V

The Indeterminacy Debate

> At any given moment there is an orthodoxy, a body of ideas, which it is assumed that all right-thinking people will accept without question...
> (George Orwell, as cited in Thorpe, 1978, p.6)

> Component and common factor analysis are often selected as much because of one's underlying epistemology or paradigm (Gorsuch, 1988) as because of knowledge... (Gorsuch, 1990, p.33).

1. *Introduction*

One might anticipate that because, as established by Theorems 1 and 2 of Chapter IV, random variates constructed in accord with (4.4)-(4.6) are *definitionally* factors to $\underline{\mathbf{X}}$, and, when $\underline{\mathbf{X}}$ is ulcf representable, there can be constructed an infinity of random variates that satisfy (4.4)-(4.6), there would have been little controversy surrounding the claims listed on Page 87, that:

i) The set C containing the common factors to $\underline{\mathbf{X}}$ contains an infinity of constructed random variates;

ii) If the variates contained in $\underline{\mathbf{X}}$ are ulcf representable, then they possess an infinity of common factors. Hence, there are an infinity of referents of the concept *common factor to* $\underline{\mathbf{X}}$, and this set of referents contains constructed random variates;

iii) Because constructed random variates are contained in C, realizations of the common (and, also, the specific) factors to $\underline{\mathbf{X}}$ can be taken. Hence, the phrase "common factors can only be estimated, but not determined", and variants thereof, are incorrect;

iv) At least some of the factors to $\underline{\mathbf{X}}$, those constructed in accord with (4.4)-(4.6), are not, then, "unobservable", "unknown", or "unmeasureable". Hence, it cannot rightly be said that "common factors to $\underline{\mathbf{X}}$ are unobservable", at least if, by *unobservable*, one means that realizations can't be taken on a random variate.

These insights might, then, have been impetus for the investigation of related issues, including:

v) The implications of the fact that Card(C)=∞ for linear factor analytic practice, and, in particular, the interpretation of generated results;

vi) whether set C *could* contain common factors to $\underline{\mathbf{X}}$ other than those constructed in accord with (4.4)-(4.6), these *other* common factors to $\underline{\mathbf{X}}$ being either unobservable causes or properties/attributes of the phenomena under study;

vii) How it could be the case that a set of linear equations and a few distributional specifications could be the ingredients of a tool that could be used by researchers to make inferences about the

constituents of unobservable domains (a clarification of the meaning of the concept *unobservable* a prerequisite to the investigation of this question);
viii) whether there was anything to the belief that the linear factor model was superior to component models on epistemological grounds.

But far from there having been general acceptance of the truth of (i) to (iv) as given by the mathematics of the linear factor model, let alone exploration of related issues, there ensued within psychometrics what has come to be known as the "indeterminacy debate."

It has been claimed that the indeterminacy debate, it having run for the majority of the life of linear factor analysis, is a debate about the implications of the indeterminacy property of the linear factor model (i.e., Theorem 2; the "non-uniqueness of the factor scores" implied by the model) for the model's "usefulness" in scientific work. Or, alternatively, whether this non-uniqueness property constitutes "a fatal flaw" in the model. But a more careful reading of the submissions to the debate reveals that the indeterminacy debate is an argument on a much grander scale. It is truly about whether the Central Account stands or falls. For if points (i)-(iv) are granted, then, at the least, a significant portion of the CA must be discarded. But, as was seen in Chapter III, psychometrics without the CA would be unrecognizable. It is the fuel that powers all of latent variable modeling and, rightly or wrongly, earns it its prestige. As a result, a procession of authors have stepped forward to attempt to put down the threat represented by the implications of the indeterminacy property. Anything to avoid the acknowledgment of conclusions (i)-(iv) and their possible consequences. This is unfortunate, because it has prevented the discipline from developing a rigorous characterization of latent variable models and modeling.

The task of Part 2 will be to show that the CA is part misportrayal and part nonsense. A rigorous account of latent variable models and modeling must dispense with it once and for all. The reality of latent variable models and modeling, for better or for worse, is found in a development of the implications of (i)-(v), and this task will be taken up in Part 3. In the present chapter, however, the aim will be merely to illustrate the various ways in which mainstream psychometrics has interpreted the indeterminacy property as a threat to the CA, and, in response to this threat, has gone about constructing protective barriers around the core theses of the CA. In particular, a selection of the commentaries on indeterminacy that comprise the indeterminacy debate are described, and it is shown that a number of these commentaries are tacit defenses of the Central Account.

2. *The indeterminacy debate*

E.B. Wilson (1928, *Science*)

By 1927, Charles Spearman had published extensively in support of his two-factor theory. Using normal statistical theory, Garnett had, it was thought, proven the "uniqueness" of the linear factor analytic resolution of intellectual measures into general and specific intelligences. In the *Abilities*, Spearman had called Garnett's "proof", which, in fact, did not establish what it claimed to establish, a "momentous theorem" (p.vii). The *Abilities* was the culmination of a weighty and lengthy research program. American mathematician Edwin B. Wilson reviewed this book for the journal *Science* in 1928. In his review, he acknowledges the importance of the book, but also states, in reference to Spearman, that "It may well be that he does not know exactly what his theories and facts signify; it is certain that I do not" (p.244). He reports that the mathematics contained in the book appears to be without error, and yet, that he is "...not entirely happy, satisfied" (p.245). As he explains, one source of his unhappiness is the *Abilities*' lack of an example worked through to the determination of the general and specific intelligences of a set of individuals. Whether he would have been satisfied with the explanation standardly offered by the modern latent variable modeler, to wit, that the quantities of interest were "unobservable", we will never know. As was mentioned in Chapter III, Spearman's work predated "unobservability talk", and, hence, Wilson quite reasonably read Spearman's stated aim to "objectively determine" *g* as the aim of producing a set of scores that possessed the properties that the two-factor theory claimed that *g*-scores should possess.

