

Factor-indeterminacy Issues are not Linguistic Confusions

William W. Rozeboom
University of Alberta

It appears from the profusion of Wittgensteinian slogans in Maraun's (1996b) commentary that his thesis on the character of common factors is strongly rooted in an outlook on language whose controversial obscurities muddy rather than clarify the factor-analytic issues on which we are supposedly focused. Hardly anything objectionable in the Wittgensteinian perspective is needed for what I take to be the core of Maraun's position on factor indeterminacy; but since he evidently wants us to swallow that whole, he needs to show some concern for whether his intended audience largely agrees with his linguistic claims or even understands them.

Here is a sampler of Maraun's (1996b) dicta on linguistic meaning:

The meaning of a concept is manifest in its rules for correct application (p. 609)

... we in fact can ... learn the correct use of a concept, [and] cite rules (standards of correctness) when disputes over meanings arise ... (p. 609)

The meaning of a concept is laid down in the rules of grammar ... (p. 609)

The meaning of a concept ... is manifest in its rules for correct use, rules laid down in grammar. (p. 611)

... grammar fixes sense, ... (p. 612)

Grammar ... fixes meaning ... (p. 613)

Identity criteria [for application of a concept] are laid down in grammar. (p. 613).

Does Maraun really think that any of us in his present audience have much operational grasp of what he means by these claims, or, were we to understand them, have reason to believe them correct? Are there any rules for correct application of "rules" and "correct application" in whatever sense he intends here? (As shown by "As a rule, men use expletives more often than do women" and "The correct application is 2 lbs. per 25 sq. ft.", these

terms are far from unambiguous.) And if there are, how should we have learned them? Can Maraunian "rules" be communicated in principle as verbalized norms (*ought*-statements) even if we acquire these recipes for proper behavior most commonly through immersion in social situations that exemplify them? (If so, how about a verbalized example or two.) Or consider "grammar". Does this word correctly apply just to logical syntax?¹ Or 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? (If we have any rules for social deportment, surely this prohibition is among them; and inasmuch as it lays down a norm for linguistic usage, does it then count for Maraun as a grammatical rule?) And what does Maraun mean by "*correct* use"? Perhaps "rules for correct use" is redundantly equivalent simply to "rules for use" with correctness ensuing as conformity to the rule. But if rules can be verbalized — and if not, how can we cite them? — and uses of concepts are correct, or incorrect, because these comply with or violate rules that have laid down these concepts' meanings, then the use of concepts in stating these rules must likewise be correct or incorrect according to rules that lay down these concepts' meanings,² and we are off on an infinite regress. Alternatively, if uses are in some fundamental sense correct or incorrect independently of any rules that formulate this correctness, how confident are we entitled to feel that a formulated rule has got it right?

Let's consider specifics. I claim, and expect you to agree, that the text you are now reading is printed in black. So the word "black" is meaningful and used correctly just now, whence according to Maraun (1996b) its meaning (sense) is fixed by grammar, and its present application is correct by virtue of rules in which the meaning of "black" is manifest. Now, any rules in which this meaning is manifest must themselves be evident (else how could they manifest anything?) — so what are they? One candidate worth mention is that "black" functions as an adjective and hence generates nonsense if made a subject of predication without first nominalizing it into "blackness" (whose second syllable is then likely to be elided by idiom). But the rule, "Ascribing predicates to adjectives is ungrammatical", doesn't apply to "black" unless this word brings pre-established adjectival meaning to the

¹ That is, to linguistic provisions for expressing the mysterious structure of complex thought without which the propositions respectively conveyed by sentences "John loves Mary" and "Mary loves John" would be the same set {"John"-idea, "Mary"-idea, "loves"-idea} of conjointly activated but otherwise disconnected concepts.

² Arguably, concepts don't *have* meanings but are themselves meanings. To speak otherwise is to risk conflating the meanings (senses) of linguistic expressions with their potential referents (designata).

attempted predication. Nor does this rule together with the information that "black" is an adjective even hint at this word's meaning, much less make it manifest. Unless our considerable repertoire of everyday beliefs embodying the concept expressed by "black" ("Coal is usually black"; "If your toes have turned black, you need medical attention", etc. etc.) qualify as grammatical rules, I haven't a clue how to fix the meaning of "black" by appeal to rules for its correct usage, especially rules that make it correct to call the print you now see "black" while "red" would be incorrect. Can Maraun enlighten us?

