Multivariate Behavioral Research, 31 (4), 603-616

Meaning and Mythology in the Factor Analysis Model

Michael D. Maraun
Simon Fraser University

Wittgenstein (e.g., 1967) claimed that many scholarly confusions are induced by the "mythology in the forms of our language", by metaphors and analogies that beguile their users, by grammar projected onto reality, etcetera. Fields that involve mathematics (both applied and pure) are traditionally rich ground for the growth of language induced confusions, for symbolic representations, while notable for their compactness, are singularly able to mislead. Indeterminacy is a paradigm case of *philosophical* confusion in psychometrics, and most of the trouble arises from a failure to see factor analysis for what it is (i.e., to get past its mythology). The "pictures" that inform factor analysis, and make it such an intriguing model, are, at the same time, powerful beguilers, having the tendency to persuade the investigator that the facts of indeterminacy must answer to the pictures. The issue of indeterminacy ultimately speaks to the kinds of *claims* that can be made when **Y** is described by the model. And it is mainly on this issue that problems arise, for psychometricians have engaged in the dangerous practice of speaking of *the* factor, *unobservability*, *latency*, *causes*, etcetera, without being clear about what is meant. The predicate *unobservable*, for example, is standardly applied to *objects*, and when it is imported to describe a *variate* one is indeed concocting a dangerous potion. What makes a variate *latent* versus *manifest* is left unclear, and is then fully sublimated, allowing for factor analysis to be incorrectly cast as a tool for the *discovery* or *detection* of (possibly) causal entities that are, *in some sense*, unobserved or unobservable. In my article, I show that the criterion for latent common factor to **Y** (hereafter LCF to **Y**) is laid down internal to the model (just as in PCA), and that latent common factors are constructions (just as in PCA), of which there is potentially an infinite number. My aim, however, is certainly *not* to disparage the model, a model of no greater significance than PCA, a model that may be useful or useless in a particular context, but to see it for

---

This research was supported in part by a President's Research Grant awarded to the author by Simon Fraser University.

Correspondence: Dr. Michael Maraun, Dept. of Psychology, Simon Fraser University, Burnaby, B.C., Canada V5A 1S6. Fax: (604) 291-3427.

what it is. In effect, to demystify it. Only then will indeterminacy be a non-issue. I will comment here on a number of issues that have, and continue to be, especially problematic in the rendering of a clear picture of indeterminacy.

The Random Variate

It is somewhat surprising that the concept of random variate should be so contentious an issue. But as the reviews suggest, indeterminacy has a way of inspiring disagreement. Bartholomew (1996) claims that the ASP is founded on a misunderstanding of the random variate concept. According to him, the ASP is a mathematical model that contains no random variates. But this claim is false, and may well reflect a confusion over the terms of a statistical model. To refresh, X is a symbol for the concept "LCF to $\mathbf{Y}$", a concept that has specific rules of application. However, in contrast to a manifest variate, there are no rules of common language for the application of "LCF to $\mathbf{Y}$" to particular random variates. Instead, the criterion of identity is laid down internal to the model. We are therefore in need of an explication of this criterion. It turns out that the criterion *establishes* the constructions $X_i = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} + pS_i$ as LCFs to $\mathbf{Y}$, and these variates are indeed random variates. The situation should not be so intolerable to Dr. Bartholomew, for it is akin to asking for the nature of the random variate known as the *residual*, $\mathbf{e}_{\text{lin}}$, in the linear regression model $\mathbf{Y} = Y_{\text{lin}} + e_{\text{lin}}$, and, in response, providing the following construction: $\mathbf{Y} - \mu_Y - \boldsymbol{\sigma}'_{yx}\mathbf{\Sigma}^{-1}{}_x(\mathbf{X} - \boldsymbol{\mu}_x)$. Inquiring as to the nature, or construction, of a random variate hardly renders a model "mathematical". Furthermore, the symbol X in the factor analytic equation manages to bewitch if the pychometrician believes it to fix the sense of that which it is a symbol for (as if the shape of the sign + fixes what addition is). McDonald (1996) claims that his 1974 article establishes that the *constructions* aren't random variates, while Schönemann (1996) claims that the *factors* aren't random variates because the relation from the test space to the factor space is many-many. On page 216 of McDonald 1974 we have: "Different values of ξ correspond by definition to mutually exclusive outcomes in the sample space of which ξ is (uniquely) a function. By definition of an outcome space, we cannot say that one point of it corresponds to two distinct sets of coordinates ξ and ξ^*, say." But this is a bad move, once again manifesting beguilement with the symbol ξ. For indeed, ξ is merely a symbol for a concept (LCF to $\mathbf{Y}$) that stands for (potentially) an infinity of distinct random variates. As McDonald states, any number of random variates may be defined on a sample space without fear of contradicting the "foundations of mathematical statistics", in which case they have a joint distribution, as do any possible set of LCFs $X_i = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} + pS_i$. Schönemann (1996) too seems to forget that indeterminacy implies an *infinity* of possible maps from the test space to the factor space. Finally, Rozeboom (1996) claims that I ignore the diversity of models within the random-factors and fixed-scores categories, and refers pejoratively to the sampling-theoretic "security blanket" manifest in dealing exclusively with the random common-factors model. Obviously there is truth to this, but of course one can choose to focus on any model one pleases. I chose the random common-factors model because it is in keeping with Bartholomew (1981) and Williams (1978) whose treatments were, at least initially, my point of departure.