Wilson was one of the first, and very few, to ask of latent variable modeling, "what does all the talk *mean*". Perhaps he was able to pose such questions so candidly because he was not native to the social or behavioural sciences. Whatever be the case, he describes Spearman's talk of "intelligence as energy", and "special abilities as engines", as allegory, and asks how Spearman and Garnett could possibly use the term "unique" to describe the factor analytic resolution of variates into general and specific capacities. In doing so, he is expressing his discomfort with the kind of terminological ambiguity that has both informed the Central Account, and, ironically, protected it. For a target that cannot be put in focus is all the more difficult to strike. Because the linear factor model is stated in terms of equations, and Wilson is a mathematician, he reasonably enough believes that the "uniqueness issue" is resolvable in mathematics. What he perhaps does not realize is that he is trying to make sense of an applied mathematics through which is woven a metaphysics. Spearman's talk of uniqueness is not about the mathematical terms of the model, but about putative, extra-mathematical referents of the concept *g* whose relation to the mathematical symbols of the model remains unclarified.

Beginning on page 246, Wilson provides a worked example of his own. For a fictitious set of data representing the scores of six students on three tests, Wilson produces, for each student, two maximally distinct general intelligences, and two sets of specific intelligences. He concludes that "What we have shown is that the complete solution can be obtained but is indeterminate" (p.246). His comment that "We do not need the generalized Bravais distribution (as used by Garnett)...to make determinate (if it does) that which without it seems indeterminate" (p.246) seems to suggest that he was not aware, at the time, that the indeterminacy property remains even if normality is invoked (i.e., Garnett's (1922) claim to the effect that *g* is unique was in error). He discusses the state of Spearman's theory under transformation of the variates, and argues (p.247) that, in addition to the indeterminacy inherent to the model, the concept *general intelligence* "...is relative to the set-up..." That is, *g* is not, in general, invariant under transformation of the variates. He pointedly notes that "Although hypothetical unrealities may illuminate the significance of realities, it is the realities that make science. All I was trying to do was to supplement Spearman's discussion of the universality of g with a little contribution on the relativity of g..." (p.247).

E.B. Wilson (1928, *Proceedings of the National Academy of Sciences*)

In this article, Wilson formalizes and extends his earlier analysis of the indeterminacy property of the linear factor model. He provides a geometrical analysis to further clarify the meaning of indeterminacy, and derives the $\mathbf{D}_{\theta}+\mathbf{I}_{\theta(i)}$ style decomposition of the common factor into two components, one a linear combination of the manifest variates, the other arbitrary save for mild moment restrictions (see 4.13 of this book).

E.B. Wilson (1929, *J. of General Psychology*)

In this article, Wilson reviews T.L. Kelley's *Crossroads in the Mind of Man: A Study of Differentiable Mental Abilities* (1928). Kelley's book was concerned with the resolution of tests into a set of general and unique factors, and, hence, was a primitive attempt at what would later be called multiple factor analysis. Wilson notes that "The author makes it clear that his general factors or traits are group phenomena. He does not go on, any more than did Spearman, to assign to each individual in the group examined his appropriate rating on each general factor" (p.154). Throughout the article, Wilson seems to take pains to be polite about factor theory, and, yet, is clearly irritated by the lack of precision in its prevailing statements. He pushes for an acknowledgment of the differences between issues of unreliability and indeterminacy: "...this problem is highly indeterminate- not in the sense that the tests are unreliable in that the individuals do not do themselves perfect justice (neither more nor less), but in the sense that the conditions imposed by the theory of the general and specific factors are not sufficient to determine the individual values, even supposing the scores made on the tests to be perfectly reliable" (p.154). He provides perhaps the first serious discussion of the identifiability of the parameters of the linear factor model, states that "...Kelley gets a solution and I understand that he thinks it is unique" (p.157), and, at several points, asks rhetorically "...how the author is entitled to his belief...that he has a unique solution" (p.159).

He questions what is meant by the concept *general factor*, and hints at the existence in factor theories of a conflation of, or, at least, equivocation over the relation between, the technical term *general factor* as employed in factor analysis, and ordinary language trait-terms (p.161). Indeed, both Spearman and Kelley, and many others since, pass between the trait terms of ordinary language, e.g., *creativity*, *dominance*, *agreeableness*, on the one hand, and the technical term *g* or *factor*, on the other, without so much as a pause, thus implying their interchangeability. Anticipating one of the lines of argument that will be pursued in the current work, Wilson questions the degree of isomorphism between the factor model and that which it is supposed to model, and, in particular, factor theory and Kelley's "canal theory of traits": "How does this bear on the canal theory of traits? I do not know. Mathematically the theory of resolution into general and specifics gives no indication of canals..." (p.164). Following Wilson's lead, it will be argued, herein, that, for anyone but a native raised on "latent variable talk", it is far from clear how "unobservables", "causal sources", and "latent properties" emerge out of the equations and distributional specifications of a latent variable model. Finally, Wilson questions how the use of factor analysis could yield measurements of particular traits, when the traits believed measured would, according to the equations of the model, change with the particular tests factored: "To my way of thinking this is fatal to the whole theory unless we mean by the theory only a convenient analysis of a specified group of tests" (pp. 167-169).[1]

C. Spearman (1929, *J. of Educational Psychology*)

In Spearman's first response to the concerns raised by Wilson, he opens with: "If any event is more likely than another to quicken the progress of psychological mathematics, it is the

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1 Behaviour domain theory will have something to say on this issue.