Maraun's (1996b) examples of certainty achieved by grammatical fiat are less convincing that he posits. Consider his little list of nonsense:

... the grammar of colour terms makes it incoherent to assert that an object is both red and green at the same time, ... (p. 612).

Comment — not so. My very first philosophical publication (Rozeboom, 1958) pointed out perfectly natural circumstances in which it is appropriate to say that something is simultaneously both red and green.³ I also observed there how it is possible to understand "red" and "green" in a sense that does make these incompatible by virtue of meaning; and conceivably this sense prevails in everyday color-talk. But the meaning analysis required to clarify the conditions of color incompatibility exposes deep puzzles about the generic nature of mutual-exclusivity among the attribute alternatives, such as weights, lengths, and temperatures, that constitute scientific variables (variates). I can smile at Maraun's (1996b) faith that rules of grammar provide all the insight we need in such matters, but if he were a professional philosopher I would be appalled.

... the grammar of *dominance* makes it incoherent to assert that Joe is very dominant but has never *behaved* dominantly, ... (p. 612).

Comment — not so. Insofar as "dominant" and most other ordinary-English adjectives with suffix "ant", "ous", "ile", or "able" (not a complete list) sustain a grammar of correct usage, their Maraunian rules authorize arguments like the following:

³ After technical preliminaries, the argument commences with observation that commonsense has no qualms at all about allowing objects such as checkerboards to have two different colors at the same time. But that is only warm-up for a flight of concept analysis that slips the surly bonds of color clichés. (Let me add that this flight was no more a violation of proper language usage than Owen's flights violated earthly physics. Rather, it drew out implications of commonsense color talk that had not heretofore been recognized. Should you care to check out this article, skim or ignore its formalistic beginning and concentrate on Section III.)

1. Although there is a strong meaning connection between our concepts of (a) being dominant and (b) dominating (i.e., behaving dominantly), predicates "x is dominant at time t " and "x dominates (that is, behaves dominantly) at time t " are neither synonymous nor mandated to be co-extensive.

2. There are certain circumstances — call them "domination-permitting" — such that it is not possible for a person x to dominate at a time t unless x 's circumstance at t permits domination. (For x 's circumstance at t to permit domination, it presumably must include one or more persons co-present with x at t . Much more is also required — for instance, at least one of the others must be neither vegetative nor shielded from x 's behavior at t — but this is Maruan's, 1996b, example so he can fill in details as he pleases.)

3. If x 's circumstance at t does not permit domination, x 's failure to dominate at t does not preclude the possibility that x is dominant at t .

4. If x dominates at time t , it is possible and indeed likely that t is included in some time interval of appreciable duration such that x is dominant at each moment t' therein even when x 's circumstance at t' does not permit domination.

5. If x dominates at the first time t_0 when x 's circumstance permits domination, it is possible and indeed likely that for some time t' prior to t_0 , x is dominant throughout the interval from t' to t_0 .

6. Consequently, it is possible for persons to be dominant prior to their first display of dominance.

Unlike 1.-4., which surely follows from any grammar that accurately reflects how we actually use concepts like "dominance", 5. is a chancier hypothesis. For while 1.-4. characterize dominance as an enduring state in contrast to episodic dominations, x 's becoming dominant may require some triggering event. Acting dominant for the first time (perhaps even after past failures to dominate in permitting circumstances) might well be such a trigger. But so might other ego-boosting experiences in circumstances not domination-permitting. 1.-4. entail that whether x is dominant at t is in many circumstances not revealed by x 's behavior at that time; and if x 's frequent past dominating leads us to infer not merely that that x is now dominant but might have become so even before his first act of dominating, 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.

... the grammar of *mind* makes Cartesian dualism incoherent (mind is not a substance) (p. 612).

