XI

Rebuttals and Comments

1. *Introduction*

The Central Account is an ürbild. By their very natures as world views, ürbilds *feel* right. If they contain nonsense, the nonsense feels right, and so escapes critical appraisal. It is humans that construct ürbilds, and it is human nature to defend them. The following are a sampling of defenses of the Central Account that I have encountered.

2. *"But scientists find the Central Account useful"*

Scientists who employ latent variable modeling technology do not standardly consider the issue as to whether or not the Central Account is useful, for it is an ürbild, and is presupposed in their work. The CA is not an aid to scientific work, but the very lenses through which the researcher see his latent variable modeling. The CA provides the means by which results are interepreted, and a common language with which to discuss the products of latent variable modeling. But even if applied researchers and psychometricians were aware that their employment of the CA represented a philosphical commitment, it would be fatuous to use their claims that they find the CA to be "useful" as grounds for retaining it. Such a defense would be akin to attempting to defend phrenology by noting that "individuals plan their lives according to the output from phrenological analyses." Presumably, the fundamental aim of the social and behavioural sciences is to arrive at a *correct* account of that segment of natural reality that is their focus. If this is true, then the only relevant issue in regard the use of latent variable modeling technology within these disciplines is whether or not it leads to *correct* conclusions about natural reality. *However, as it is currently employed, correct conclusions cannot be made because these conclusions are expressed in terms of a mythology, and mythologies bear not on natural reality.*

3. *Latent variable models are "quantitative criteria." In particular, a latent variable model is a definition of "causal source of the phenomena represented by variates $\boldsymbol{X}_j$, j=1..p" (CAC) or "property/attribute of the phenomena represented by variates $\boldsymbol{X}_j$, j=1..p" (CAM).*

Here, the portrayal of the latent variable model as a detector of the members of a particular class of entities is abandoned. In its place, the argument is made that the conditions $\{c_1,c_2,...,c_r\}$, equivalent to $\underline{\Omega}_T \subset M_{lvm}$, constitute a criterion of application (with respect model lvm) for *causal source of the phenomena represented by variates $\boldsymbol{X}_j$, j=1..p* (CAC) or *property/attribute of the phenomena represented by variates $\boldsymbol{X}_j$, j=1..p* (CAM). That is, some property/force/phenomenon, σ, of natural reality, signified by concept "σ", is a cause of the phenomena represented by variates $\mathbf{X}_j$, j=1..p (CAC) or a property/attribute of the phenomena represented by variates $\mathbf{X}_j$, j=1..p (CAM), if conditional on a variate **Y**, whose scores are signified by concept "σ", and produced in accord with a rule r_σ, the joint distribution of the $\mathbf{X}_j$ has properties $\{c_1,c_2,...,c_r\}$. Model lvm thus is seen as defining what is meant by *causal source of the phenomena represented by variates $\boldsymbol{X}_j$, j=1..p* (CAC) or *property/attribute of the*

phenomena represented by variates $\boldsymbol{X}_j$*, j=1..p* (CAM). Given this definition, the researcher would assemble a list of candidate properties/forces/phenomena, φ_i, signified by concepts, "φ"$_i$, find some way to produce variates, $\mathbf{Y}_i$, whose distributions are comprised of measurements with respect "φ"$_i$, and apply the criterion to each $\mathbf{Y}_i$ in turn.

Clearly, this does not even remotely resemble the actual employments to which latent variable models are put in the social and behavioural sciences. Researchers simply do not use latent variable models as definitions for concepts (*causal source*, *property/attribute*), these definitions then used to screen *phenomena* of interest for their status as, e.g., causes. And this is because researchers who employ latent variable models are not truly engaged in hunts for, e.g., causes, but instead conceptualize their work in accord with the mythology of the Central Account.

