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The Claims of Factor Analysis

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The focus of this response will be, following Bartholomew's (1996) challenge, a discussion of several of the practical consequences of indeterminacy in general, and the ASP in particular. Secondarily, I will touch on a number of the points made by the reviewers in the second round of commentary on the central issue of meaning.

Claims Made on the Basis of Factor Analysis

Factor analysis is a tool used in the commission of scientific investigation, and hence those who employ it ultimately make claims about what they have learned through its employment. Now the popularity of factor analysis cannot be questioned, and thus it follows that factor analytic investigations have spawned a great many claims. Science, however, is not well served if manifest unclarity attends the concepts that enter into investigations and reporting. The indeterminacy debate "matters" just because it reveals a lack of clarity, and, at times, a basic lack of understanding of the concepts that inform factor analytic investigation, and, by implication, the claims that arise from such investigations. Moreover, the ASP is the correct take on indeterminacy and has profound implications for the claims that can be *legitimately* made about factor analytic results. I will discuss several of these.

Claims about Factors

Investigators engaged in factor analysis standardly make claims about what is meant by a "common factor to Y". The following claims are incorrect.

I would like to thank Roland Chrisjohn, Ross Traub, and Stanley Mulaik for sharing with me their views on indeterminacy.

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A) The common factors of factor analysis are *underlying variates*; *unmeasurable variates*; *hypothetical variates*; *hidden variates*; *unobservable variates*; etcetera.

These claims can be found in many technical treatments and applications. Nevertheless, common factors are none of these things. The common factors of factor analysis (latent common factors in modern parlance) are *constructed* or synthetic variates that replace the manifest variates $\mathbf{Y}$ in an optimal sense. This *follows from the criterion* for the phrase "X is a latent common factor to $\mathbf{Y}$", a criterion provided by the functional constraints (and side-conditions) given by the equations of the factor analysis model when it describes $\mathbf{Y}$. There is no other criterion for "common factor to $\mathbf{Y}$", although in the social sciences there are many other senses of *factor* (e.g., causal factors, the factors of ANOVA, the factors responsible for ..., etc.), these being distinct from the concept of common factor (i.e., external to factor analysis). The fact that the criterion is provided internal to the model is precisely what makes the factors of factor analysis *latent* (as distinct from *manifest*, which implies denotation by a concept with criterion external to the model). Researchers have unfortunately misunderstood what makes a latent variate different from a manifest variate, incorrectly characterizing it as *underlying*, *unmeasurable*, *unobserved*, and *hypothetical*. The criterion inherent to the common factor analysis model *establishes* any construction $X_i = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} + pS_i$ as an "LCF to $\mathbf{Y}$" when $\mathbf{\Sigma} = \mathbf{\Lambda\Lambda}' + \mathbf{\Psi}^2$. Hence, claims like "... factor scores derived from a common factor analysis are indeterminate in the sense that they are imperfectly correlated with the hypothetical factors" (Jensen, 1983, p.313), which imply that factor analysis deals with some *other* factor variate (e.g., a *hypothetical* variate) are confused (or perhaps represent the fanciful thinking that seems to flourish in factor analytic soil).

Indeterminacy Property

In applications involving a finite number of manifest variates, the construction formula for latent common factors grants the possibility of constructing an infinity of replacement variates each of which is fully a latent common factor to the manifest variates. This is why the ASP is correct. Depending on the particulars of the application, these replacement variates may not be very highly correlated. In fact, it is well known that the most uncorrelated pair of latent common factors, $X_i = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} + pS_i$ and $X_j = \mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{Y} - pS_i$ have a correlation of $2\mathbf{\Lambda}'\mathbf{\Sigma}^{-1}\mathbf{\Lambda} - 1$ (Guttman, 1955). Hence,

B) When $\mathbf{\Sigma} = \mathbf{\Lambda\Lambda}' + \mathbf{\Psi}^2$, claims that involve "*the* factor" or "*the* latent variate" are (at least) misleading.

To point this out is not to be overly fussy about grammar. It is instead to insist on clarity. For if this point is not kept in clear view the truth about common factors is quickly sublimated, at which point there is a strong tendency to drift into misconceptions (A) and (C) through (E) (see the following). As an aside, Schönemann (1996, p. 652) claims that common factors are not random variables because "..random variables are usually defined by *a* map" from a sample space to $\Re$. But *a* common factor, that is, a particular X_i, is just such a map. It is not a map from the test space, but probability theory does not require it to be so. The "muddle" psychologists have gotten themselves in is not over "indeterminate random variables", but over their failure to grasp the property of indeterminacy, that is, the possibility of constructing an infinity of random variates each of which is a common factor to $\mathbf{Y}$.