entry on the scene of a mathematician so eminent and so free of prejudice as Professor E.B. Wilson" (p.212). He acknowledges Wilson's demonstrations that "...attempts to determine "g" fail really to do so" (p.212) and reiterates Wilson's finding that "...g remains within large limits undetermined; *it cannot be regarded as unique*" (p.213). He then claims that he "...was urging very much the same thing myself" (p.213), and supports this claim with a reference to the appendix of his *Abilities*. But that he "was urging very much the same thing" is simply untrue. In the appendix to which he refers, he *does* provide a $\mathbf{D}_{\boldsymbol{\theta}}+\mathbf{I}_{\boldsymbol{\theta}(i)}$ representation of *g* (based on a determinantal expansion) akin to that of Wilson, in which the indeterminate part is described as "...any new variable uncorrelated with all the others." As Piaggio (1933, p.89) would later claim, Spearman was thus the first to reveal the indeterminacy inherent to his model. Unfortunately, he was *unaware* of his accomplishment, for while Wilson had correctly characterized the indeterminacy property as the non-uniqueness of the variates that could rightly be called "factors" to a set of measures, Spearman portrays it as an issue of *unpredictability*. In particular, he takes the $\mathbf{g}=\mathbf{D}_{\boldsymbol{\theta}}+\mathbf{I}_{\boldsymbol{\theta}(i)}$ decomposition to be analogous to the $\mathbf{g}=\hat{\mathbf{g}}+\boldsymbol{\varepsilon}$ decomposition of a linear regression analysis, in which $\boldsymbol{\varepsilon}$ is the error inherent to the prediction of the *single*, *unique*, *thing*, **g**, on the basis of a team of tests.

Spearman must, therefore, be credited with the invention of a manoeuver which will be put to good use in the years to follow. That is, he gives the impression of acknowledging the truth of the mathematics of indeterminacy, while, at the same time, misrepresenting them to protect the Central Account. In this case, he speaks as if he is granting the truth of the mathematics, while simultaneously mischaracterizing them as being about prediction error. In the first place, as Thomson will later comment, Spearman would only have the right to this portrayal if he were able to produce, prior to a factor analysis, the rule by which the scores he is trying to predict can be generated. For, without such a rule, it is not clear at all what it is that he is trying to predict, and without clarity in this regard, there cannot be clarity in regard the meaning of the term *measurement error*. But Spearman is in no position to provide a rule for the generation of *g*-scores prior to a factor analysis. Secondly, Wilson shows that, if a particular set of variates is describable by the linear factor model, then, *following analysis*, such a rule does exist (see (4.4) and (4.6), Chapter IV). However, this rule makes clear that the variates that Spearman wishes to employ as *predictors* of **g**, i.e., the tests, are actually components in the construction of **g** itself (i.e., they are a part of **g**). For both reasons, Spearman has no right to portray the tests as predictors of *g*, hence, the $\mathbf{I}_{\boldsymbol{\theta}(i)}$ component as an error in the prediction of **g**. His manoeuver perhaps generates a strong enough illusion to protect the essential idea that there is but *one* thing signified by *common factor to* $\underline{\boldsymbol{X}}$ (regretably, predicted with error) against the wholly unsatisfactory implication of Wilson's work that *common factor to* $\underline{\boldsymbol{X}}$ signifies a set of constructed variates. His work can then proceed under the illusion that *g* might yet signify an existing causal source, e.g., mental energy.

In the final section of the paper, Spearman provides a primitive version of a now popular "solution" to the unpredictability problem that he has diagnosed. He states that Wilson's "...writings have left on many readers the impression that the indeterminateness of *g*...is essential and final. Whereas I believed myself to show how it can be remedied" (p.214). His suggested remedy is to find a (k+1)th test that happens to be perfectly correlated with *g*, from which "...there at once ensues without a shadow of doubt the perfectly determinate value for *g*, namely an individuals score on this test. In regards the possibility of actually finding such a test, Spearman claims that "Unpublished work in our laboratory has more than once obtained for an

r_{ag} values of .99...nothing stands essentially in the way of raising it much higher still; in fact, as near as desired to unity" (p.214).

E.B. Wilson (1929b, *J. of Educational Psychology*)

In Wilson's rebuttal to Spearman (1929), he reiterates his "transformation theory", and the conclusion he had previously drawn from it, that "the solution of *g* depends on what the psychologist determines it shall be...There is the possibility that it throws some suspicion on the objectivity of *g* unless all psychologists agree to make the determination in the same way or in equivalent ways..." (p.221). Here, Wilson is, once again, taking exception to what Spearman seems to imply in his work, i.e., that he has discovered a particular, unique, thing. Note that if Spearman had made the claim that he had discovered a new kind of bear, his scientific peers would at least have been able to judge the correctness of his claim, for there exists a concept *bear*, and terms to single out particular types of bear, whose rules for correct ascription to certain living entities they would have grasped. Wilson's point is that the only criterion of application Spearman has for the concept *g to* $\underline{\mathbf{X}}$ is that provided, in an application of the model to $\underline{\mathbf{X}}$, by the model itself (and spelled out in Theorem 1, Chapter IV, of the current work). The model provides a *definition* of this concept: *g to* $\underline{\mathbf{X}}$ is correctly applied to a variate that satisifies the requirements spelled out in Theorem 1 of Chapter IV. The model cannot therefore be used to test for the existence of some constituent of natural reality that is signified by a concept *g to* $\underline{\mathbf{X}}$ that is defined external to the model. The situation is in marked contrast to quantity terms such as *electrical resistance* and *power*, whose meanings *are* laid down once and for all by stipulative definition, and which denote natural phenomena whose properties can then be studied by scientists.

In regard to Spearman's suggestion about a (k+1)th test that is perfectly correlated with *g*, Wilson's comment is as follows: "Fine! That would suit me provided it can be done psychologically rather than analytically. It is impossible that r_{ag}=1 without making a the test of *g*. There is no specific ability whatsoever, nothing but sheer general intelligence required to score in test a. Once we have come by any means whatsoever to an a which makes r_{ag}=1 we may throw away our scaffolding" (p.221). Here, Wilson carefully distinguishes between a *g implied* in a set of model equations (analytically), but with no antecedently stated definition, and something signified by a concept *g* that has been *defined* (externally to the model). The distinction between a model based "definition" and a proper stipulative definition has often been obscured in the latent variable modeling literature. Lovie and Lovie have recently claimed that Wilson's aim in his analysis of Spearman's theory was to "rescue Spearman from what he considered to be the ill-thought-out consequences of the mathematics of the two-factor system" (1995, p.238). This characterization is pretty close to the truth of it. What Wilson hadn't bargained for was Spearman's unwillingness to be rescued.