The Latent Variate Concept

McDonald (1996) states that the definition I provide for *latent variate* is incorrect. According to him a latent variate is "defined by the principle of local independence" (p. 595). But this is clearly not the case, for this characterization, nothing but the general notion of statistical independence, in no way distinguishes a latent variate from a manifest variate (any one of which may, of course, render $\mathbf{Y}$ conditionally independent). Furthermore, while I fully realize that the treatment of most psychometricians begins and ends with a hand-wave towards local independence, implicit in my article is the claim that they have not understood the *grammar* of the latent variate concept well enough to avoid falling into an array of conceptual traps. This claim renders my position quite distinct from that of merely subverting, or ignoring what McDonald takes to be the normative version of this concept. Rozeboom (1996) suggests that the term *latent common factor* is a locution. But this is not the case, as is made clear by a cursory survey of the literature (e.g., Bartholomew, 1981; Loehlin, 1990, p. 28; McDonald, 1974, p. 216; Mulaik, 1993; Williams, 1978). But more essentially, regardless of the term one attaches to X, my explication accurately clarifies the grammatical differences between the concept symbolized by X and the concept of *manifest variate*. Mulaik (1996) also takes issue with my characterization of the concept of latent common factor to $\mathbf{Y}$ because "the latent common factor ... has no criterion of identity, at least not one that would pick out just one variable, whereas in component analysis the component variable is singled out uniquely." (p. 586). But the *number* of latent common factors that can be constructed in a particular application is irrelevant to the *criterion* for LCF to $\mathbf{Y}$. In terms of realizations and extensionality, components and factors have the same status (they are both constructions).

Interpretation and Criterion

Mulaik (1996) seems to generally agree with my characterization, but prefers to view indeterminacy as a matter of interpretation: "Maraun's problem with "interpretation of *the* factor" is that he forgets that there *is* but one common factor in his model ... although indeterminacy means there may be multiple interpretations ..." (p. 584). Why is indeterminacy *not* a matter of distinct interpretations? In the first place, an interpretation is *of* something, and so presupposes a criterion for the concept that denotes that which is being interpreted. Now if what is being interpreted in Mulaik's view is an LCF to **Y**, then we need a criterion for this concept. But the criterion for LCF to **Y** establishes each of $X_i = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} + pS_i$ as *criterially* an LCF to **Y**. They are not interpretations of anything, but instead *are* LCFs to **Y**. Hence, there are indeed many LCFs to **Y**, and indeterminacy is not a matter of a multiplicity of distinct interpretations. But more generally, it is not easy to find coherence in Mulaik's framing of indeterminacy as an issue of interpretation. Let's examine the issue in a similar context, Schönemann's (1996) Equation 1. Equation 1 has, of course, two solutions, 99 and 1. Assume that it is declared that the existence of two solutions is really a matter of distinct interpretations. What could such a claim mean? That 99 and 1 are distinct interpretations? Both 99 and 1 *are* solutions, not interpretations. An interpretation might, on the other hand, legitimately involve the conceptualization of the solutions as the values of T when $\mathbf{Y} = T^2 - 100T + 99$ equals 0, or, if function **Y** is intended to model a real-world scenario, might mean that we take the solutions to be features of this scenario (map them onto reality as it were). The point is that an interpretation is *of* a state of affairs, while indeterminacy *is* a state of affairs. So while factor analysts do indeed engage in many forms of interpretation (of their data, their results, etc.), indeterminacy is manifestly *not* about the non-uniqueness of interpretation. It is a grammatical issue, and so is logically prior to any form of interpretation. McDonald (1996) incorrectly asserts that factor indeterminacy, as I treat it, has no implications for factor interpretation, and that my treatment implies that the interpretation of factor loadings is external to "the model". Instead, what I correctly claim is that, for reasons given previously, interpretation (of the results, of the loadings, etc.) is external to the issue of *indeterminacy*, which rests on the criterion for LCF to **Y**. As an aside, what might be meant by "interpreting the loadings"? One might provide an interpretation of the loadings as the correlations of manifest variates with any of the LCFs $X_i = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} + pS_i$, as regression weights, or, equivalently, as the slopes of the regressions of the manifest variates on any of the LCFs. These interpretations are perfectly coherent.