Claims about Factor Analysis as an Investigative Tool

The illegitimate conceptions listed in (A) and (B), often motivate or are at least interlocked with an incorrect set of claims regarding the *kind* of tool factor analysis is. The following are incorrect:

C) Factor analysis *detects* the presence or existence of causal influences on the p manifest variates.
D) Factor analysis *detects/measures* the *influence* (perhaps not causal), *presence* or *existence* of a $(p + 1)^{th}$ variate called the factor (which, in distinction from the manifest variates, is *underlying*, *hidden*, *unmeasurable*, *hypothetical*, etc.)
E) Factor analysis is a test of the hypothesis that there exists some external $(p + 1)^{th}$ variate Z such that $C(\mathbf{Y}|Z) = \mathbf{\Psi}^2_{diag}$.

The incorrectness of (C) has, at least, been acknowledged by a number of experts in the area (see e.g., McDonald, 1996). However, as implied by (D) and (E), the point is a far more general one. The reason that (C), (D) and (E) are false is that they effectively cast the model in the role of a "litmus test" for the presence or existence of a $(p + 1)^{th}$ variate thought to have a particular relationship with the manifest variates. However, the model cannot play the role of a litmus test. For a test T to be a litmus test for the presence/existence of a $(p + 1)^{th}$ variate, say Z, denoted by the concept "common factor to $\mathbf{Y}$", something like the following would be required: (a) A criterion for "common factor to $\mathbf{Y}$" so that one can identify a Z denoted as such *independently* of the supposed test of its existence/presence; (b) When such a Z is present/exists, T responds in a predictable, reliable fashion. However, in factor analysis the criterion for "common factor to $\mathbf{Y}$" is *provided* by the model itself (i.e., we have no criterion before $\mathbf{\Sigma} = \mathbf{\Lambda\Lambda}' + \mathbf{\Psi}^2$ is shown empirically to hold), and the Zs in question are constructions. Hence, the model cannot be a litmus test for the detection or discovery of a "common factor to $\mathbf{Y}$", or any other $(p + 1)^{\text{th}}$ variate. It instead provides a blueprint for the construction of variates denoted by this concept. Would one claim that in PCA we *detect* the first principal (*underlying*?) component? Of course not, because the criterion for "1st principal component to $\mathbf{Y}$" is embodied in the model itself. That is, there is no sense to the notion of "principal component" without the model (they are internally related in that each presupposes the other). This is exactly the situation that holds for factor analysis. The investigator who would like to examine issues (B), (C), and (D), that is, whether there exists some other brand of factor that is causal/responsible for $\mathbf{Y}$, will require a different approach than factor analysis. Exactly why a modest little model like factor analysis was ever thought able to deliver such a huge epistemological payload (say in relation to PCA) says a lot about the wishful thinking of a number of its most prominent advocates.

Factor Analysis and PCA

Data analytic techniques represent trade-offs of optimal characteristics. The indeterminacy characteristic of common factor analysis represents the trade-off of lower-rank approximation for the non-uniqueness of the constructed common factors. That is, the very thing so coveted in factor analysis, a lower rank approximation due to the decomposition of the "communality corrected" matrix $(\mathbf{\Sigma} - \mathbf{\Psi}^2)$, instead of the full rank $\mathbf{\Sigma}$, *costs* something, that being the non-uniqueness of factor analytic replacement variates. The models manifest a "replacement variate" logic, and occupy opposite ends of a continuum from low rank approximation/non-unique replacement variates to high rank approximation/unique replacement variate. Hence,

> F) Factor analysis is not a special, remarkable tool that delivers fundamentally different information than PCA, but instead is informed by a "replacement variate" logic analogous to PCA and other component models.

Furthermore, despite popular belief

> G) The only legitimate sense in which factors "go beyond the test space" is in the mundane sense that the replacement variates, that is, the X_i, include an arbitrary component S_i that is uncorrelated with the $\mathbf{Y}_i$ (Schönemann & Steiger, 1978).