Piaggio (1931, *Nature*)

Piaggio begins by paraphrasing Spearman's theory: i) If all tetrad differences vanish, "each variable may be considered as the sum of two parts (or "factors") which are numerical multiples of a general factor *g* (the same for every variable) and a specific factor (different in each case). These N+1 factors are uncorrelated with each other" (p.56). This he takes Spearman to have proven in the appendix of *Abilities*; ii) "The attribution principally to mere error of sampling of the non-vanishing of the small tetrad differences formed from dissimilar mental measurements" (p.56); iii) "The interpretation of *g* as general mental energy, and of each *s* as a specific ability" (p.56). He notes that in the appendix of *Abilities*, "The value of *g*...is given in the form of a complicated determinant involving a variable *i* which is undefined except that it is "any new variable uncorrelated with all the others"" (p.56). His aim is to provide a "...straightforward method by which an equivalent but much simpler expression can be obtained, and to show the nature of the mysterious variable *i*" (p.56), and, true to word, he provides formulas for *g* of the "determinate plus indeterminate part" variety (i.e., formulas equivalent to (4.13).

While it further elaborates the mathematics of indeterminacy, Piaggio's work is also a defense of the Central Account, as is evident in his interpretation of indeterminacy. In particular, Piaggio notes that "Some may consider that the occurence of the chance or uncertainty factor *i* in the above result robs it of all real value. But if the two-factor theory is true, the uncertainty cannot be avoided, for from N equations we cannot determine the (N+1) unknowns (one *g* and N *s*). Moreover, by increasing N the coefficient of uncertainty term can be made as small as we please" (p.56). That is, Piaggio is in agreement with Spearman that the indeterminacy property is just the unavoidable uncertainty inherent to the determination of an individual's *true*, existing *g*-score. Each individual *has* a true *g*, but, under usual circumstances, the factor analyst cannot *know* it. Furthermore, he hints at the same kind of solution as in Spearman (1922): One should add additional tests to the original team of tests. Finally, he urges psychologists to step out from the factor analytic framework in their attempts to identify *g*: "...consider whether there is sufficiently good correspondence of these variables with what on other grounds may be considered general mental energy and specific ability" (p.57). This is an important point. Clearly, Piaggio believes that *g* may well be an entity of some sort, something that could, potentially, be identified on grounds independent of the factor model. This is just CA2 together with CA5: i.e., that a latent variable model may be used to detect existing entities whose identities must then be revealed.

C. Spearman (1931, *Nature*)

In his response to Piaggio (1931), Spearman endorses Piaggio's mathematics, but corrects him in that he does not consider "...the interpretation of *g* as general mental energy, to be any essential part of the theory." Essential for Spearman is "...that the determination of *g* and *s* leads on to that of "group" factors; and then the varying magintude of all three kinds of factors under varying conditions connect them up with all the laws of the human mind...Thereby, I believe, psychology is placed upon a new basis, in which the old but still prevalent "faculties" are replaced by statistically established unitary functions...all these positive observations are at present being side-tracked by undue prominence given to such speculative (however luminous) hypotheses as that of a "general energy"" (p.57). These are ambitious plans given that Spearman had yet to adequately respond to Wilson's suggestion that key of the terms in his theory were mere allegory, and given that he had not bothered to clarify what he meant by "statistically established unitary functions". Given his equivocation over what he meant by *group factors* (i.e., the meaning of the concept *common factor to* $\underline{\mathbf{X}}$, the class of entities that are its referents, if, in fact, it does denote), it is equally unlear what Spearman meant by the claim that such factors can be "connected with all laws of the human mind". Finally, Spearman's response illustrates the have-your-cake-and-eat-it-too attitude of the proponent of the CA. After making quite some noise about "mental energy", Spearman feigns its unimportance when Piaggio tries to make it stick.

B.H. Camp (1932, *Biometrika*)

Using N-dimensional geometry, Camp derives, with extensions, the results of Wilson, Piaggio, and Irwin. His comments on the indeterminacy property are particularly interesting. He states that "...the values of g may be assigned to the N individuals in many ways, at least provided N is greater than n+1. This means that we may choose, for example, for the general factor applicable to the first individual a value twice as great or half as great as the one assigned to the second individual, at pleasure, and still meet all the conditions of this theorem. This sort of factor is not, I should think, what psychologists desire in order to establish the two-factor theory of mental abilities. If the number of ways in which one may assign values of the general factor to the N individuals is very large instead of unique, it would seem to me doubtful whether from a psychological point of view it would be meaningful to assert the existence of such a "factor" at all..." (p.424). Several points are worth noting. First, Camp reminds the psychologist that there exist different senses of the concept *factor*, the technical sense that arises in factor analysis only one possibility, and suggests that the factor analytic sense may not be what is wanted to anchor a psychological theory of mental capacities. This point requires emphasis because latent variable modelers often seem to take technical terms inherent to their trade, e.g., *latent trait*, *latent variate*, *common factor*, as synonymous with ordinary language concepts such as *factor responsible for...* and trait- and disposition-terms such as *intelligence* and *mathematical ability* without ever establishing that the grammars of these concepts license such a treatment. Second, Camp raises the issue that there may exist a lack of isomorphism between the intended meaning of Spearman's theory and the mathematical representation he chose for it. Indeed, there is nothing in Spearman's theory of intellectual capacity that implies his mathematical treatment (and, in fact, he was merely trying to "objectively define" *general intelligence*). Third, he introduces an unresolved sense of the concept *existence*. E.B. Wilson had already shown that, when an $\underline{\mathbf{X}}$ is described by the linear factor model, there exist an infinity of variates each of which is a common factor to $\underline{\mathbf{X}}$. In questioning the sense of asserting the existence of factors given the indeterminacy property, Camp has clearly introduced a distinct, unclarified, sense of the concept *existence*. It appears that he is, in fact, speaking to the issue of the existence of the unobservables mentioned in the Central Account.