On the other hand, what I correctly object to is the belief that one is inferring the nature of *the* (unobservable) factor by examining the loadings. An LCF may be examined directly by examining any of the $X_i = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} + pS_i$.

The Place of Metaphor

Mulaik (1996) and Rozeboom (1996) view metaphor as the "driving force" behind applications of factor analysis, with Mulaik claiming that the common factor model is inspired by what he calls the "object metaphor". I couldn't agree more. But metaphors have a habit of getting away on their users, and this is exactly what has happened with the object metaphor of factor analysis. The lack of careful handling of this metaphor has led many a psychometrician to impute to factor analysis powers that far outstrip what the model can actually deliver. It is essential that model generating metaphors be distinguished from the meanings of factor analytic concepts. This is one of the jobs of conceptual clarification. Metaphor is, in a sense, a technique for picturing the world, while meaning is *constitutive* for (and thus prior to) the development of metaphors. Indeterminacy, centering on the criterion for LCF to **Y**, has severe implications for the claims that can be made when using factor analysis, in particular those about "factors". The model generating metaphors and analogies of factor analysis do not bear on this issue (because the issue is grammatical), and in fact provide a substantial obstacle to sorting out legitimate claims about factor analysis from nonsense. It is interesting that factor analysts tend to make big claims about the powers of the model until these claims are subjected to careful scrutiny, at which point the scrutinizers are chided for taking things too literally (cf., Schönemann, 1987). Contrary to McDonald (1996), one does not need to "guess another's mental processes" in order to make a sound case that aspects of a metaphor have been conflated with a criterion. Category errors such as these are standardly *manifest* in scholarly work. My correct observation is that the "object metaphor", in Mulaik's terms, has often been conflated with what the model actually delivers, and, in particular, with the concept of LCF to **Y**. My claim about McDonald (1974) is based in part on the following: "The same contradiction is contained in any attempt to say that different values of ξ can be associated ["at the same time", is understood throughout this discussion] with one subject" (McDonald, 1974, p. 216). One does not both wear and not wear earrings at the same time, one does not both own and not own a house at the same time, hence (via an apparent confusion over the grammar of substantive expressions) one does not *have* different values of *the* factor at the same time.

Reactions to Grammar are External to Grammar

Mulaik (1996) states that I appear "unwilling to consider how we arbitrarily and often implicitly impose additional rule-based constraints to resolve these indeterminacies in ways that allow us to get on with our research" (p. 579-580). But this is not correct. It is just not the issue with which my article deals. And there is a very good reason for excluding such "next-steps", or reactions to the fact of indeterminacy: It is hard enough just to get straight what the facts are (witness the great range of disputes, often irrelevant, and confusions that spring up when indeterminacy is considered). Hence, it is essential to clear away the extraneous details, and focus on the central issue, the criterion for LCF of **Y**. He also states that "Neither I nor Maraun nor others of the alternative solution position have produced clear and scientifically important examples of alternative interpretations for the factors that demonstrate the importance of factor indeterminacy" (Mulaik, 1996, p. 584). But Mulaik needs to get straight the fact that I may work with any of one the $X_i = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} + pS_i$ and attempt to show how it is "scientifically important" in any number of ways. Regardless of what I succeed in showing empirically I am still working with one of the LCFs to **Y**. Even if a particular X_i turns out to be unimportant in some sense, it is still an LCF to **Y** (it is an *unimportant* LCF), just as a component is a component regardless of what is done with it. The fact that there are potentially an infinity of LCFs to **Y** when **Y** is described by the model might simply be an unfortunate cost of the supposed virtues gained in using factor analysis over PCA. What did psychometricians think that extra term would cost? Rozeboom (1996) also is concerned with "best" choices and discusses the possibility of choosing LCFs that are closest to the supposed causes of the manifest variates. Best choices are, once again, secondary moves in a game that begins with an understanding of *what* we are choosing among, that being the LCFs $X_i = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} + pS_i$. Schönemann (1996), on the other hand, expresses a somewhat higher opinion of the infinite variables position than what he takes to be my view. But I am not *against* this position, but instead view it as an entirely different model (much as in ANOVA) (see Steiger, 1990, 1996, for the correct viewpoint). If this *is* the model in play, if indeed this is the model that should be viewed as generic common factor analysis, then we need just be clear about it, and get on with an explication of *it*. The danger is in allowing the issue to remain ambiguous, and then taking the infinite variable domain invention as a *remedy* to the original problem (which pertained to a different model).