4. *Mulaik's variant of (3)*

As was seen in Chapter V, in responding to concerns about factor indeterminacy, Mulaik has suggested a program of latent variable modeling in which results from latent variable modeling analyses are employed as input into further research in which latent variable models do not play a role. Let us say that a researcher has run an initial latent variable analysis, and has decided that $\underline{\mathbf{X}}$ is, in fact, described by latent variable model lvm. As part of the analysis, he estimates the vector of variate-latent variate correlations (loadings), $\underline{\hat{\boldsymbol{\rho}}}_{X,\theta}$, and, perhaps, provides a prelimary interpretation of the latent variate. Mulaik's idea is that the researcher should then employ $\underline{\hat{\boldsymbol{\rho}}}_{X,\theta}$ in further non-latent variable analytic research (e.g., experiments, further correlational research) to identify a "variate in the world" that is the "true common factor": "The resolution of controversy over the interpretation of factors will not come about from an inspection of factor scores or correlations of candidate variables with the original variables, but will become apparent after a more careful analysis of the causal network among the variables in question. Such analysis, performed outside of the factor analysis or in later factor analytic studies with additional observed variables, should allow us to rule out some potential candidate variables for the interpretation of the factors that otherwise satisfy (2) or (3) respectively" (p.254); "...the analysis will be useless if one cannot come up eventually with additional criteria that identify a common factor with a variable in the word" (Mulaik, 1996, p.661); "One identifies the common and unique factors of the analysis with variables in the world one believes have these same correlations among themselves and with the observed variables. But the indeterminacy of the common factor model means that variables in the world answering to this description are not uniquely determined by this description. So, whatever interpretation one gives to the factors need not be unique. Other researchers may form equally viable interpretations" (1990, p.55). In certain installments of this argument Mulaik has described this search as being for the true causal source of the $\mathbf{X}_j$ and in others he has described its aim as being to distinguish between the single "true factor variate" and the many impostors, all of which are contained in set *C*.

Mulaik's position is related to the view that to test a latent variable model is "to test whether there is any possibility whatsoever that the data arose in the manner represented by the model". But it is also a variant of (ii). Mulaik's proposal goes further than (ii) by providing a next step in the event that such a test yields an answer in the affirmative. To provide a reasonable critique of Mulaik's proposal it will be necessary to reorient his account, for, as was discussed

earlier in this chapter, and as is clear from the use of the expression "variate in the world", Mulaik seems to mistakenly believe that variates are elements of natural reality, the subject matter of science. A paraphrase of Mulaik's proposal might run as follows:

a) A set of random variates $\underline{\mathbf{X}}$ are described by latent variable model lvm, and the vector of variate-latent variate correlations (loadings), $\underline{\rho}_{X,\theta}$, is estimated to be $\underline{\hat{\rho}}_{X,\theta}^{*}$.

b) Research then focusses on a set of variates $\{\mathbf{Y}_1,\mathbf{Y}_2,...,\mathbf{Y}_n\}$, the scores on $\mathbf{Y}_j$ produced in accord with rule r_j of score production, and signified by some concept "φ"$_j$. That is, the scores that comprise the distribution of $\mathbf{Y}_j$ are measurements with respect property φ_j. The variates themselves are certainly not constituents of natural reality, but rather are created by humans to represent, in statistical analyses, the distribution of properties φ_j, j=1..n, in the population *P* under study (and with all of the attendant distortions that such representations entail).

c) The investigator screens each $\mathbf{Y}_j$, by calculating the vector of correlations, $\underline{\mathbf{r}}_{X,Yj}$, between $\mathbf{Y}_j$ and $\underline{\mathbf{X}}$. If, for a particular $\mathbf{Y}_j$, say $\mathbf{Y}_*$, $\underline{\hat{\rho}}_{X,\theta}^{*}$ and $\underline{\mathbf{r}}_{X,Y*}$ are judged to be "close enough", then the property φ_* becomes of primary interest as a candidate for the title of common factor to $\underline{\mathbf{X}}$. Further research (perhaps experimental research), now focussing on $\mathbf{Y}_*$, is carried out to further investigate the possibility that φ_* is, in fact, a cause of the phenomena represented by $\underline{\mathbf{X}}$.

Problems associated with Mulaik's position were documented in Chapter V, including the fact that Mulaik would require a new criterion to fix the sense of the concept *cause of the phenomena represented by* $\underline{\mathbf{X}}$. What must be emphasized is that Mulaik's suggested approach plays no role in current practice. Psychometricians and applied researchers do not engage in this kind of research when they employ latent variable models in their work. Instead, they carry out one-shot analyses "explained" in terms of the Central Account, and there is a very good reason for this: Latent variable modeling is undertsood in terms of the CA, and, hence, is tied to the belief that when $\underline{\mathbf{X}}$ is representable by some latent variable model, a single unobservable property/attribute (causal source) of the phenomena represented by $\underline{\mathbf{X}}$ has been detected. The sense of unobservability in play is a metaphysical one, and, hence, has no bearing on the study of phenomena. The CA further explains to the researcher that, when he decides that he has detected an unobservable property/attribute (cause), the inference is made that a particular set of manifest variates is described by a particular latent variable model, he must "interpret the latent variate", and, aside from unavoidable uncertainty over the correctness of his interpretation (the decision about the name of the property(cause) he believes he has detected), his job is done. If, with appropriate modification, Mulaik's program were to become the received program, then there would be no need for the Central Account, nor, in final analysis, latent variable models. The researcher would just go about screening variates created to represent particular phenomena for their ability to render statistically independent sets of input variates.