Camp closes the paper by drawing an important distinction. Piaggio and Irwin had discussed the situation in which g was "almost unique." By this they meant that, under certain circumstances, distinct g constructions (variates) might be nearly identical in a mean-square difference sense. But Camp notes that this is not the same thing as establishing that the g scores of a *given individual*, let alone those of every individual, are nearly identical across distinct common factors to $\underline{\mathbf{X}}$ (i.e., variates contained in C). In fact, even when the distinct g constructions are "nearly identical" in the sense of Piaggio and Irwin, "...whatever individual be selected, it is possible to find at least one member of that family for which his g will differ greatly" (p.424).

Piaggio (1933, *British J. of Psychology*)

This paper was a detailed elaboration of Piaggio's 1931 paper. In it, Piaggio proves the necessity and sufficiency of his factor construction formulas for the unidimensional common factor model. That is, he proves that factors constructed in accord with his formulas must satisfy the requirements imposed by the model, and, converesly, that any variates which satisfy the model imposed requirements for factor-hood must be expressible as per the formulas. He begins by noting that Spearman "...at one time asserted...that *g* was unique if the tetrad relations were satisfied. This statement was challenged by E.B. Wilson...and it has now been withdrawn" (p.88). Piaggio views the determinacy of the factors of the model as a necessity for "...the two-factor theory to be valid in a form applicable to psychology" (p.97), and the indeterminacy inherent to particular factor analytic representations as fixable. He believes that "...*g* exists and is real, but its numerical value is indeterminate" (p.89), and that "...we find that no set completely satisfies the condition for the uniqueness of *g*, though recent sets do so more nearly than older ones" (p.89). To clarify, Piaggio believes that there exists a real thing, *g*, detected in the use of the factor model, and, with respect to which, measurements are desired. He disagrees with "...Thomson's contention that no general factor exists" (p.101). Each individual in the population under study has a unique measurement with respect *g*, but this measurement cannot be determined without error. Indeterminacy is then, according to Piaggio, a measurement problem. The larger issue is "...that the demonstration of the existence of a real and approximately numerically unique general factor arising from mental tests is only one step, from a psychological point of view. Having obtained what so far is merely a statistical entity, the next step is to enquire what psychological properties, if any, it possesses" (p.102). Once again, this is just CA2 with CA5.

In practice, the number of individuals is ..."taken large to reduce the error of sampling as much as possible and so is much greater than *K* [the number of tests], and *i* is then highly indeterminate, and consequently *g* is indeterminate also, unless the coefficient of *i*...is zero" (p.97). Determinacy is achieved in the limit if $\sum \frac{r^2_{ag}}{1-r^2_{ag}} \rightarrow \infty$. As did Spearman, Piaggio mentions the possibilty of achieving determinacy by finding a test that is correlated perfectly with *g*, but concludes that, if such a test can't be found, the researcher can always take "...the number of tests to be infinite" (p.97). Now, if indeterminacy is simply a measurement problem, i.e., a problem having to with imprecision in claims about some particular property κ, made on the basis of numbers yielded by application of a rule for the measurement of property κ, then Piaggio's treatment might suffice. However, as with Spearman, he provides no support for his presumption that the application of the linear factor model yields measurements of a particular property called *general intelligence*.

Finally, Piaggio discusses the measurement of indeterminacy and suggests taking the ratio of the standard deviations of the indeterminate and determinate parts of *g*. Later (p.105) he briefly considers taking the ratio of variances, and acknowledges that this is the method prefered by Holzinger and Spearman. He states that "...the only practicable way to make the indeterminate part of *g* negligible is to find an r_{ag} which is nearly unity" (p.98).

Spearman (1933, *British J. of Psychology*)

In this reply to Piaggio (1933), Spearman begins by noting that "...the equation $x^2=a^2$ yields the solution that $x=a$; but this result is not unique inasmuch as an equally good solution would be $x=-a$. And instead of two possible solutions to an equation, there may be a great number" (p.107). He then contrasts this "non-uniqueness" with "non-exactness": "Thus the rise of temperature may be measured sometimes with an error not exceeding a millionth of a degree; but vain would be the attempt to measure it without any error at all" (p.107). According to Spearman, the difference between the two problems is as follows: "It would seem that in the former the indeterminateness or freedom is thoroughgoing; it pervades the whole affair. Whereas in the cases charged with lack of exactness, only the measurement is indeterminate, not the magnitude to be measured. There *is* a unique distance from the person to the bridge..." (pp. 106-107). With regard to the indeterminate component of **g**, the *i* component in Spearman's notation ($\mathbf{I}_{\boldsymbol{\theta}(i)}$ in the current work), Spearman asks, "Should it be regarded as a lack of uniqueness, or only as one of exactitude?" (p.107). His answer is a reiteration of his 1929 rebuttal to Wilson: "...this *i* is nothing else than the error just mentioned as being due to the limited number of tests available for the purpose of measuring. Moreover, it is nothing more than the probable error given by Holzinger for the regression equation...I suggest that to charge *g* with not being "unique" because of its "*i*" is linguistically inappropriate and is likely to cause some serious under-valuation of its significance" (p.108). Hence, in Spearman's opinion, there do not exist multiple *g*s in the same way that there exist multiple solutions to the equation $x^2=a^2$. Why does he believe this? Certainly, as Wilson proved, Spearman's opinion cannot be supported mathematically, for set *C* has infinite cardinality. So Spearman must have in mind some other form of evidence. Now, if he were to have claimed that there existed only one red-striped zebra in England, he would have been expected to present observational evidence of the existence of this single zebra, and, also, evidence that there existed no more than just this single zebra. Because Spearman cannot resort to mathematical support for his belief, has he then *seen* just one *g* per individual? In truth, his stance is a *presupposition* of his theory and employment of factor analysis, and, of course, a cornerstone of what is herein called the Central Account.