The Difference Between Meaning and Fact

The issue of indeterminacy crosses paths with many philosophical issues native to psychometrics, among them the issue of whether empirical investigation has anything to say about meaning. McDonald (1996) views Guttman's claim that correlation is irrelevant to definition/meaning to be authoritarian and gnomic. He then calls to the authority of other test constructors to verify that "abstract conceptualization and empirical evidence" are, in fact, both relevant. While no one could doubt that in psychometrics and psychology McDonald's picture is accepted practice, the big question is whether it should be (i.e., is it legitimate). While Guttman never provided a detailed philosophical rationale for his claim, it is based on far more than mere authoritarianism. The husk of a rationale is as follows.

1. *The meaning of a concept is grammatical.* The meaning of a concept is manifest in its rules for correct application (Wittgenstein, 1953). This is implied by the fact that we in fact can understand the meaning of a concept, be correct or incorrect in our use of concepts, teach and learn the correct use of a concept, cite rules (standards of correctness) when disputes over meaning arise, etcetera.

2. *A rule is autonomous and non-discoverable.* In a given context, the only thing that could establish the claim that "there is a rule ϕ" as correct or incorrect is a comparison to the rule itself (if there is a rule in play at all). It is obvious, however, that such a comparison *presupposes* an understanding of the rule itself. Hence, there is no such thing as empirical evidence establishing the nature or existence of a rule. Rules are autonomous and non-discoverable (Wittgenstein, 1953; Ter Hark, 1990). Instead, rules are taught, learned, violated, etcetera.

3. *The meaning of a concept is autonomous and non-discoverable.* The meaning of a concept is laid down in the rules of grammar, and rules are non-discoverable. Hence, empirical evidence cannot establish, reveal, or clarify the meaning of a concept (although it can certainly *motivate* investigators to modify their concepts). On the contrary, rules are *constitutive* for empirical evidence: *These* empirical findings are not about τ at all unless they are in fact denoted by concept "τ". And to make *this* case, one already requires an understanding of the rules for the employment of "τ", that is, its meaning. McDonald should consider what makes empirical evidence *relevant* to the task he envisions. For the correlation between the sets of numbers x and y is not the correlation between, for example, height and weight, at all, unless x and y are *already* justifiable *as* measurements of height and weight. And such a justification presupposes an understanding of the meanings of *height* and *weight*. Hence, relevant empirical results are, so

to speak, "gifts" of having already settled questions of meaning. The same line of reasoning is what reveals the incoherence of the notion that concepts are (under)determined by "perceptual experience". A concept's meaning is given by the rules for its correct application. Rules are constitutive for its meaning. Hence, meaning is not about *knowing* or *perceiving*, but is instead constitutive for knowing and perceiving (e.g., one knows about the *weather*; and what is meant by *weather*?). I do, however, agree with Mulaik (1996) that invariants in perceptual experience are a precondition for the application of rules, though this is an entirely different matter.

Signals and Factors

Steiger (1996) provides an excellent example that *shows* why the posterior moment position is incorrect. But I wonder why Steiger thinks that factor analysis answers his Question 1 (p. 540). To claim that the model tells one about the amount of noise present in a signal presupposes that one has a standard of comparison, and the only standard of comparison is the signal itself. That is, what, in Steiger's example, enables one to conclude that the model provides information that is in agreement with the actual error in the recordings is that one *has* direct knowledge of the signal. But in the uses to which the model is standardly put, this knowledge is not present. If it were present, the model would then be superfluous.