What makes these models different from, say, multiple regression, is that they both involve variates denoted by a concept (the "*latent variate* to $\mathbf{Y}$" in factor analysis, the "*principal component* to $\mathbf{Y}$" in PCA) whose criterion is supplied by the model. Hence, they could just as well *both* be called latent variable models, if the concept of *latent* is used correctly, since the "additional" properties tied to the concept (e.g., hypotheticality, unmeasurability, etc.) represent a mythology. The symmetry of the models can be seen when they are spelled out as follows, with a slight modification to the classical treatment of PCA.

PCA

Let $\mathbf{Y}$ be a p-component vector of random variates with $\boldsymbol{\mu}_\mathbf{Y} = \mathbf{0}$, and covariance matrix $\mathbf{\Sigma}$. Construct a replacement variate $C = \mathbf{Y}'\mathbf{v}$ such that $\mathbf{v}'\mathbf{\Sigma}^2\mathbf{v}$ is maximal for $\mathbf{v}'\mathbf{\Sigma}\mathbf{v} = 1$. That is, find C so that the sum of squared projections of $\mathbf{Y}_i$ on C is as large as possible over all possible candidate Cs with unit variance. Then, $\mathbf{v} = \mathbf{\Sigma}^{-1}\mathbf{u}$ in which $\mathbf{u}$ is the first eigenvector of $\mathbf{\Sigma}$ normalized to $\lambda^{1/2}$, and $C = \mathbf{Y}'\mathbf{\Sigma}^{-1}\mathbf{u}$.

Factor Analysis

Let $\mathbf{Y}$ be a p-component vector of random variates with $\boldsymbol{\mu}_\mathbf{Y} = \mathbf{0}$, and covariance matrix $\mathbf{\Sigma}$. Construct a replacement variate $X_i = X_\mathrm{D} + X_\mathrm{I}$ with $V(X) = 1$, $X_\mathrm{D} \subset SP(\mathbf{Y})$, and $X_\mathrm{I} \subset SP(S_i)$ [in which $SP(S_i)$ is a 1-dimensional sub-space of random variates with mean zero, variance unity, and $C(\mathbf{Y}, S_i) = \mathbf{0}$], and such that $C(\mathbf{Y}|X) = \Psi^2_{DIAG}$, positive definite. That is, replace the $\mathbf{Y}_i$ with X_i in the sense that conditional on X the $\mathbf{Y}i$ are uncorrelated. If such a replacement is possible then an infinity are possible, each given by the construction rule $Xi = \Lambda'\Sigma^{-1}\mathbf{Y} + pS_i$.

This optimal replacement argument is the sense in which correlations are "explained" in factor analysis. However, in basing the construction of X_i on more than $SP(\mathbf{Y})$, the uniqueness of the replacement is compromised. On the other hand, a construction of X_i based on $SP(\mathbf{Y})$ would fail because Ψ^2 would be of rank less than p, and non-diagonal (Schönemann & Steiger, 1976). The replacement argument that links the logic of PCA and factor analysis is analogous to the regression component conception of Schönemann and Steiger, except that the replacement variates of factor analysis are not in $SP(\mathbf{Y})$ and so are not components in Schönemann and Steiger's terminology. In fact, they prove that $\Sigma = \Lambda\Lambda' + \Psi^2$ if and only if there exists a regression component decomposition of $\mathbf{Y}^* = \Psi^{-1}\mathbf{Y}$, $\mathbf{Y}^* = \mathbf{ab}'\mathbf{Y}^* + \mathbf{e}$, in which $C(\mathbf{e})$ is idempotent and of rank $(p - 1)$.

Unfortunately, instead of facing up to what factor analysis really delivers, the response to indeterminacy has bordered on the hysterical: ETS will have to be closed down, all latent variable models suffer from indeterminacy and hence are useless, etcetera, etcetera. I believe the issue should be viewed more evenly. We make choices all the time about the properties that our models should have. What indeterminacy makes clear is that a particular choice has been made. However, the mere fact of indeterminacy hardly means that latent variable models should be given a blanket judgment of "useless". Whether they are useful or useless clearly depends on the purposes of the researcher, and in particular, the claims he would like to justifiably make. In factor analysis, indeterminacy means that one buys lower rank for non-uniqueness. This trade-off has consequences that must be carefully considered. For example, a consideration of how well an external criterion is predicted by a common factor, that is, an X_i, is rendered pointless, because, for any external variate P, one can construct an X_i such that $\rho(X_i, P) = 1$ (Schönemann & Steiger, 1978).