As pointed out by Wilson, Spearman himself never offered up anything that he claimed to be the measurements of the *g*'s of the individual's he studied, nor a rule by which such measurements could be produced. And with good reason. The concept of *distance* has a well established meaning, and is founded on a normative practice of measuring the distances between geographical locations, whereas the meaning of the concept *general intelligence* was the subject of marked equivocation, it possessing no "definition" other than given by the model itself. The strong feelings of recognition researchers have for the concept are purchased through questionable identifications of it with various ordinary language concepts (*intelligent*, *brilliant*, *dull-witted*, etc.). While competent adults are expected to grasp normative rules for the measurement of *distance*, and be able to *take* measurements of the distances between, e.g., towns, mountains, points, etc., it would not be at all clear to even a sophisticated scientist what was meant by the request "please measure Joe's g for us". There exist standards of correctness for making the claim that *these* numbers, {1.4., 1.3, 2.4, 6.5}, are measurements of distances between towns, namely that they were produced in accord with rules for the measurement of distance. The existence of such rules is what allows one to coherently discuss various types of *error* in the measurement of distance, and defend one's claim that *these* numbers are measurements of the distances between the following pairs of towns. Moreover, the fact that there exists a normative practice of measuring distances allows for the refinement of techniques for the measurement of distance. And, while the physicist is acquainted with techniques that far outstrip what the child can offer in regard the exactitude of measurement, the more sophisticated techniques nevertheless presuppose the more primitive. Spearman's response is nothing but the

Central Account: i.e., the belief that there exists but one particular thing, *g*, but this single thing is measured with error.

The final contribution of the paper is a more forceful statement of Spearman's "infinite number of tests" solution: "..indeterminateness derives from the fact that the valuation of *g* can only be obtained from a limited number of tests, each having only a limited correlation with *g*. Here again, there is obviously an ideal value aimed at; it is that which would be obtained from an infinite number of tests..." (p.107). This is the seed from which will grow the behaviour domain domain response to indeterminacy, and, eventually, McDonald's abstractive property position.

B.H. Camp (1934, *Biometrika*)

In this response to Spearman's (1933) reply to Piaggio (1933), Camp explains that he "...cannot but ask a brief space in order to point out as tersely as possible and without adjacent forbidding-looking mathematics two quite definite errors which it seems to me Spearman has made in his reply" (p.260). In the first place, Camp disputes the legitimacy of Spearman's distinction between the indeterminacy inherent to the factor model, and the non-uniqueness inherent to the solution to the equation $x^2=a^2$: " It seems to me impossible to recognise a difference in *type* between these two cases" (p.261). He paraphrases Spearman as saying that "...the arbitrary term (k*i*) in the formula for *g* represents the "inexactitude" of one's *determination* of *g*, not a multiple nature in *g* itself" (p.261), and asserts that "Spearman appears to think that, because the variability of *g within each of these sets* is limited, it follows that the variability of a *g* from set to set for each fixed individual is also limited. This is not true..." (p.261). According to Camp, "...it is easy to select a group of sets of *ki*'s in which the variability within each set is as small as he indicates but for which the variability from set to set for any individual chosen arbitrarily in advance is very, very large" (p.261). That is, Spearman has no business treating the indeterminate component $\mathbf{I}_{\boldsymbol{\theta}(i)}$ as if it was a measurement error term, because this wrongly implies that, as its magnitude decreases, the variability inherent to *each* individual's set of *g*s (over constructions in *C*) decreases. Camp claims that Piaggio's language "...encourages one to fall into this error" (p.261). However, while Camp is quite correct in his analysis of this brand of variability, he misunderstands *why* Spearman misinterprets the fact of there existing many *g*s. Certainly, Spearman's misunderstanding does not result from his belief that "because the variability of *g within each of these sets* is limited, it follows that the variability of a *g* from set to set for each fixed individual is also limited", but rather his commitment to the metaphysics of the Central Account. That is, the mistake Camp reveals in Spearman (1933) is not an isolated mis-step, but part of a general commitment to the picture herein called the CA.

Camp's conclusions regarding the implications of indeterminacy for Spearman's project are unambiguous: "If, before looking at Smith's scores on the tests, one may choose a number at random (subject only to the broad limitations mentioned before), and can then demonstrate that this number can be assigned as Smith's *g*, as well asany other number, and in perfect harmony with all the other hypotheses, then it is meaningless to assert that Smith *has* a *g*" (p.261). Furthermore, he follows Wilson (1929b) in reminding factor analysts of the essential distinction between the technical concept *factor* as it arises in linear factor analysis, and senses of the term external to the model: "It is not, of course, contended that a unique general factor does not, in truth, exist, but that its existence does not follow from Spearman's hypotheses. Moreover, it would seem inherently impossible that it could follow from his hypotheses, for into them he has introduced only group averages..." (p.261).

Thomson (1934, *British J. of Psychology*)

Following Spearman's employment of the "error of prediction" interpretation of indeterminacy, Thomson sets out to make "...a more complete comparison between Spearman's *g* technique and the ordinary method of the regression equation than has to my knowledge been set out before" (p.92). In short, Thomson takes exception to Spearman's regression interpretation: "Spearman's case is exactly the same as the above except for the important fact that he has no "criterion", no measure of *g* except through the team of tests..." (p.94). To paraphrase, what constitutes a **g** variate to a team of tests is settled via the team of tests themselves and the model equations, or, less tangentially, equations (4.4)-(4.6). On the other hand, in the prediction of marks in senior year by tests at entrance, "...there is no doubt about the actual existence of these latter. They are awarded by quite independent means and the accuracy of the prediction can be checked" (p.97). Two points bear noting: i) As did Wilson and Camp, Thomson carefully distinguishes between the technical concept *factor* that is applied to any variate that satisifies the model imposed requirements for factor-hood, and other senses of the term that might arise in other contexts; ii) Along with many latent variable modellers, Thomson appears confused over the concept of *existence*. If the issue is *mathematical* existence, then the evidence one must supply is a mathematical proof. As will be recalled, Wilson provided just such a proof: When a set of manifest variates $\underline{\mathbf{X}}$ is described by the linear factor model, then there exists an infinity of factors to $\underline{\mathbf{X}}$, these being variates constructed in accord with (4.4)-(4.6). Hence, there is no question as to the *existence* of *g*-variates. But, in fact, *existence*, as employed in the writing of Thomson, and others working in the area of latent variable modeling, strays from its mathematical sense into various metaphysical and ontological commitments. Interestingly, the bluring of these various senses of *existence* serves a defensive role in regard the propogation of the CA, for it allows latent variable modellers to tacitly assert the ontological commitments of the CA, while at the same time pretending that these commitments are a product of cold, objective mathematics.