Comment — not so. “Mind” is a mass-noun like “water”, “gold”, “flesh”, “air”, etcetera, and as such is designed by its grammar to name a substance that can without a trace of incoherence be conjectured to differ from physical matter. The big trouble with Cartesian dualism is not incoherence but its received ontology: As presaged by Aristotle’s astute claim that mentality is form rather than substance, and once again appreciated by modern philosophy after two millennia of substance abuse, the essential contrast between Mental and Physical events lies not in the nouns their descriptions embody but in their predicates. Our conception of mentality comprehends a rich repertoire of attributes — believings, desirings, perceivings, feelings, imaginings, endeavorings, and much more — whose paradigm cases differ profoundly in grammar (most crucially in the logical structure of intentional acts) from attributes we ordinarily regard as physical. In contrast, it is hard to motivate a mind-stuff/body-stuff division between the subjects of mental predications and those of physical predications; and it is tempting to hold (as I do) that mind-stuff distinct from matter is a myth or, if substantival minds are thought needed to satisfy mental predicates, that these are just specialized bodies.⁴ Meanwhile, attributive Dualism holding that mental properties are fundamentally different in ontic kind from physical properties remains very much a live prospect. I happen to share the view that mental properties are certain poorly understood organizations of physical properties. But whether this is (a) largely correct or (b) egregiously wrongheaded is not for rules of linguistic usage to arrogate.

⁴ Here is an instructive little thought-experiment. Suppose that the two sentences, “I weigh 160 lbs.” and “I hope that it won’t rain tomorrow” are both true of you. (If you want truth for real, revise these as appropriate.) Then consider what happens to these truths if you replace “I” in each by “my body” on one hand and “my mind” on the other while appending “s” to their verbs. Although some of these revisions feel decidedly odd, it’s not for me to tell you which ones if any you should consider false or nonsensical. But don’t be too quick to presume that your body but not your mind has Weight, that your mind but not your body does Hoping, and that both predicates hold for your substantival “I” because you are an amalgam of two distinct substances, body and mind. There is simply no reason beyond linguistic tradition (Maruanian grammatical rules?) to make this move. An alternative position — the one I personally favor though by no means the only arguable alternative — is that the substantival I is the same as my body give or take a few parts, while minds posited to be a nonphysical substance that embodies the properties ascribed by mental predicates are neither needed for that purpose nor have any other reason to exist.

However, Maraun does not need to flail at us with so authoritarian a view of language in order to justify the core of his thesis (its periphery is another matter) on the nature of common factors. That some things we say are true or false by virtue of their meanings, rather than contingent on empirical fact, has long been a mainstream orthodoxy whose explication was a major target of analytic philosophy earlier this century. The orthodoxy has been that statements are either *analytic* (truth-determinable just from consideration of their meanings) or *synthetic* (true or false according to something external to their meaning), and moreover that this contrast is a sharp dichotomy. Quine's famous "Two dogmas of empiricism" contention (see Quine, 1953), that this dichotomy is bootless, seems to have pulled the plug on obsessing over that even though many philosophers of language would still like to retain analyticity as a matter of degree (see especially Putnam, 1962). So Maraun's (1996b) contempt for my "... conflation of empirical and conceptual issues ..." (p. 611) and "... muddle of empirical assertions and conceptual confusions" (p. 615) sounds like unrequited yearning for the simplistic but comforting certainties of an era past. Deplorably, conflations, muddle, and confusions similar to mine have become sufficiently widespread in modern analytic philosophy that we could well profit were Maraun to publish an exposure of these errors in a mainstream philosophic journal.

Even so, hardly anyone disputes that new terms can be introduced by explicit definitions or that some terms already in use can be explicated as largely equivalent to more articulate expressions of their intended meaning, thereby enabling some statements using these defined/explicated terms to be classically analytic. And so far as I can tell, that is all Maraun's (1996b) "identity criteria" do for him — except that he gratuitously twists those into a mystique entirely unauthorized by any fixing of conceptual meaning by rules of grammar. My initial commentary's effort to set this aright seems not to have caught hold, so with some impatience I will try again.

When discussing language usage, it is often important to distinguish among (a) the overt words/phrases (sign-designs) that physically convey our communications, (b) the concepts (meanings, senses) these supposedly express in our thinking, and (c) entities known to semanticists as "objects" (in a sense much broader than "material things") which some meanings designate (refer to, signify, are concepts of) in contexts that permit aspirant reference to succeed.⁵ We often ambiguate among these because our

⁵ This is of course only the coarsest outline of an enormously complicated and prodigiously disputed story. I can't even overview what the larger complications are, much less how they might be dealt with, in the space available. If I speak simplistically here, its not because I wouldn't like to say more.