On Rozeboom

Rozeboom's (1996) general condemnation of my article includes the claim that I ignored the diversity of indeterminacies inherent to any given factor analysis model. This is not true. I chose to focus on one particular kind of indeterminacy because it is more problematic than other forms. The "classical" indeterminacy centering on the latent common factor *is* a special case, as can be deduced from the many years of debate it has stimulated. And this is so because it is strongly tied in with factor analytic mythology, and hence is more likely to create confusion. Rozeboom's perplexity over the attention given this problem is telling, for while this indeterminacy is problematic *because* of the potential to conflate metaphor and meaning, his native empirical realism has never been able to keep separate conceptual and empirical issues, instead ploughing ahead with a philosophically vacuous insistence that theoretical terms denote "unobservable" causal entities. He is especially troubled by my "failure" to comprehend the inductive logic by which explanatory concepts originate and evolve. Alas, here too we are off to a very bad, but predictable, start. For here we are knee-deep in the

standard empirical realist conflation of empirical and conceptual issues: Philosophy as science; conceptual clarification as a set of progressive epistemological moves approximating to some shadowy "truth" about a concept; in short, the stuff Wittgenstein (1953) summarily routed in the first half of this century. The meaning of a concept, both in science and in common language, is manifest in its rules for correct use, rules laid down in grammar. Inductive logic, and its empirical fodder, at best may *motivate* investigators to make alterations to the grammar of the concepts they employ. Hence, if inductive logic is relevant at all to a concept, it is merely as part of the anthropology of the concept's development, and as such does not even constitute a philosophical issue (until confusions like Rozeboom's arise). Just as telling is Rozeboom's conclusion that the problem of classical indeterminacy has much to do with "our flaccid grip on the logic of causality and the ontology of scientific variables" (Rozeboom, 1988, p. 225). This is misguided and merely parallels his past problems in distinguishing grammar(meaning) from, among other things, hypothesis, existence, and theory (e.g., Rozeboom, 1960, p. 364). Prior to the coherent phrasing of questions of ontology, let alone causation, the meanings of the concepts that will organize such work must be made transparent. Indeterminacy is a conceptual issue because the grammars of factor analytic concepts have been left unexplicated. This causes philosophical confusions to arise. To phrase indeterminacy as an issue of causality or ontology is like trying to determine the winner of a game for one does not understand the rules.

Consider Rozeboom's (1996) "Luser" example. He believes that, on my account, the specification of a criterion for Luser somehow entails the claim that "the definition of 'Luser' already tells us all there is to know about Lusers" (p. 566). But this is incorrect. What *is* true is that a coherent empirical investigation centering on "Lusers" would be *predicated* on the criterion for "Luser". But the rules of application of the predicate "Luser" say nothing *about* Lusers, but instead establish what is a Luser. There is nothing to speak of without the criterion, for it is constitutive for a discussion of Lusers. Later, we are treated to another bout in which Rozeboom asserts that "... if two distinct entities X_1 and X_2 are both LCFs to Y, a criterion of X_1's individualized identity should in principal distinguish X_1 from other things, X_2 in particular ..." (p. 568). Unitary reference, however, is *shown* to hold empirically, but with *respect* to the denotative concept in question. So there is no question of unitary denotation "in principle". The number of things to which a concept can be applied is not deducible from its grammar. But more fundamentally, if one insists on "suitably restricted contexts of usage" one has not achieved unique reference with respect to the original predicate, but has in fact changed the rules for the application of the predicate (i.e., changed its grammar). Rozeboom states that: "Maraun's frequent reference to *the* common-factor is less objectionable than a strict grammarian might insist, insomuch as a locution of the form 'the *P*' wherein '*P*' is a predicate with many exemplars may indeed achieve unique reference in suitably restricted contexts of usage" (p. 556). As if "context" can be divorced from the grounds for correct application of a concept! Once again, if the context of application is necessarily restricted then the criterion of application (i.e., the concept's meaning) has effectively been changed.

