulation of experimental conditions,' from that of the corresponding factor in the second." (1952a, p. 246)
>
> "The mere mechanical pursuit of literal "invariance of factor loadings" is not enough. We must not forget that basically we are in pursuit of scientific meaning, and "invariance" is secondary in the sense of being one manifestation of a scientific entity, and even so, it needs to be evaluated with regard to these principles showing what kind and degree of matrix to matrix 'variation' is really the evidence for the highest degree of variation." (1966a, p. 199)

Thus Cattell, in contrast to Thurstone, promotes invariance, not simply as a consequence of simple structure, but as "the most fundamental principle of parallel proportional profiles" (1944a, 1948c; see Figure [3] here). Furthermore, as is typical of Cattell's analytic work, he reminds us that invariance is only a necessary starting point for higher scientific goals. The principles he enunciates, of course, are based on experiences accumulated over practical research in the personality and ability domains—here Cattell was constantly faced with variables of high complexity and equally good but alternative simple structures. It follows that these natural science principles were not always accompanied by straightforward mathematical proof (but see Meredith, 1964; Horn, et al., 1983).



1. Plot of Loadings from First Experiment for Variables x, y, and z

2. Plot of Loadings from Second Experiment for Variables x', y', and z'

Figure 3: The "Principle of Parallel Proportional Profiles" (after Cattell, 1946a, p.291)

For several decades now Cattell has been adamant in his insistence on multiple group comparison using the principle of parallel proportional profiles. Unfortunately, as Cattell himself is well aware, the mathematical basis of this direct rotational comparison has not been fully developed. Cattell used generalized inverses to solve for the "orthogonal confactor" rotational position (Cattell & Cattell, 1955), but he did not uniquely identify the more critical "oblique confactor" problem (Cattell & Brennan, 1977). Interestingly enough, Cattell now recognizes the identification problems in these models, but he also knows the a priori selection of fixed zeroes or unities is too restrictive to serve his purposes (also see Horn, et al., 1983). The complicated procedures of "True Zero, Real Base" factoring procedures (1972b), which involve meaningful scaling units and experimental manipulation, have similar analytic problems.

Cattell clearly promotes the comparative study of fit using alternative models and methods—a contemporary viewpoint. Cattell is emphatic in his belief that "any experiment without appropriate statistical checks was a waste of time for everybody." (1977c). While there are some notable inconsistencies in his adherence to this principle, Cattell was far ahead of his time in recognizing that simple indices for complex questions are difficult to develop and can easily mislead. For Cattell, questions answered by "chi-square comparisons" are useful but do not tell the whole story. Factor analysis questions, he says, involve both psychometric 'sampling of variables' and statistical 'sampling of subjects' issues, and these issues cannot be dealt with separately.

Some view these model comparison complexities as an inescapable "Catch-22," which promotes circular thinking and prevents useful scholarship. Others merely use model comparisons as ammunition for target practice on "straw men," or as briefs for the legalistic defense of "pet theories." But, at the very least, Cattell consistently tries to develop new leads and new tools, and advocates still more complex mixtures of comparison testing, including multiple analyses and multiple experiments. For Cattell, the inherent complexity in comparison is a storm that can best be weathered by developing better navigation charts.

SUBSTANCE—From Evaluated Models to Psychological Theory

The explicit specification, estimation, and comparison among alternative models with empirical data provides implications for psychological theory. A few key principles in this phase of modeling may be listed as follows:

(10) Action Intepretation—Models are 'decomposed' into multi-parameter substructures (e.g., 'paths,' 'bridges,' 'indirect effects') to highlight the ways in which variables and parameters act in consort to produce data 'connections.' Latent variable labels are assigned to conform with these internal subsystems and presumptions about the manifest variables.

(11) Selecting Models—Final choices among the class of alternative models are resolved by comparative mathematical and statistical bases, by clear interpretations of model action, and by integration into existing psychological data and theory.

(12) Structural Readjustments—Both mathematical and substantive modeling issues suggest progressive adjustments to existing model specification, data design, and comparative analyses.

Here too, Cattell "navigates the same seas with a different compass."