5. *"But mere linguistic concerns are irrelevant to progress in science"*

The chief concern of the current work has been the legitimacy of the claims made by latent variable modellers. This concern has necessitated that a great deal of attention be given to conceptual issues, for claims are expressed in language, and, hence, in terms of concepts. Such claims can be judged as true or false only if the concepts in terms of which they are expressed

are correctly employed. Some will undoubtedly continue to believe that mere language is irrelevant, and that, in the end, regardless of what is *said*, the correct view will ultimately reign. One reviewer noted that "computer programs don't care about metaphors and other linguistic devices", and, while true, it must be remembered that it is not the computer, but rather humans, that makes claims regarding the powers of latent variable models, but rather humans. Nor is it the computer, but rather the human, that reacts to such claims, constructs programs of science based on such claims, etc. And humans most certainly can be taken in by their language. To quote another reviewer, "This is just about semantics, and semantics are trivial. In the end, repeated application of latent variate models will lead to greater and greater understanding as to what we are getting at when we use these models." But this view is mistaken, for facts can be expressed only in language. Scientific thought is possible only because of the availability of concepts by means of which it can be expressed. The "mere" words and their rule-governed connections constitute our conceptual scheme. We give articulate expression to our thoughts and knowledge about the world by means of language. The sentences we use to describe our experience and the objects of our experience are made up of words combined in rule-governed ways. If the language is misused, if concepts are misemployed, then the result is not still propositions whose truth can be worked out via empirical research, but, rather, nonsense. And nonsense makes no claims that could be put to the test in a program of science.

6. *Confirmatory models*

It is likely that some psychometricians will argue that, in contrast to the earlier exploratory versions of latent variable models, confirmatory models *do* specify in advance the precise identity of the referent of *latent variate to* ***X***. But this is mistaken. In a confirmatory analysis, the researcher merely places more constraints on the moment conditions that must hold between manifest and latent variates. To do so does not, and cannot, fix the sense of the concept *latent variate to* ***X***, for the sense of this concept can be fixed only in language. In employing a confirmatory model, one has not tested an hypothesis that some particular entity or property (as alluded to in the confimatory hypothesis) is "responsible for" **X** because the modeling exercise is not preceded by the laying down of a rule that fixes the correct employment of any such a concept, nor, then, is it based on measurements of a property denoted by any such a concept. Confirmatory models allow the researcher to exercise greater control over the properties that a random variate must possess in order that it is a latent variate to **X**.

7. *"The CA provides an essential account of measurement error"*

As part of the indeterminacy debate, McDonald (1996a, p.597) has commented that "As I see it, Schonemann and Steiger faced a Scylla and Charybdis problem. To escape factor indeterminacy, they must render themselves unable to give an account of error of measurement, even in the simplest cases." In expressing their preference for latent variable over component models, Bentler and Kano (1990, p.68) state that "We would argue that the error structure of component analysis is not appropriate to most real applications, and that the search for appropriate models should typically be limited to models with uncorrelated additive errors." There is a strongly held belief that latent variable models serve an essential role of enabling researchers to account for measurement error. But it is not the latent variable model per se that gives an "account of measurement error", but, rather, the Central Account, and, in particular, the

CAM. Is it correct to say that the latent variable model paired with the CA provides an account of measurement error?