This brings us to the crux of the matter. Rozeboom (1996) portrays my case as being that if we attribute to purported factors any properties not entailed by a given application's *LVr*( ) (his symbolism), "we are not speaking of common factors in this context" (p. 566). He also claims that my exclusion of a signal from being an LCF to **Y** is much like "insisting that familiarity with my wife precludes my discovering that she is a closet Luser" (p. 567). But he misunderstands. The issue is certainly not the attribution of properties to LCFs to **Y**, but instead that the criterion for LCF to **Y**, its rules for correct application, excludes certain things from *being* common factors ("LCF to **Y**" can't be correctly applied to certain things). The signal cannot be a common factor because the model does not play the role of a test of the impact of *any* variate's impact on the manifest variates. It does not involve *any* analysis of possible causes. Hence, to enshrine the signal as the LCF to **Y** is not to have made an empirical error (e.g., the case was not strong enough), it is to have confused distinct concepts (i.e., concepts with different grammars). Rozeboom's problem is that he, in confusing conceptual and empirical aspects of investigation, believes that there is always the *possibility* that, once we advance our understanding a little more, something like the signal might *really* turn out to be the factor. But grammar fixes sense, and hence makes certain claims not *impossible*, but *incoherent* (just as the grammar of colour terms makes it incoherent to assert that an object is both red and green at the same time, the grammar of *dominance* makes it incoherent to assert that Joe is very dominant but has never *behaved* dominantly, and the grammar of *mind* makes Cartesian dualism incoherent [mind is not a substance]). Factors are indeed not disembodied spirits. They are constructions, and hence are excluded from consideration as possible causes, cannot be described coherently as unobservable, etcetera. And this is an internal (grammatical) matter: What makes a particular variate an LCF to **Y** is that it was in fact *constructed* as such (just as what would make 6.2 a measurement of the height of a tree [in feet] is that it was taken according to the rules for the measurement of height). There is no empirical issue here. The problem is that Rozeboom's work manifests too simplistic a

conception of language to support an analysis of the indeterminacy issue. In particular, it is run through with aspects of the primitive linguistic theory that Wittgenstein undermined under the heading of "Augustine's picture of meaning" (see e.g., Baker & Hacker, 1980, for a detailed review). Augustine's picture takes words to be correlated with objects. The meaning of a word is the object to which it is correlated. Language is the shadowy reflection of objects in the world, it forging an (imperfect) link between thought and the world. Philosophical analysis is the progressive, ongoing search for the essence of the "objects" that are *really* what words signify. Wittgenstein diagnosed the root of this misrepresentation of meaning as centering on, among other things, confusions induced by substantive expressions (Hacker, 1986). "What is a common factor" sounds superficially like "what is a mountain", or "what is a nickel". This superficial similarity in grammatical form is all the less wary require to begin a search for the object that corresponds to *common factor*. And indeed, this explains the empirical realist insistence that theoretical terms denote causal sources, a ridiculously flamboyant, and pointless empirical *hypothesis*, that has no place in a conceptual clarification.

Finally, we arrive at Rozeboom's (1996, p. 569) interpretation of my comments on factor analysis as a litmus test. He states that "But Maraun could not have found an example more antithetical to his thesis", and that "tests such as [I] envision here have long been recognized ... as the root of *disposition* talk." In fact, Rozeboom could not have chosen a better grounds to render transparent the inadequacies of his own understanding, for his treatment is a mischaracterization of dispositions. In particular, if previously there were symptoms of Augustine's picture, this entry signals a full-scale outbreak. We are told that "... the ontology of dispositions is still in dispute ..." (p. 569), that "*DSR* merely claims the presence of *some* property with relevant causal effects..." (Rozeboom, 1984, p. 215), and that "... there is near-perfect agreement that our conceptions of these — 'identity criteria' if you like — are defined in some fashion (just how being an important technical question still open) by stories we tell about the behavior of objects in disposition-identifying circumstances." (p. 569). Where to begin? In the first place, one does not have a *test* of the occurrence or presence of anything unless one has a criterion for the concept that denotes the thing whose occurrence or presence is in question. Hence, in this context, Rozeboom misuses the concept of a *test*. An empirical test presupposes grammar, but here the issue *is* grammar (i.e., the meanings of "LCF to **Y**" and *disposition*). Grammar, which fixes meaning, does not contain any tests, but instead is a precondition for the construction of tests. Identity criteria are laid down in grammar. What makes something a criterion for ϕ is established in grammar, for a criterion is constitutive for the meaning of ϕ (Ter Hark, 1990). Hence, it is incoherent to suggest that "... identity criteria for features of distal reality discerned through their observable effects are semantically fused ... with tests of their occurrence ..." (Rozeboom, 1996, p. 570). Identity criteria establish *what* there is to discern, and what one might test for. One cannot even make sense of the statement "T is a test for the presence of that which is denoted by ϕ" unless the meaning of ϕ is already clear, and to understand the meaning of ϕ is to understand the criteria for ϕ (this is why the factor analysis model, which establishes the criterion for LCF to Y, cannot be a test for the presence of LCFs). But, in any case, this is all irrelevant to the meaning of dispositional concepts, for they do not "refer" to any entity, let alone "claim the presence of a property" (let alone one with relevant causal effects). Rozeboom's characterization is nothing more than a conceptual confusion. Humans make claims about existence, presence, and causality, but grammar, which fixes the meaning of the concept *disposition*, does not. A disposition is instantiated by certain behaviours, and the behaviours are internally related to the disposition (Ter Hark, 1990). To understand the meaning of a disposition is, therefore, to understand, among other things, its grounds for application, just as to understand a rule is to understand what behavior accords with the rule. The belief that dispositions have to do with the possession (or existence) of a property is, in Wittgenstein's terms, a confusion over the grammar of substantive expressions: He *has* a great deal of aggression (so let's find out where it is located, and whether it *really* exists). The confusion of conceptual and empirical issues occurs again and again in Rozeboom's work. For example, the idea of "disposition identifying circumstance" incorrectly implies a gap between a particular disposition and its grounds for instantiation, and begs the question as to the meaning of the disposition (consider a coherent sense for "disposition identifying circumstance" for ϕ, in the absence of an understanding of the meaning of ϕ). Similarly, one does not *hypothesize* "object x to have a disposition δ_{SR}." An hypothesis is in need of empirical support, while the instantiation of a disposition rests on grammar. If x behaves in such a way as to justify application of a particular disposition, then x *has* the disposition, this being a *grammatical* certainty.