Thomson clearly recognizes Spearman's assertion of many of the theses of the CA, and does not object in principle, but, rather, suggests that the case for the CA is not iron-clad: "In the Spearman case however there is no other evidence of the existence of "*g*" than the magnitude predicted for it....Its only attributes are mathematical estimates, and to the extent to which these are indeterminate, one may perhaps hold that "*g*" itself, being nothing else, is indeterminate...It is of course convenient to make the hypothesis that a real *g*, perfectly determined, exists and that the quantity *i* expresses merely an uncertainty in the measurement, not any doubt as to the existence. But it is only a hypothesis in this case" (p.97). The question that Thomson must answer is the following: If the linear factor model is the only means by which the technical concept *g* is defined, and Wilson has proved that if $\underline{\mathbf{X}}$ is described by the model, then $\underline{\mathbf{X}}$ possesses an infinity of *g*-variates, what could be meant by a "real *g*" that is "perfectly determined", and whose existence is in question and must be hypothesized? What is this other *g* to which Thomson refers?

Thomson (1935, *J. of Educational Psychology*)

In this important paper, "The main object...is to elucidate further the nature of both of the indeterminate variate *i* in the measurement of *g* and of *g* itself" (p.241). While Thomson makes a major contribution in developing what Steiger and Schonemann (1978) would later call the transformation approach to indeterminacy, what is of interest to the current work is his interpretation of indeterminacy. On this note, he states that "An orthogonal matrix will be arrived at which will transform any set of values for *g* and the specifics, possessed by a given individual, into any number of different sets of values for *g* and the specifics which will give exactly the same scores in a *hierarchical* set of tests, so that we do not know with certainty which set of values the individual really possesses..." (p.241). Now this sentence is curious, for, in speaking of "not knowing with certainty which set of values the individual really possesses", Thomson implies that an individual really does have, independent of analysis, a single "true" set of scores ("true" measurements in respect the factors), but that, because of the indeterminacy property, these scores are not knowable. This is very much in keeping with tacit commitments present in his 1934 paper. Once again, Thomson is not against the CA. In fact, it informs his own analysis of Spearman's work. Rather, he finds Spearman's *case* for the CA scientifically problematic.

In the final sections of the paper, Thomson states that "There are then many *g*'s. Whether we care to call one of them general intelligence is a psychological, not a mathematical, question" (p.261). Here, his comments echo those of Wilson and are at odds with his earlier tacit assertions of the CA. But this likely speaks to the strength of the CA to undermine even the best attempts to get things right. The tension between the CA and reason will very often produce logical inconsistencies in the indeterminacy debate. Considering the question of determinacy in the limit, Thomson's views are as follows: "Any three tests *define* a *g*. But they do not *measure* it exactly unless it happens that the product of two of their correlations exactly equals the third correlation" (p.260); "...when it is said here that a certain *g* can thus be measured exactly, what is meant is that the inexactitude due to using a hierarchical team of tests has been abolished" (p.261). So, here, Thomson supplants Spearman's prediction analogy, which he had previously criticized, with a measurement analogy that is equally CA infused. At the root of both analogies is the idea that there exists a unique, particular something to be predicted/measured, but which cannot be measured/predicted with exactitude on the basis of a finite team of tests.

J.O. Irwin (1935, *British J. of Psychology*)

In this paper, Irwin calls constructions of **g** (variates constructed in accord with (4.4) and (4.6)) "estimates" of **g**, implying, once again, that variates constructed to satisfy all model specified requirements for common factor-hood are not *really* common factors to $\underline{\mathbf{X}}$, but rather, mere approximations to some unspecified *true* common factor to $\underline{\mathbf{X}}$. He reiterates the oft-repeated claim that as the number of tests increases indefinitely, the multiplier of *i* tends to zero, and, hence, *g* to determinacy, and concludes that "In this sense the *g* estimates may be regarded as determinate" (p.394). It is not clear what Irwin intends by the latter claim. Presumably the variate that is determined in the limit, should such limiting determinacy obtain, *is* **g** rather than a "determinate estimate of **g**". Moreover, he, along with many others, conveniently fails to note that determinacy in the limit obtains only under very special circumstances, and, hence, is not a consequence of simply increasing the number of tests indefinitely (as will be discussed in Chapter XV).