The Constructed Factors as Proxy for the Real Thing

Mulaik (1996) expresses frustration at my unwillingness "to consider the broader issue of what kinds of additional rules of pragmatics and semantics

are imposed when we use the common factor model to represent something in the world ..." (p. 655). He claims that "... one must go beyond the merely syntactic rule which Maraun uses to provide a criterion for a common factor, ..." (p. 655), and asks of the constructions "to what in the world might each of these distinct candidates for the common factor in the mathematics refer" (p. 656). I am sorry for Mulaik's frustrations, for I agree with many of his views on indeterminacy. On the other hand, it is clear from this last quotation that our views differ in a fundamental way. In particular, the last quotation is faulty: Constructed variates *are* in the world, in that they play a role in worldly activities (just as do principal components). What makes a constructed variate somewhat difficult to deal with is not that it is not "in the world", but that it is not denoted by a common-language concept like self-esteem, anger, depression, or, in other words, the stuff of interest to social scientists. But this problem attends all constructed variates including principal components (is a principal component not in the world?), arithmetic means, etcetera. Moreover, I do not believe that my account is insensitive to the role of pragmatics. Instead, I properly distinguish between constitutive and regulative or modifying rules (pragmatics if you would like; Ter Hark, 1990). Constitutive rules are grammatical and so fix sense. Violations of constitutive rules result in nonsense. Regulative rules, on the other hand, presuppose constitutive rules. They are norms that facilitate the smooth running of a practice. Ter Hark cites the recipes of baking as examples of regulative rules. For example, a recipe for cherry pie is useful in bringing about a desired result: A good cherry pie. It facilitates successful baking because it can be referred to and, if followed, will most likely ensure the production of an adequate cherry pie. However, a recipe for cherry pie is not a grammatical rule, and hence is not constitutive for the term *cherry pie*. One could violate the rules given by the recipe and still wind up with a cherry pie, whereas if one violates the grammar of *cherry pie* (e.g., reliably applies the term to a sample of pumpkin pie) one is not speaking of cherry pie at all, but is instead speaking nonsense. The criterion I describe for "LCF to $\mathbf{Y}$" *is* a constitutive rule: A variate not constructed as $X_i = \Lambda'\Sigma^{-1}\mathbf{Y} + pS_i$ is not an LCF to $\mathbf{Y}$. It is therefore foundational for a discussion of common factors, and so is logically prior to the pragmatics of factor analytic practice. Having said this, I would paraphrase the most essential of Mulaik's (1996) concerns to be as follows:

1. $\Sigma = \Lambda\Lambda' + \Psi^2$ and so the set of $X_i = \Lambda'\Sigma^{-1}\mathbf{Y} + pS_i$ are latent common factors to $\mathbf{Y}$.

2. Further investigation reveals that a variate Z denoted by a common-language concept (e.g., self-esteem) has covariances Λ with $\mathbf{Y}$.. Isn't Z really the factor to $\mathbf{Y}$, or at least *a* factor?

This is a fascinating scenario. Could it attain normative status? Certainly, but it would represent a *change* in the grammar of "LCF to Y", as when symptoms of something occasionally attain normative status as a criterion. The fact that this scenario would represent a change in the grammar of "common factor to Y" is why my argument is not overly zealous and should not inspire frustration. The same difficulties would arise if one were to rephrase the scenario in terms of PCA, and assert that the external variate was *really* the first principal component (No, a principal component is a *constructed* variate). Moreover, factor analysts currently do not behave as the scenario depicts. That is, they do not eventually attempt a search for some Z denoted by a concept with common-language criterion and such that $C(Z, \mathbf{Y}) = \Lambda$. If this was indeed their approach, they could well bypass factor analysis and immediately commence a search for an appropriate Z. Instead, they wrongly believe that what factor analysis gives them is some version of (A) through (E). That is, they take the figurative language of factor analytic practice as a literal description of the claims that the model can support. At the very least, an individual behaving as in the scenario would have to carefully insulate his claims against the possibility of inducing confusion.