metalinguistic resources are clumsy at these distinctions and in everyday applications they usually don't matter much. Moreover, when addressing the semantics of predicates (which range from elemental adjectives to complex open sentences containing multiple placeholders for nominals), it is also important to distinguish the property that a predicate *designates* from the objects that *satisfy* this predicate, that is, which have the property signified by this predicate's meaning. Traditionally, the set of objects that satisfy a given predicate is known as this predicate's "extension", while the property it signifies has often been called, albeit with considerable risk of confusion, its "intension".⁶ In particular, when (a) a dyadic predicate " $R(_)$ " signifies a relation (property of pairs) R such that for each object x in a domain X there is exactly one object y in a range Y such that $R(x,y)$, and (b) " f_R " is defined as a nominal-forming expression such that " $f_R(x)$ " is shorthand for "the entity y such that $R(x,y)$ ", then (c) " $y = f_R(x)$ " is equivalent in meaning to " y is the entity such that $R(x,y)$ " [which almost though not quite has the same meaning as simply " $R(x,y)$ "], and (d) we can view " f_R " as the name of a *function* which, as object, consists of two parts: (i) an extension comprising all pairs $\langle x,y \rangle$ such that $R(x,y)$, and (ii) an intension consisting of relation $R(_)$. (This formulation of a function's nature is disagreeably sketchy, but it will suffice to ground some important closing thoughts on the ontology of scientific variables.)

It has been my impression that when Maraun (1996a) speaks in his lead article of "the criterion for the phrase ' X is a latent common factor to Y '" (wherein "criterion for" is presumably short for "criterion for correct application of"), he envisions an explicit definition or meaning analysis of the predicate " $_$ is an LCF to Y ", such as some fleshing out of my schematic " $LV_r(_)$ " might provide. Then for any nominal expression " X ," the sentence " X is an LCF to Y just in case $LV_r(X)$ " is not merely true but true by virtue of its meaning. And we can see how Maraun might reasonably view "Something is an LCF to Y if and only if $LV_r(it)$ " as a rule of correct application laid down in grammar for the concept expressed by "is an LCF to Y ". But in almost the same breath, as though merely paraphrasing what went before, he also speaks repeatedly of a *criterion of identity* for an entity

⁶ The main confusion is that a predicate's *meaning* has also at times been taken to be its intension. This ambiguity is encouraged by the extensionalist doctrine (considerably more common early this century than today and, I would argue, importantly untenable) that properties *are* meanings. It is also promoted by confusion with the intentionality of meaning in mental acts. Even so, I adopt the term here because it is servicable if used with care, there is no alternative that would be much less troublesome, and Mulaik has already invoked it in his contribution to this symposium (Mulaik, 1996, p. 587).

X which is not a word-string or concept but is semantically an object, notably a putative common factor. But what is it for an *object X* to have an identity criterion? Shouldn't the entity for which we seek an identity criterion be a *concept of X*? And what could this criterion be if not some other concept of *X* that better captures the essence of *X*? If Maraun insists that it is really *X* itself, not some concept thereof, for which we want an identity criterion, does any concept that refers specifically to *X* thereby suffice to identify *X*, or must an *identifying* concept not merely designate its object but do so in some specially informative way? A case can indeed be made for treating "identification" as more than just individuating reference (see Rozeboom, 1988, p. 211). But Maraun has neither argued for this nor given us reason to think that it is at all relevant to common-factor indeterminacy. What he has put at issue is definition of the predicate "is an LCF to *Y*", not the individuating essences of objects that satisfy this predicate; and he has *not* shown how to verbalize an identity criterion for any particular object that may be an LCF to *Y*.

Until Maraun (1996b) clarifies his choice of wording here, I shall continue to insist that he is conflating his criterion for the *property* of being an LCF to *Y*, which can fairly be viewed (if one likes to talk that way) as a criterion for whether some entity *X* has the LCF-to-*Y* property, with a criterion for the identity of *X*. (His more elaborate "criterion of identity for '*X*, a latent common factor to *Y*'" formulation on Maraun, 1996a, p. 527, which can be shifted just by converting the comma after "*X*" to "is" from talk of an identity criterion for a particular object which happens to have the LCF-to-*Y* property, to talk of a criterion for whether this object does in fact have this feature, demonstrates the slide that may be responsible for this confounding.) But in Maraun's (1996a) lead-article discussion of testing for the presence of *it* (p. 528), he seems to insist unambiguously on an identity criterion for *it*, rather than for some conjectured property of *it*. So perhaps on pages 523-524 and subsequently he really does think that "the criterion of identity for [latent variate] *X*" identifies a particular *X* that is LCF to *Y*, even though the only identity criterion envisioned for *X* here is being an LCF to *Y* — which would simultaneously be an identity criterion for every other object that is LCF to *Y* as well.