What might *causes* have to do with dispositions? Nothing at all if the issue is the *meaning* of a dispositional concept. A causal claim about *it* is supported by *empirical* investigation, but is predicated on a criterion for the concept that denotes *it*. That is, meaning is constitutive for causal claims and investigations (Wittgenstein, 1980). One might, on the other hand, coherently consider the causes of the behaviours τ_i. But to study the causes of behaviours τ_i that are the instantiators of disposition ϕ would presuppose

the grammar of ϕ, which *establishes* that the τ_i instantiate ϕ. In other words, an investigation into the causes of the behaviours that instantiate ϕ presupposes an understanding of the meaning of ϕ. What then do Rozeboom's (1996) proliferation of schemas amount to? If the issue is the clarification of dispositional concepts then they are but examples of, in Wittgenstein's words, attempted exactness and actual irrelevance. For the meanings of dispositional concepts are manifest in their rules for correct use, and rules are normative (they are public, taught, learned, etc.), and so cannot be prescribed, but only clarified (made transparent). In fact, Rozeboom's schemas look remarkably simplistic and confused when held up against the grammar of dispositional concepts. Rozeboom is, of course, perfectly free to paint his endeavour as ongoing work in the construction of a technical notion. But then he has no business billing it as a clarification of *dispositions*. For those who are interested in an honest-to-god philosophical clarification of dispositions, read Ter Hark (1990; who also provides a full analysis of the confusions inherent to approaches like Rozeboom's; see "psychological theories of meaning"). What Rozeboom's account requires is some Wittgensteinian acid to boil down his muddle of empirical assertions and conceptual confusions.

References

Baker, G. & Hacker, P. (1980). *Wittgenstein: Meaning and understanding*. Chicago: University of Chicago Press.

Bartholomew, D. (1981). Posterior analysis of the factor model. *British Journal of Mathematical and Statistical Psychology*, *34*, 93-99.

Bartholomew, D. (1996). Comment on: Metaphor taken as math: Indeterminacy in the factor model. *Multivariate Behavioral Research*, *31(4)*, 551-554.

Hacker, P. M. S. (1986). *Insight and illusion: Themes in the philosophy of Wittgenstein*. Oxford: Clarendon Press.

Loehlin, J. C. (1990). Component analysis versus common factor analysis: A case of disputed authorship. *Multivariate Behavioral Research*, *25(1)*, 29-31.

McDonald, R. (1974). The measurement of factor indeterminacy. *Psychometrika*, *39*, 203-222.

McDonald, R. P. (1996). Latent traits and the possibility of motion. *Multivariate Behavioral Research*, *31(4)*, 593-601.

Mulaik, S. (1993). The critique of pure statistics: Artifact and objectivity in multivariate statistics. In B. Thompson (Ed.), *Advances in Social Science Methodology, Vol. 3*. Greenwich, CT: JAI Press.