In Cattell's grand modeling scheme factors represented traits, trait-changes, states, unique inter- and intra-person influences, and virtually anything else that the substantive and methodological mix permitted. Somewhat in defiance of the typical cautions (e.g., Cliff, 1983), Cattell was outspoken in his belief that "One is quite as much entitled to reify a factor, of simple structure, etc., as to use a substitute for . . . the cost of living, or the family next door." (1952a, p. 321). Naturally the teleological underpinning of this perspective required strong empirical evidence, and here Cattell most wisely invoked a basic modeling principle: ". . . the factor itself is not a unity; it is only the evidence of a unity . . ." (1952a, p. 315). This ". . . definition of a factor as an empirical construct follows remarkably closely the procedures stated by Bacon and refined by Mill for arriving at the essential nature of anything." (1952a, p. 338). Thus, although Cattell talked freely about the 'causes of covariation,' he also recognized that all he could hope to see were the 'foot prints' of real influences in data.

To follow these tracings, Cattell consistently advocated the design of structural experiments which had both 'homogeneity' with respect to things other than the factor, and 'heterogenity' of things that indicate the factor (e.g., 1952a, p. 354). He tried to examine the influences that acted to effect changes in a factor both internally, within a specific experimental situation (e.g., 'perturbations,' Cattell & Dreger, 1977), and externally, between multiple sampling experiments (e.g., effects of age, education, etc., Gillis & Cattell, 1978). He also analyzed specific experimental situations and proposed likely action-based interpretations. For example, he said, "If certain variables are in fact independent and outside the system this will be shown by zero loadings in the factors that comprise the system" (1952a, p. 362). He distinguished "factor fusion," where two or more factors that are operationally distinct nevertheless join together, from the action of "cooperative factors." Other action among factors was recognized in terms of "Brazil-nut" or non-cooperative factors, or as the result of scoring mechanisms leading to "eccentricity" or difficulty factors.

Amidst all of these colorful portraits, however, factor identification questions were primarily solved using empirically based ideas about invariance and replication. Cattell routinely made intricate efforts to show how good variable measurement combined with good factor rotation could help reduce uncertainty and lead to a high probability for invariant results. Because the measurement mixture was critical, as in the way a factor operated over many different experiments, Cattell consistently used alternative measurements and carried out many different kinds of experiments.

An unusually clear recognition of the problems of model 'misspecification' is evidenced by Cattell's analyses and interpretation of numerous results from higher-order factoring. By definition, factor analyses of any mixed density of lower-order variables always yielded higher-order factors. But Cattell added that such analyses should also yield traces of the true influence level, or "strata," and he provided mathematical guidelines for distinguishing influence patterns of the most likely strata (1965j, 1966a, 1978b). His own practical results also led Cattell to expand his basic factor model and ". . . suggest that an alternative to the 'influence' model, namely the 'emergent' model, must also be considered at higher-orders." (1966a, p. 212). So, again, Cattell placed heavy emphasis on the value of scientific empiricism.

The result of these kinds of experiments led directly back to an alternative model that may be specified, estimated, and compared to other alternatives. Cattell found that, "In practice it seems that two or three rounds of experiment are likely to be necessary . . . but an inspired guess may hit the bull's-eye sooner" (1952a, p. 339). The unique mixture of scientific and practical thinking led Cattell to envision a broad role for structural modeling:

". . . Thurstone, one of the leaders in this field, calls attention to the fact that "factor analysis has its principal usefulness on the borderline of science" where fundamental concepts are still lacking and crucial experiments cannot be easily devised. This is a very sound appraisal, providing we substitute base for borderline, but it overlooks possibilities of application beyond the classical use of factor analysis, possibilities which create the apparent paradox that factor analysis belongs both to the very earliest stages of research and the very last." (1952a, p. 15).

". . . an emphasis on the powerful hypothesis-creating qualities of multivariate methods should not be taken as any reflection on the degree of their hypothesis-testing utility." (1966a, p. 14)

"it is for this reason that the traditional term "hypothetico-deductive method" is so misleading. For this describes only one part of the cycle—the legalistic and *disputative rather than the exploratory and more scientifically creative part.* If there is any part of the spiral which can be called the scientific beginning, it is in the induction rather than the deduction. But what we can be certain about is that the complete cycle is an inductive-hypothetico-deductive-experimental-inductive one, no matter where we decide to cut it." (1966a, p. 15)

This 30 year old view very clearly paints a different picture than is seen in several contemporary perspectives on these issues (compare Gould, 1981, pp. 316–320; Sternberg, 1977, pp. 11–36). But for Cattell, this view is self-evident: Hypothesis formation, no less than hypothesis testing, should be empirically based, and the "inductive-hypothetico-deductive" spiral requires tools, such as factor analysis, and data at every level.