Naming Factors and Correspondences

McDonald (1996) claims that the common factor of a set of tests or items "corresponds to their common property" (p. 670). He once again attempts to maneuver around the reality that common factors are just constructions: "It is presumably not seriously intended ... that we actually use Guttman's (1955) roulette wheel to construct solutions. If we were to do so, the resulting quantities would not be measures of properties of the persons, but joint properties of the mythical beasts that arise from a union between humans and roulette wheels" (p. 669). But McDonald has placed the cart before the horse. He seeks to deny what the model delivers because it doesn't jibe with what he desires. Sadly, it is far too late to *require* that the model deliver on particular desiderata. Any requirements should have been factored into the formulation of the model. It is pointless to now speak as McDonald does ("It is presumably not seriously intended...") as if there is something to be negotiated. The model was (apparently unwittingly) formulated in terms of constructed variates that include an arbitrary component, and that is just the way it is. If the common factors had been properly characterized in the first place, instead of being wrongly cast as *unmeasurable*, *unobservable*, *hypothetical*, etcetera, there would be no need for this post-hoc begging for things that the model can't deliver. If factor analysis does not provide a reasonable correspondence with the "properties of people", and this feature is deemed a necessity, then a new model is required.

But more needs to be said about "common properties", a notion that arises frequently in factor analysis. In the context of factor analysis, what exactly does this mean? In what sense does any constructed variate correspond to the common property of its constituent parts? Since there are an infinity of factors, are there an infinity of properties? If indeed a common factor corresponds to a property, what property is it? It is of course circular to respond, "that which the tests share". McDonald (1996), I believe would suggest that one examines the factor loadings to answer this question. But then if two variates are perfectly correlated, does one correspond to the common property of the other? The use of "common property" in this context is misguided. In the first place, the correct identification of A as the common property of C and D presupposes a meaning for A. But the loadings of factor analysis do not explicate a *meaning* for the common factor constructions, but instead are the regression coefficients of the $\mathbf{Y}_i$ on the constructions X_i, and regression coefficients do not establish meaning. In fact, the correct claim that the correlation between A and B is the correlation between τ and Ω presupposes knowledge of the meanings of Ω and τ. Moreover, indeterminacy, involving as it does an infinity of X_is, is a paradigm case of misdirected attempts to pin down via correlations even the empirical characteristics of a variate. McDonald's common property conception manifests the same slippery logic involved when a common-language term, for example, *anxiety*, is given to the empirical "overlap" between two variates (i.e., the correlation is explained by envisioning a "part" of each variate that they have in common, and this "part" is given a name). In fact,

> H) The naming of common factors is a response to the fact that, as constructed variates, common factors are not denoted by common-language concepts (in other words, in naming them the investigator attempts to give them common-language status).

Whether this tack is legitimate is an interesting question. Constructed variates (e.g., the arithmetic mean) are standardly "explained" by providing their rules of construction, and are *justified* by listing their uses. On the other hand, the correspondence relation that McDonald (1996) wrongly asserts is absent from my account is of the same kind that runs through homogeneity characterizations (see e.g., Gifi, 1990). That is, the common factor are constructions that replace the manifest variates in the optimal sense prescribed by factor analysis: $C(\mathrm{Y}|X_i) = \Psi^2{}_{\mathrm{DIAG}}$.

Infinite Behavior Domains

To begin, the behavior domain formulation of factor analysis is a different model than the *p* variates case, and hence is not a solution for the indeterminacy of the latter model. I'm not sure why McDonald (1996) was left with a feeling of pleasure after the last round, when all of the big issues remain. He would have us believe that the behavior domain formulation is the model that should be used, and is in fact the model that factor analysts implicitly invoke as the basis for their investigations. The latter sentiment, however, is highly questionable, given the way factor analysis is employed and spoken of. Specifically, very few even consider the definitional issues that the domain conception presupposes. In fact, construct validation theory which misguidedly takes empirical results as having the power to legislate on issues of meaning is still in full force. Furthermore, the popularity of jargon like *unmeasurable*, *unobservable*, etcetera, suggests that most researchers buy into misconceptions (C), (D), and (E), which contradicts McDonald's wishful assertions about their motivations. But let's assume for the moment that random-effects factor analysis *was* the model in play. McDonald asserts that we are already clear about behavior domain theory, and later urges that "... behavioral scientists — at ETS, ATC, and elsewhere, can go on doing what they wish to do" (McDonald, 1996, p. 663). But this is entirely too fast, and represents the same speedy exit from difficult issues that characterized the early factor analytic response to indeterminacy (Steiger & Schönemann, 1978). The logical issues inherent to behavior domain theory, and associated with latent variable models in general, have hardly been touched. There are many issues that need clarification, including whether it is even reasonable to pair the behavior domain conception with the notoriously messy grammars of the psychological concepts that denote phenomena of interest, and the pragmatic "what would be gained by the adoption of behavior domain theory"? One gets the feeling that McDonald's answer to the latter would be "this might get rid of the