In my original commentary (Rozeboom, 1996), I suggested that Maraun's (1996a) apparent conflation of his criterion for LCF-to-*Y*-hood with individuated identity criteria for the diverse objects that have this property may explain his apparent positing of an ontology for common factors that I characterized as "ghostly". For when on page 521 he insists that "... a latent factor is exactly what is specified by Equation 3, and nothing

more" and, with "δ" and "X" therein read as constrained placeholders, Equation 3 schematizes a predicate that identifies latent-factorhood but nothing beyond that, how is Maraun's "nothing more" disclaimer here to be construed if not as a slide from his legitimate if debatable contention, that Equation 3 identifies exactly what it is to be a common (latent) factor, to the silly proposal that individual common factors have no properties other than their common-factorhood. (By the Identity-of-indiscernables principle, if latent factors were nothing more than what is specified by Equation 3 they would be identical with one another.) And this apparent denial that individual common factors have attributes unspecified by the model is seemingly repeated by the claim on page 524 that a common factor's "character is determined solely by the equations ..." followed by instances of properties that being a common factor allegedly precludes. But if Maraun allows an object to have attributes additional to its "character" he owes us an explanation why being unobservable, hypothesized, underlying, etc. are incompatible with latent-factorhood character.

It turns out, however, that a key verb in Maraun's (1996a) text following this passage, to which I was insufficiently sensitive previously, does suggest an argument for these preclusions. The argument as given is only a hint with an untenable tacit premise, but it does raise some issues of considerable interest. In brief, Maraun contends that common factors (latent variates) are *constructed* out of other variables (this much of the thesis is explicit), and thereby receive a character incompatible with the metaphorical properties that so many factor analysts so fondly ascribe to them.

To critique this proposal, we need to be clear on certain fine details of (scientific) variables' ontic character that I must review here rather dogmatically. (For more extensive discussion, see Rozeboom, 1966.) Most fundamentally, a "natural" variable over a domain D of entities is a function α from D into a set A of mutually exclusive attributes such that, for each d in D , $\alpha(d)$ is d 's one and only attribute of kind A .⁷ [$\alpha(d)$ is then the "value" of natural variable α for its argument d .] In practice, however, we find it more technically powerful to represent values of α by real numbers, which we accomplish by scaling.⁸ Specifically, a (real-valued numeric) scale for natural variable α is a function ϕ from A into the real numbers; and the ϕ -scaled variable x_A representing α is the composition $\phi\alpha$ of α into ϕ , that is, the value of α 's scaling x_A for any argument d is $\phi[\alpha(d)]$. Unfortunately,

⁷ In Rozeboom (1966), I took contrast class A in itself to be a "natural" variable. But since scientific discourse treats its variables so unrelentingly as functions, it seems better to conceive natural variables as functions from the outset.

⁸ Scale values of non-numeric kinds are also sometimes useful.

numeric scaling makes additionally strange an ontology that is already uneasily obscure. It is straightforward enough to view the intensions of natural variables such as Height, Weight, and Temperature as the sets of attributes into which they map their arguments.⁹ But "The (approximate) Height-in-inches of ___ is ___" prima facie signifies a single relation of certain physical objects to cardinal numbers which plainly differs in extension from the relation signified by "The (approximate) Height-in-cm. of ___ is ___",¹⁰ and arguably differs somewhat in intension as well, albeit we would like to say that the *core* of these intensions in both cases is the same natural variable Height. Even though scaled variables Height-in-inches and Height-in-centimeters mirror natural Height, they are by human contrivance derivative from the latter; and the lawlike regularities in which they participate are epiphenomenal reflections of laws governing natural Height. That is, although John's being 5' 10" tall today may well have genuine causes and effects, his having values 70 of Height-in-inches and 178 of Height-in-centimeters are explained by their noncausal derivation from his natural height.¹¹ Even so, when number-valued variable x_A is an entry-level scaling of some known natural variable α , we can talk about causes and effects of x_A while understanding this as just a convenient way to say such things about α .