Mulaik, S. A. (1996). On Maraun's deconstructing of factor indeterminacy with constructed factors. *Multivariate Behavioral Research*, *31(4)*, 579-592.

Rozeboom, W. W. (1960). Studies in the empiricist theory of scientific meaning. Part I. Empirical realism and classical semantics: A parting of the ways. *Philosophy of Science*, *27*, 359-373.

Rozeboom, W. W. (1984). Dispositions do explain. Picking up the pieces after hurricane Walter. In J. R. Royce & L. P. Mos (Eds.), *Annals of Theoretical Psychology, Vol. 1*. New York: Plenum.
Rozeboom, W. W. (1988). Factor indeterminacy: The saga continues. *British Journal of Mathematical and Statistical Psychology, 41*, 209-226.
Rozeboom, W. W. (1996). What might common factors be? *Multivariate Behavioral Research, 31(4)*, 555-570.
Schönemann, P. (1987). Jensen's *g*: Outmoded theories and unconquered frontiers. In S. Modgil & C. Modgil (Eds.), *Arthur Jensen: Consensus and controversy*. New York: Falmer Press.
Schönemann, P. (1996). The psychopathology of factor indeterminacy. *Multivariate Behavioral Research, 31(4)*, 571-577.
Steiger, J. H. (1990). Some additional thoughts on components, factors, and factor indeterminacy. *Multivariate Behavioral Research, 25(1)*, 41-45.
Steiger, J. H. (1996). Dispelling some myths about factor indeterminacy. *Multivariate Behavioral Research, 31(4)*, 539-550.
Ter Hark, M. (1990). *Beyond the inner and the outer: Wittgenstein's philosophy of psychology*. London: Kluwer Academic Press.
Williams, J. (1978). A definition for the common factor analysis model and the elimination of problems of factor score indeterminacy. *Psychometrika, 43*, 293-306.
Wittgenstein, L. (1953). *Philosophical investigations*. Oxford: Basil Blackwell Ltd.
Wittgenstein, L. (1967). *Remarks on the foundations of mathematics*. Oxford: Basil Blackwell Ltd.
Wittgenstein, L. (1980). *Remarks on the philosophy of Psychology, Vol.1* (G. E. M. Anscombe & G. H. von Wright, Eds.). Oxford: Basil Blackwell Ltd.

Multivariate Behavioral Research, 31 (4), 617-630

Coming Full Circle in the History of Factor Indeterminacy

James H. Steiger
University of British Columbia

Nearly 70 years ago, eminent mathematician Edwin Bidwell Wilson attended a dinner at Harvard where visitor Charles Spearman discussed the "two-factor theory" of intelligence and his just-released book *The Abilities of Man*. Wilson, having just discovered factor indeterminacy, attempted to explain to Spearman and the assembled guests that Spearman's two-factor theory might have a non-uniqueness problem. Neither Spearman nor the guests could follow Wilson's argument, but Wilson persisted, first through correspondence, later through a series of publications that spanned more than a decade, involving Spearman and several other influential statisticians in an extended debate. Many years have passed since the Spearman-Wilson debates, yet the fascinating statistical, logical, and philosophical issues surrounding factor indeterminacy are very much alive. Equally fascinating are the sociological issues and historical questions surrounding the way indeterminacy has periodically vanished from basic textbooks on factor analysis. In this article, I delineate some of these historical-sociological issues, and respond to a critique from some recent commentators on the history of factor indeterminacy.

Factor indeterminacy has been the subject of controversy for almost 70 years. As this special issue of *Multivariate Behavioral Research* has illustrated, it is a complex topic. Maraun (1996a, 1996b) has done an admirable job clarifying and separating many of the common statistical and philosophical positions taken by writers on the subject.

The significance of factor indeterminacy as an issue goes beyond the mere statistical, as it illustrates important aspects of the sociology of science as well. In particular, it illustrates the familiar themes of (a) how scientific progress often moves through the "path of least resistance," (b) how history often repeats itself, and (c) how unpopular points of view (and their proponents) are often demonized, ignored, and subsequently "filtered out" of popular sources.

Factor analysis is a popular technology, because it has provided its users and developers with a number of tangible benefits. In the 1940's and 50's, simply performing a factor analysis was often sufficient to obtain a Ph.D. Factor analysis offers a rich field of technical problems, of wide-ranging difficulty, to be mined by researchers. These problems kept a whole generation of psychometricians gainfully employed. Factor analysis was,