problem". He asserts that the ASP "has no fangs after all", making it sound as if it is but a minor annoyance. But the ASP, as it is called, is a *description* of a state of affairs, and what it describes (latent variates as constructions, problems with uniqueness of construction, etc.) must be carefully considered in all latent variable contexts. Furthermore, illegitimacies (A) and (C) through (E) would still hold in the infinite domain formulation, only now uniqueness of replacement would be an asymptotic possibility. Such a great deal of artifice to give factor analysis a justification as fully a component model.

Latent Variable Models

Many of the powers attributed to factor analysis arise from misconceptions engendered in large part by the densely figurative language that has grown up around the practice of factor analysis. The term *latent* is but one such term that conjures up images of different realms, etcetera. Now I accept Mulaik's (1996) claims about science and metaphor, and accept that it may be the case that scientists, for any number of reasons, do their best work with latent variable models. But the ability to distinguish between the *mythology* of latent variable models, and what each model can in fact deliver, matters a great deal when it comes to the making of scientific claims. It matters when claims are made about what PCA and factor analysis can deliver, whether, and in what ways, one is superior to the other. Furthermore, (A) and (C) through (E), with (B) an issue specific to the model in question, hold generally for latent variable models. Once again,

> I) The feature that makes a model particularly a *latent* variable model is that it involves variates denoted by a concept, "latent variate to Y", whose criterion is given by the model itself when it describes Y. Hence, a latent variable model involves constructed or replacement variates, the form of the construction depending on the particular model under consideration.

Vittadini (1989), for example, provides the construction rules for the latent variates of LISREL models. It should be of great interest to investigate the construction rules and uniqueness properties of specific classes of latent variable models (e.g., item response models (Maraun, 1990), and non-linear factor analysis).

Meaning

Both McDonald (1996) and Rozeboom (1996) give significant attention to the issue of meaning, and here I return the favour. In many ways it is hard to believe that McDonald has read any of Wittgenstein's work. What is promised as a demonstration of the "self-refutation" that he claims runs through my work, turns out instead to be a demonstration of serious shortcomings in his grasp of the issues. I base this chiefly on his belief that the claim that meaning is grammatical, autonomous, and non-discoverable implies the claim that language cannot function referentially and descriptively. This is seriously confused. I will here provide a brief outline as to why. A central theme of Wittgenstein's later work is that grammatical rules are standards of correctness in a linguistic practice. Grammatical rules are taught in the learning of language and referred to in disagreements over meaning. When one clarifies what one means by a concept (explains its meaning) one cites relevant segments of grammar, for grammar is internally related to meaning: "Giving an explanation consists in displaying some of the connections in the grammatical reticulation of rules" (Baker & Hacker, 1980, p.36). However, grammatical rules are autonomous in the sense that: (a) They are not determined by phenomena, but are instead *constitutive* for phenomena; (b) they are not justified, but are the grounds for justification (i.e., they are standards of correctness in the use of concepts): "Grammar is not accountable to any reality. It is grammatical rules that determine meaning (constitute it) and so they themselves are not answerable to any meaning and to that extent are arbitrary" (Wittgenstein, 1978, p. 133). Stated more fully, "Grammatical rules themselves are not justified by something external to them.... Grammatical rules cannot occasion a discussion as to whether they give a correct rule for a certain word. For without these rules the word has no meaning. These rules are constitutive, arbitrary, or autonomous in the sense they give words their meaning in the first place" (Ter Hark, 1990, p. 66). Furthermore, the grammatical rules that are constitutive for the meaning, say for example, of τ are not discoverable, since any supposed discovery could only be justified as, *in fact*, a discovery by comparison to the rules themselves, and such a comparison obviously *presupposes* knowledge of the rules (and eo ipso the meaning of τ). In Ter Hark's (p. 32) terms, "one cannot learn a criterion as an object of knowledge, since any information that is supplied ... presupposes familiarity with the criterion ..." Instead, grammatical rules are taught, learned, referred to, etcetera, as part of linguistic practice. The constitutive nature of grammatical rules, their autonomy and non-discoverablity, are characteristic marks of the normative character of language. Of course language involves