Cattell's research results provide the strongest possible testimonial to his remarkable breadth and depth in defining, conceptualizing, and extracting psychological meaning from structural analyses. This tremendous body of work reflects serious study in practically all of the major areas of psychology—genetics, clinical practice, moods, motivation, ability, personality, physiological processes, societal associations, etc. This work extends into many other areas of scholarship as well. In the face of swirling winds of substantive confusion, however, Cattell seems never to bat an eyelash. In dealing with the always difficult confusions that arise in interpreting factors, for example, Cattell advises that, "One should soak oneself for some days in the evidence as to the nature of a factor and then sleep on it." He all to calmly adds that it is "... essential also to keep a constant reference to other 'factors' ..." (1952a, pp. 338–339).

Cattell's grand modeling scheme is nicely illustrated by his most recent attempts to pull together all of his vast resources into a model "the total personality system" presented in Figure [4] here. Although some (e.g. Eysenck, 1981), have argued that he sails too far on this particular ship, few will doubt that Cattell's interpretations represent provocative predictions about new structural models.

Even by contemporary standards, Cattell shows remarkable resolve in the pursuit of complex structures, and in integrating vast experiences and practices. Cattell seems little concerned that his own structural models are not yet accompanied with precise values, or that, in important cases, they are not yet even identifiable by the mathematical standards of factor analysis. For Cattell, structural models represent "meta-models" for advanced substantive thinking and for the future refinements of tools for data analysis. This position is stated most clearly as his highest structural goal—the "integrative challenge of multivariate experimental psychology." (1966a, 1966f).

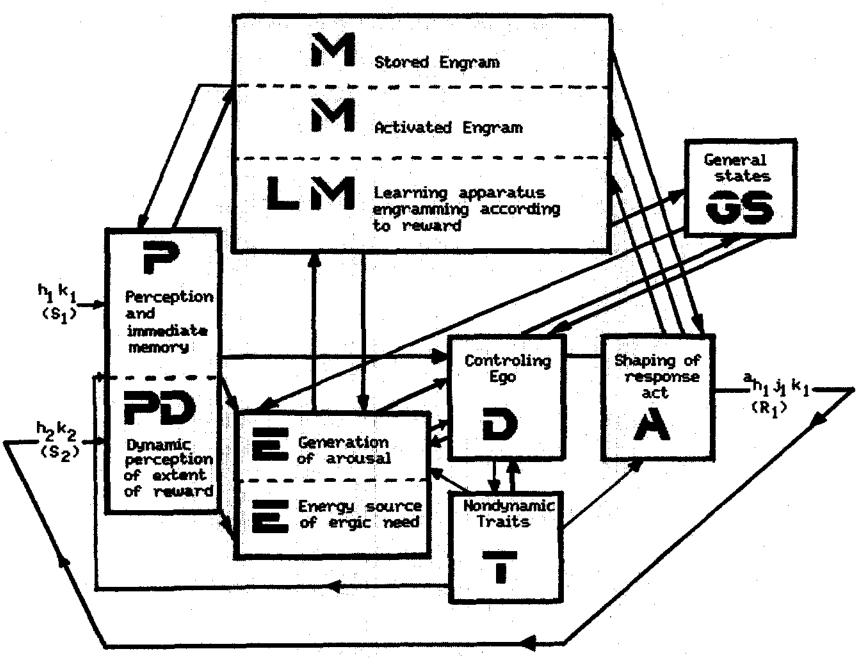


Figure 4: The "VIDAS Systems Theory Model" of Personality Action (after Cattell, 1982b, p.20-21)

On Madness in Modeling

We have looked only at a few jewels in Raymond B. Cattell's treasure chest of methodological contributions. I think it is important to examine these in the contemporary light of structural equation modeling. I do not wish to imply that he foresaw it all, or that there is nothing fundamentally new under the modeling lamp; but I do wish to suggest that much of what is regarded as excitingly new about structural equation modeling was also excitingly new in Cattell's work. More than this, I suggest also that Cattell's approaches, if not his solutions, are in some cases still ahead of contemporary thinking. In Cattell's work we find 'the same seas navigated with a different compass.' While his compass may now be a rusty antique, we should still ask the question "have we yet produced a suitable replacement for his sextant?" (Horn, 1984, personal communication). From my point of view, Cattell's work has been, and also remains, theoretically innovative and technically advanced.

This reasoning may be scorned by some contemporary modelers. After all, Cattell has not developed current day structural equation methods or algorithms, nor is he likely to use them now. It might seem to some that, in contrast to the formal organization of structural equations, Cattell's work is loosely hinged on arbitrary rotational visions, and the technical and conceptual limitations of his factor analytic tradition. If this impression persists, then maybe I have mismatched Cattell's work with structural equation modeling.