The epiphenomenal or (to use a concept of increasing importance in recent philosophy of science) "supervenient"¹² character of scaled variables becomes more ontically troublesome, however, when we introduce combinatorial transformations of entry-level scales. Suppose we stipulate

⁹ Two esoteric complications that arise even here: (a) "___ is five feet ten inches tall" signifies approximately the same property (i.e. intension) as does "___ is 178 centimeters tall"; yet the concepts (meanings) respectively expressed by these predicates differ appreciably in their reference to measuring procedures and cardinal numbers which, moreover, are arguably extrinsic to the nature of Height. So we shouldn't presume that all entities referred to by parts of these (or of any other) predicate-concepts are included in the properties they signify. (b) It is not plain that *Having the 178-cm.-tallness value of Height* is entirely identical with *Being 178 cm. tall*, though we would surely prefer that it be.

¹⁰ Please take "(approximate)" here to set up small intervals around the exact Height-values with boundaries giving these two predicates exactly the same extension.

¹¹ Thus illustrating an important principle of explanation: Not all because are causes.

¹² The concept of "supervenience" is a seminal ontological intuition still struggling to achieve full birth, and a satisfactory definition has yet to be published. But it attempts to capture the notion that some properties are noncausally necessitated by or, as I prefer to put it, are abstractively contained in others. Here are two simplified examples: John's being tall supervenes on his being 78 inches in height, and the mean and SD of Height-in-inches in John's basketball team are supervenient on the collection of those team members' individual heights. To check out the philosophical literature on this topic, see especially Kim, 1993.

that variable y over the domain D of Height and Weight is the Weight-in.-lbs. (w) scaling of natural Weight plus the Height-in-inches (h) scaling of natural Height, or that the value of variable m_w on D 's power set is by definition, for each subset d of D , the geometric mean Weight-in.-lbs. in d . Such y and m_w plainly supervene on Height and Weight, yet it is difficult to see how they might be viewed as entry-level scalings of natural variables; and the challenge to make their intensions clear seems mind-boggling at present, though it needn't remain so. And if we further define variable h' on D as $h' =_{def} y - w$, h' is extensionally identical with h but intensionally supervenient directly on w and thus indirectly, through w 's mediation, on h and hence different from that in intension. Regardless of whether we will ever manage to incorporate variables like y , m_w , and h' in supervenient laws that are genuinely causal at molar levels of organization,¹³ we can surely agree — not by rules of grammar but from deep intuition that explanatory relations are antisymmetric (else we wouldn't consider them explanatory) — that no variable whose semantics makes its intension supervenient on the intension of another variable can be a cause of the latter.

It follows that if variable X in Maraun's (1996a) common-factor Model 1 is constructively defined by his Equation 3, then X supervenes on data variables Y and, again by intuited antisymmetry of explanation, cannot be a cause of Y . (Why that construction should also preclude the other properties that Maraun says X cannot have remains unclear.) However there are three impediments, two somewhat peripheral but the third at dead center, to this argument's attempted sundering of common factors from causal efficacy. First, unless Maraun has managed to break new ground in construction technology, Equation 3 can define X in an application only if the model fits the data exactly — which never holds in practice. Secondly, Equation 3 can describe an actualized construction only if all inputs to this are identified; and Maraun never lets on how to choose any specific S_i in Equation 3 from the space of options for this. (Even with an exact model fit, this is harder than he may realize.) And most importantly, even were Model 1 to fit the application exactly, a central flaw in Maraun's construction argument would be that although every $\langle X, \delta \rangle$ that satisfies Equation 1 under the application has the same extension as some $\langle X_i, \delta_i \rangle$ constructed from Y and a suitably constrained S_i by recipe Equation 3, it does not follow that every $\langle X, \delta \rangle$ satisfying Equation 1 is *intentionally* identical to some $\langle X_i, \delta_i \rangle$ so

¹³ In unpublished work on scientific lawfulness, I have argued that we can because we must. (The argument is more motivational than impeccably deductive.) This intuition of possibility is especially strong for lawfulness governing variables such as mw whose values represent properties of groups of individuals abstracted from properties of the group's individuals.

constructable. (Presuming that to be so is the "untenable tacit premise" to which I alluded earlier.)