reference and description, but McDonald clearly does not understand how. First, statements that function referentially or descriptively hardly exhaust meaningful linguistic behavior. Secondly, grammatical rules are *constitutive* for describing and referring, since they establish the meanings of the concepts that inform descriptions, references, denotations, etcetera. Hence, the possibility of description and reference does not *disagree* with the autonomy and non-discoverability of rules, but instead *manifests* these features of normative linguistic practice. McDonald makes the same old mistake of envisioning the "outer world" as somehow in conflict with "mere" language (his "extralinguistic reference"). However, the elementary Wittgensteinian insight is not that there is no "real world", but that grammatical rules are constitutive for discussions of the world, for describing, denoting, etcetera. That which is denoted is *internally* related to the concept that denotes. Hence, "The limit of language is shown by the impossibility of describing the fact which corresponds to a sentence (is the translation of it), without simply repeating the sentence... (Wittgenstein, 1980, p. 10). Grammatical rules determine what makes sense, not what is true or false. I should also say that McDonald's (1996, p. 665) assertion that "Scientists cannot wait ... for definitive philosophic accounts of what they in any case do", which is absolutely true, has nothing whatever to do with the *conceptual* considerations that run through scientific investigation. The issue is not about "getting the world right", but about avoiding the nonsense that results when linguistic confusions and unclarities are left unchecked:

> Surveying this argument today it is perfectly clear that a way out of this dilemma could only be found by turning away from the world of facts to a consideration of concepts. ... 'What exactly does it mean to say that they are simultaneous?' ... If it is used to refer to events in quite different places, we require a statement of what it is to mean in this new context. This step was taken by Einstein. He neither discovered hitherto unknown facts, nor did he suggest a hypothesis which explains better the known facts; rather he cleared away from the concept of simultaneity the confusion which had surrounded it (Waismann, 1965, p. 12).

And while indeed there is no single way of resolving philosophical problems, philosophical problems nevertheless arise with regularity from misunderstandings about the grammars of concepts, necessitating, as Wittgenstein (1953) showed throughout the Investigations, the careful explication of grammar (to "release the fly from the bottle"). Finally, the Carnapian distinction between analytic and synthetic truths, restated by both McDonald and Rozeboom, is *not* equivalent to the distinction between grammatical rules and empirical propositions. Grammatical rules are not *truths* in any standard sense (e.g., they cannot be false, although they can be violated, misunderstood, improperly phrased, etc.).

The title of Rozeboom's (1996) entry wrongly implies that I claim that indeterminacy issues are conceptual confusions. I do no such thing, but instead assert that interpretations of indeterminacy such as Rozeboom's are conceptual confusions. He expresses bewilderment over rules: (a) "... How confident are we entitled to feel that a formulated rule has got it right?" (p. 638); (b) "Are there any rules for the correct application of 'rules' ..." (p. 637); (c) "... is 'grammar' more or less synonymous with 'rules for correct use', with their correct application including, say, proscription of loud utterance of obscenities during solemn ceremonies?" (p. 638).

1. Grammatical rules are constitutive for meaning and hence are not right or wrong (any more than the rule "stop at the stop sign" is right or wrong). A grammatical rule is not a proposition but instead establishes the meaning of *what* is to be discussed. Put another way, a grammatical rule is internally related to that which satisfies it. If *A* and *B* are internally related: (a) It is not possible that they do not have this relation to each other (i.e., there is no such thing as the identification of *A* without the identification of *B*); (b) their relation is not mediated by a third term; (c) their relation presupposes a practice of linguistic behavior (Ter Hark, 1990). Hence, the conception of a right (correct) grammatical rule is incoherent.

2. Following from property 2 of internal relations, there are not rules for the application of grammatical rules. A grammatical rule is not a *cause* of linguistic behavior, but is instead a standard of correctness for linguistic behavior. Hence, a rule and its application meet in a behavioral practice: "Only within a practice, within a way of life, can one say that this rule means this, that this action obeys this command" (Ter Hark, 1990, p. 50). To put it another way, the bridge between rule and application can only be crossed in practice (Baker & Hacker, 1980), that is, a practice in which the rule is in fact *taken* as a standard of correctness for linguistic behavior.