For me, however, the principal danger of miscommunication runs another way. Perhaps I have left the impression that the contemporary categories for modeling represent the standard against which Cattell's work should be evaluated. Actually, I think the opposite is true. The contemporary categories presented here represent only artificial "surface clusters" for Cattell's deeper "source trait" contributions. In an attempt to better define these constructs I have located and marked a mysterious line that runs throughout and, in some sense, holds together all of Cattell's work: I have simply defined this line as the madness in Cattell's method.

To unfurl one of the sails controlled by this line, consider the effect of Cattell's outspoken multivariate chastizements of the unexpecting mainstream psychologists. From the earliest days to the present Cattell has claimed that one bunch of landlubbers—the bivariate experimental psychologists—"walk with the ball-and-chains of bivariate, controlled-manipulative, brass-instrument thinking." For these lost souls the nostrum required to unravel the "dissociation of their birth trauma" may exist only in novel mixtures of advanced psychometrics and "concominant-variational" design. Cattell has been a bit less scathing in references to clinical psychologists. At least they have "sure instincts," he allows, albeit mixed with "non-metric observation and fallible memory." At another end of the spectrum, psychometric specialists have found that in reading Cattell they must deal with an unusually new vocabulary embedded in a writing style that aims to inject life into their dry language. The reward for their continuing efforts is atypical scientific speculation typically lacking formal mathematical proof. In a short space, Cattell can usually manage to offend, threaten, and turn off members of almost every band of scientist. For those who see Cattell's work in this way the madness in his method is best defined as "lunacy."

The ease and flexibility of multivariate methods are frequently extolled by Cattell. Unfortunately, in actual applications, practitioners find startling new requirements for extended measurement operations, lengthy increases in data design and data gathering, and the need for training in many nuances of mathematics and statistics. The specialist, who recognizes rapid growth in computer solutions, is not infrequently surprised to find that proposed systems of equations have no unique or even any clearly specified parametric solution. Yet Cattell continues to boldly and confidently navigate amidst geometer's hyperplanes in search of a most appropriate axes placement. For those who see Cattell's work in this way the madness in his method is best defined as "great folly."

Cattell has also lashed out at well-meaning researchers who have failed to heed other storm warnings he forsees. He castigates those who fail to see the fundamental importance of a "recording of observations, quantitative or qualitative, made by defined and recorded operations and in defined conditions, followed by examination of the data, by appropriate statistical and mathematical rules, for the existence of significant relations" (1966a, p. 20). And in a spirit of fairness he has railed at the quantitative specialist who can only seem to provide elegant mathematical solutions for the wrong scientific questions. For those who see Cattell's work in this way the madness in his method is best defined as "great anger."

In discussing his futuristic plans for mixing multivariate methods with experimental data, Cattell can be enthusiastic and entertaining. Many experimentalists, clinicians, and quantitative scientists get caught up in his vision of a marvelous journey through the uncharted psychological territory, and his promise of the wild riches which await only the most serious factor analyst. For some, such talk of a brave new world evokes the science fiction vision of a Hari Seldon statistically planning for the "psycho-historians" of a "Second Foundation" (Asimov, 1982). Others view Cattell as having the prescience of a modern day Bacon—with a "Novum Organum" called "scientific factor analysis," and a "New Atlantis" called "Beyondism." (1972a). For those who see Cattell's work in these ways the madness in his method is best defined as "wild excitement."

In this essay I have avoided mathematical and statistical criticisms of Cattell's positions that represent the paradoxical dilemma of applying modern standards to historical work. I have chosen to view most of Cattell's failings as places where, "Young men are fitter to invent than to judge, fitter for execution than for counsel, and fitter for new projects than for settled business." (from F. Bacon, "The Essays," 1597). But I have adopted this liberal stance because I think I now understand some of the "whys" in this madness: why with lunacy Cattell voices his strong allegiances; why with great folly Cattell practices the impractical; why with great anger Cattell criticizes the disbelievers; and why with wild excitement Cattell plans for future journeys. While this is clearly madness, I am convinced that it is the madness of great wisdom.

For over five decades now Raymond B. Cattell has been an outspoken advocate of a few fundamental navigational principles for psychological research. For those of us who see Cattell's work in this way the madness in his method and the method in his madness are both defined as "structural equation modeling."