To clarify this point, we need to enrich orthodox multivariate algebra with some theory recognizing the intensionality of its variables. The standard conception of a *space* S of variables takes this to be a set of jointly distributed variables (nevermind distributed over *what*) that is closed under linear combination. That is, the weighted sum of any subset of S -variables is also in S . And it is a standard theorem that if space S has finite dimensionality n , and none of the variables in an n -member subset b of S is a linear combination of the others, then every variable in S is a linear combination of the variables in b . This is true, however, only if "equals" is understood (as classically it has been) as "extensionally equals", meaning identity of extension. For if linear compositing is taken to be a *construction* procedure (which is the only interpretation that insures the existence of all linear combinations of extant variables) then, for any subset s of S , every linear combination y of s supervenes in intension on each variable in s whose compositing weight for y is nonzero. So when S is posited to be fully closed, intensionally as well as extensionally, by unbounded iterations of linear compositing which endlessly repeat the same extension reached through different construction routes, the relation of constructive supervenience defines a partial ordering of the variables in S . And for any S -variable s , the subset $=_e(s)$ of S containing all S -variables having the same extension as s includes infinitely many that differ from s in intension but are connected to it by supervenience at various construction distances. Moreover, when s_i supervenes on s_j in S , it is entirely possible though not mathematically guaranteed that $=_e(s_i)$ also contains S -variables *not* supervenient on s_j . That is, an s_i that derives from another, s_j , by construction can have the same extension as other variables that are constructively independent of s_j .

Application of a common-factor model, say Maraun's (1996a) Equation 1, starts by positing a set B of jointly distributed variables that includes the to-be-factored Y while having dimensionality much higher than that. In fact, we are free to presume that B is any suitably large subset of all variables that are jointly distributed in whatever population the application has selected. However this B is identified (an issue which to my knowledge has never been addressed in the factor-analytic literature), it needn't be a space closed even extensionally much less intensionally because any received B can be expanded into a fully-closed S_B by constructive stipulation. And some subset B^* of B will comprise all variables therein that do *not* supervene on any variables in Y , whence the fully-closed subspace S_{B^*} spanned by B^* is

likewise superveniently independent of y . What matters in this for Maraun's argument (assuming the model to fit the application exactly) is that even though common factors created from (in part) Y to satisfy his Equation 1 by Guttman's construction are disqualified by supervenience from being causes of Y , it is entirely possible that some of these have the same extension as certain variables in S_{B^*} , one of which might very well be an explanatory source of Y . Indeed, it would be entirely proper for factor analysts to stipulate that what they would mean by "common factor" in an application of Maraun's Model 1 is a satisfier thereof in placeholder position " X " that does *not* supervene on Y , that is, it is required to be a variable in S_{B^*} . That would rule out all Equation 3 Guttman constructions; but for any such Guttman-constructed X , any variable in the intersection of $=_c(X)$ with S_{B^*} is acceptable. To be sure, that intersection may be empty, in which case no intentionally acceptable solution exists. But practitioners have never presumed the output of common-factoring to be interpretively a sure thing. I await Maraun's explanation why adding this intensionality constraint to the model would be metaphor external to the model's math.

Although the mathematics of partially-ordered sets should help us impose some surface structure on thinking about supervenience relations defined by linear (also nonlinear) compositing, the broader logic of variables' intensions, or for that matter attribute ontology more generally, is still so obscure that we must expect progress in our understanding of this to be slow. But at least we can now sense the looming of some theory thereof awaiting development from rudiments already in hand. Maraun has insisted that we turn over this rock; and although it seems to have landed on his foot, the strange creatures in science's deeper ontology this has exposed are sufficiently fascinating and possibly important to have made Maraun's endeavor worthwhile after all.

References

- Kim, J. (1993). *Supervenience and mind*. Cambridge: Cambridge University Press.
- Maraun, M. (1996a). Metaphor taken as math: Indeterminacy in the factor analysis model. *Multivariate Behavioral Research*, 31(4), 517-538.
- Maraun, M. (1996b). Meaning and mythology in the factor analysis model. *Multivariate Behavioral Research*, 31(4), 603-616.
- Mulaik, S. A. (1996). On Maraun's deconstruction of factor indeterminacy with constructed factors. *Multivariate Behavioral Research*, 31(4), 579-592.
- Putnam, H. (1962). The analytic and the synthetic. In H. Feigl & G. Maxwell (Eds.), *Minnesota studies in the philosophy of science*, Vol. III. Minneapolis: University of Minnesota Press.

W. Rozeboom

- Quine, W. O. (1953). *From a logical point of view*. Cambridge, MA: Harvard University Press.
- Rozeboom, W. W. (1958). The logic of color words. *Philosophical Review*, 67, 353-366.
- Rozeboom, W. W. (1966). Scaling theory and the nature of measurement. *Synthese*, 16, 170-233.
- 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.