3. The proscription of a loud utterance is not a grammatical rule but a regulative rule (see p. 679).

Rozeboom (1996, p. 638) states "I claim, and expect you to agree, that the text you are now reading is printed in black", and wonders where the "Maraunian" rules that are constitutive for the meaning of *black* are. But (apparently) unwittingly his claim exemplifies the paradigmatic standard of correctness for the use of color terms, that is, the correct use of color samples (see Hacker, 1987). He appears to believe that he is making an empirical assertion (hypothesis?), while in fact what he articulates is a piece of grammar: The term *black* is internally related to color samples of black (e.g., the page in question). Hence, an individual who understands the

meaning of the concept *black* understands how to correctly *use* samples of black, because they are internally related. No *proof* could be provided that the page was black, for any other *evidence* that might be brought to bear would itself presuppose this internal relation (let's check if this page is black: And what is meant by *black*). Furthermore, his assertion exemplifies (excluding the call for agreement) the standard means of *teaching* the meaning of black and of resolving disagreements over the use of color terms (i.e., by correctly providing color samples). Hence, Rozeboom's rejection of my (flippant) claim that color exclusion is a grammatical matter is a product of his failure to grasp the grammar of color terms. Color exclusion is an aspect of color meaning, a feature of the *use* of color samples (which are internally related to color concepts; Hacker, 1987). One has nothing to say on this matter outside of a clarification of the grammar that is constitutive for color concepts (unless one would like to say something external to meaning and engage in loose empirical speculation and the formulation of causal theories, as does Rozeboom). He contradicts my claim that it is incoherent to assert that "Joe is very dominant but has never behaved dominantly" (Rozeboom, 1996, p. 639), and then, showing remarkable contempt for the individual case, proceeds to provide examples that are conceptually distinct from the dispositional use of *dominant*. In particular (p. 640):

1. This involves an entirely different concept, that is, *dominating*, as in "Joe is dominating the conversation" (behaving dominantly), so of course they are not synonymous.

2. and 3. Of course these claims are true, but, once again, feature vastly different grammars than that involved in the correct application of *very dominant* to Joe.

4. and 5. Here we have the root of the confusion, for the empirical realist believes (incoherently) that dominance, a substantive term, denotes a thing (causal mechanism?) *in* the person, and hence believes that the application of *dominant* to someone is an empirical hypothesis. If this were not incoherent, it would be nothing more than loose empirical speculation. In fact, an understanding of the meaning of *dominant* (i.e., its uses) is a *precondition* for the pseudo-empirical speculations about dominance that Rozeboom weaves into his analysis. He states that to "conjecture that Joe is now dominant even though he has never behaved dominantly may well seem implausible but it is by no means incoherent and can easily be tested" (p. 640). However, the correct use of a concept has nothing to do with plausibility or testing (let alone causality: See Rozeboom, 1996, p. 646, on "natural" height and "non-causal derivation" of units of measurement) although one might certainly entertain the possibility of testing for the causal precursors of that which is denoted by a concept.

His (Rozeboom, 1996) comments on Cartesianism manifest confusions over the term *mind*, which merely has a surface grammar that is similar to a material substantive. There are indeed physical preconditions for mental processes, but to hypothesize that they may *really* be equivalent to mental processes is to miss the point that the meanings of the concepts that denote mental processes are constituted in grammar, and a careful analysis of grammar reveals the incoherence of equating the two (see Hacker, 1990, for a detailed account). Finally, Rozeboom (p. 649) asks why adding an "intensionality constraint to the model would be a metaphor external to the model's math". Well, we could of course insist that the knight be given the same range of movement as the queen, but then we would no longer be playing chess. To insist on such an intensionality constraint is to suggest that a different model, with a different criterion for "common factor to **Y**", be adopted. Rozeboom (p. 642) characterizes my account as "unrequited yearning for the simplistic but comforting certainties of an era past". But his space-age philosophy of science represents anything but progress. For endemic confusion is not progress, and to be dazzled by incoherently phrased questions and bogus formalisms, to repeatedly confuse the aims of philosophy with those of empirical science, merely serves to emphasize just how far ahead of the pack Wittgenstein remains.

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