To provide an account of something, one must actually have something to account for. If one wishes to account for measurement error with respect the measurement of the κ (a property) of δ (members of some class), then one must be able to stipulate what it *means* to correctly measure the κ of δ. But to be able to stipulate what it means to correctly measure κ of δ presupposes that the concept "κ" is embedded in a normative (rule guided) practice of measuring the κ of δ. And given the existence of a normative practice of measuring the κ of δ there would exist, as part of this practice, rules r_κ for the measurement of the κ of δ, units in which to express these measurements, and grammatical rules governing the correct formation of measurement sentences involving "κ". Given a normative practice for the measurement of the κ of δ, there is a *sense* to the claim "this number is a measurement of the κ of δ^* (some particular member of the class of δ)". The claim could be paraphrased as "this number was taken in conformity with rules for measuring the κ of δ", a claim whose correctness would be judged by comparing behaviour (how the number was actually produced) to rules (how a number must be produced for it to be a measurement of the κ of δ). Indeed, if an individual flouts the rules for the measurement of the κ of δ, then he has not taken a measurement of the κ of δ^*. The existence of a normative practice of measuring the κ of δ^* gives a sense to the expression *correct measurement*, and, hence, *mismeasurement* and *accuracy of measurement*. The practice may have evolved "vertically", with more sophisticated techniques, capable of delivering more accurate measurements, replacing less sophisticated techniques. And, certainly, it is possible to repeatedly follow the rules r_κ for the measurement of the κ of δ, thus yielding a sequence of measurements of the κ of δ^*. Various kinds of latitudes allowed by the rules r_κ, along with fluctuations in the physical background conditions under which the measurements were produced, will assure that there will be variability within such a sequence.

But these ingredients of normative measurement are precisely what are missing from the practice of latent variable modeling. And in their place is instituted the mythology of the Central Account. Latent variable models are not detectors of properties, and, hence, provide no platform on which the notion of "correct measurement of property κ" could be given a sense. Without a sense to "correct measurement of property κ", there can be no sense to the notion of "error in the measurement of κ".The lack of a sense to the concept *measurement error* as it arises in latent variable modeling explains the equivocation it often produces. McDonald (1996a, p.596), for example, comments that "It does seem that Guttman felt constrained by his own rhetoric to discard, not only the common factor model, but all psychometric models containing an account of "error" or "deviation."" But latent variable models do not provide an account of measurement error, and it is not the least bit clear in what sense *measurement error* is synonymous with *deviation*, or even what McDonald means by either expression. Moreover, given the various senses of the term *deviation* that arise in psychometrics, it is preposterous to imply that the *only* quantiative treatment possible of "deviation" is that provided by the latent variable model. Measurement error is not defined in the employments of latent variable models, because what is to be measured is not specified. Once again, it is as if to incant "error structure" when looking at the Ψ term of the decomposition $\Sigma=\underline{\Lambda\Lambda}'+\Psi$ makes Ψ an "account" or "representation" of error.

8. *"The employment of latent variable models may stimulate thought or discussion in regard phenomena of interest, and lead to insights that are of use in their own right."*

Let's say that a researcher employs a latent variable model and the results suggest to him that "anxiety is a cause of anorexia". The suggestion that anxiety might be a cause of anorexia stimulates him to embark on a program of research whose aim is to explore this possibility. He has followed the tradition of interpreting the output of his latent variable analysis in terms of the CA, but realizes that, on its own, this interpretation has no scientific merit. He realizes that, in the first place, the expression "anxiety is a causal agent of anorexia", while having a nice ring to it, lacks a sense. Thus, he begins by clarifying the rules of correct employment of each of the concepts *anxiety* and *anorexia*, so that he will be in the position of knowing *what* it is that he is to study empirically. For to investigate a causal conjecture, e.g., that A is the cause of B, requires that there be a means of antecedently identifying A and B, and this, in turn, requires a grasp of the rules of employment of the concepts, "A" and "B", that denote A and B, respectively. He then clarifies what he means by *cause*. Perhaps, here, he employs a technical sense of the term, e.g., Holland and Rubin's model for probabilistic causal inference (see, e.g., Holland, 1980). A program of experimentation eventually confirms the hunch that originally arose in his latent variable analysis: Given background conditions {...}, anxiety (concept *anxiety* defined as {..}), does, indeed, cause anorexia (concept *anorexia* defined as {..}), with concept *cause* defined as {..}.

The latent variable modeling exercise *was* then of use. The researcher, of course, can report in his published work the grand causal finding and need not mention the original latent variable modeling exercise (nor, certainly, the Central Account). It was relevant to the formulation of his thoughts on the matter of the causal origins of anorexia (a tea leaf reading or revelation while taking the bus might have done the same), but is of no scientific interest in its own right. In particular, the truth or falsity of his causal case does not rest, and could not rest, on the output from the latent variable modeling. The point is that it is clear what the researcher is claiming, and what he has found, because his claims and finding have a sense given to them by his careful clarifications of the concepts which enter into these claims. He has clarified his concepts, and, hence, is clear on the phenomena to be studied, to wit, phenomena denoted by these concepts, and, hence, is open to making discoveries about these phenomena. That is, he is able to engage in scientific work, a pastime deprived the researcher whose work is enmeshed with the Central Account.