H. Kestelman (1952, *British J. of Psychology*)

In this paper, Kestelmann extends the work of Piaggio (1933) and Ledermann (1938). In particular, he derives factor construction formulas for the case of the multiple, orthogonal model, and proves the necessity of these formulas. Steiger and Schonemann (1978) suggest that Kestlemann's views on indeterminacy were somewhat equivocal. If one were looking to Kestelman's work for a blanket indictment of factor analysis on the basis of the indeterminacy property, then this assessment would have to be granted. However, Kestelman's commentary is actually far more than the usual quick and easy indictment of factor analysis. It was, in fact, one of the first commentaries to reveal the fundamental role in the indeterminacy debate of what is called, in the present work, the Central Account. Kestelman begins by noting the pervasive belief within psychometrics that, when the linear factor model describes a set of variates, "..no exact solution can be found: we can, it is said, only calculate what are called (perhaps a little loosely) "estimated values" for the factor-measurements; and these "estimated values" will not, after all, prove to be uncorrelated, as the initial statement of the problem required" (p.1). That is to say, according to the requirements specified by the orthogonal, multiple-factor model, a random vector $\underline{\boldsymbol{\theta}}$ that contains common factors to $\underline{\mathbf{X}}$ must have the property that $E(\underline{\boldsymbol{\theta\theta}}')=I$, while, on the other hand, the so-called "regression estimator" of these factors, $\Lambda'\Sigma^{-1}\underline{\mathbf{X}}$, has non-diagonal covariance matrix $\Lambda'\Sigma^{-1}\Lambda$. Kestleman illustrates the belief that "no exact solution can be found…" by quoting from the work of factor-analytic heavyweights: "The Hotelling components...can be calculated exactly from a man's scores...whereas Spearman or Thurstone factors can only be estimated" (Thomson, 1946, p.78); "Since the total number of factors, both common and unique, exceeds the number of variables, the value of any particular factor for a given individual cannot be obtained by direct solution, but can only be estimated from the observed values of the variables" (Holzinger & Harman, 1941, p.265); "We said that one advantage (of using factors) is that the factors are independent and uncorrelated. So they are if their true values are known. But we only know their estimates, and these estimates are correlated" (Thomson, 1946, p.118).

Kestelman observes, and this is the important point, that "Other writers, echoing these statements, even give the impression that some unattainable set of measurements, at once exact and unique, actually "exist"- the "true values", and that the data only enable one to make more or less satisfactory approximations to these pre-existent "true values"" (p.2). Kestelman's emphasis of the word *exist* makes it clear that he has detected a break from formal, mathematical senses of the term. What he has detected is, of course, what is called, in the current work, CA2 and CA5, each of these theses that are not implied by the mathematics of factor analysis. Kestelman's puzzled noting of this belief in pre-existent "true values" (whose "unattainability" would nowadays be described as resulting from their unobservability) correctly diagnoses the largely unacknowledged platonic metaphysics that permeates the practice of latent variable modeling. While apparently not aware of the depth of the problem, Kestelman struggles to describe one of the greatest barriers to scholarly work within the domain of latent variable modeling, to wit, that while the vast majority of published work is of a mathematical nature, the mathematics themselves are tacitly understood to be about that which is described by an ürbild, the Central Account.

L. Guttman (1955, *British J. of Statistical Psychology*)

This was, arguably, the most important paper ever written on the topic of indeterminacy. In it, Guttman derives construction formulas for the oblique, multiple common factor model, and proves the necessity and sufficiency of these formulas. He also invents an important method for the quantification of the indeterminacy inherent to a given factor analytic representation, the minimum correlation between alternative factors (see 4.2vc of the current work), and investigates conditions for determinacy as the number of variates becomes indefinitely large. Finally, he offers a clearly stated set of opinions in regard the implication of indeterminacy for the use of factor analysis as a tool in scientific research. Indeterminacy, according to Guttman, "...has important implications for the psychological meaning of common-factor analysis, as well as for the computing procedures conventionally used" (p.66). It is these opinions that are, at the moment, of interest. It should, at the outset, be emphasized that, unlike so many others, Guttman was clear in his portrayal of indeterminacy as a lack of uniqueness in the variates that can rightly be called factors to $\underline{\mathbf{X}}$, when $\underline{\mathbf{X}}$ is described by the model. Indeterminacy was not, as Spearman, Piaggio, and Thomson had suggested, merely a matter of measurement or prediction error. Naturally, then, Guttman was no fan of the nonsensical cliche that "factors can only be estimated, but not determined" (p.78).

A major contribution made by the paper was its reporting of the ρ^*s of a number of published factor analyses. This offered the practicing factor analyst an idea as to how bad the indeterminacy problem was in practice. Guttman's view was that the situation was indeed serious: "Godfrey Thomson reported that the ρ for nine 'primary traits' in Thurstone's original analysis range from .630 to .908, and remarked that: 'Those correlations do not look so bad' [15, p.339]. If we look at these ρ from the point of view of ρ^*, it seems that the sought-for traits are not very distinguishable from radically different possible alternative traits for the identical factor loadings" (p.74). And if the ρ^*s provided in factor analytic research of the social and behavioural sciences are typically low in value, then, in Guttman's opinion, the implications included the following:

a. The practice of "factor interpretation" is illogical. As Guttman notes, "It has been especially true with respect to common-factors that they have been *named* according to the content of the observed variables that have 'high' loadings on them..." (p.79). But if, as is the case, the same set of loadings is associated with all of the common factors to $\underline{\mathbf{X}}$ contained in *C*, and these common factors to $\underline{\mathbf{X}}$ are not, at the least, highly positively correlated, then it is self delusion to speak of interpreting *the* common factor to $\underline{\mathbf{X}}$.

b. The sought after property of factorial invariance is illogical. In Guttman's words, "Thurstone has further insisted that *"it is a fundamental criterion of a valid method of isolating primary abilities that the weights of the primary abilities for a test must remain invariant when it is moved from one test battery to another*" [16, p.361, italics his]. Here again is expressed the belief that A_n delimits some particular [common factor scores] with no reference to the ρ_n or ρ_n^*" (p.79). Here, Guttman diagnoses in Thurstone's writing the key CA belief that the concept *common factor to* $\underline{\boldsymbol{X}}$ signifies some single, particular class of constituents of natural reality. And, in his opinion, the indeterminacy property means that *no* single thing has been isolated when $\underline{\mathbf{X}}$ is described by the linear factor model. Hence, it makes no sense to believe that there is anything that can be invariant over batteries.

c. The practice of second-order factoring is suspect. As Guttman points out, second order factors will usually be more indeterminate than their first order counterparts.

d. The practice of factor rotation is pointless. In particular, Guttman questions whether undue importance has not been placed on the issue of factor rotation because the symbol for the common factor to $\underline{\mathbf{X}}$, to which the rotated loading matrix presumably refers, actually stands for a set of common factors to $\underline{\mathbf{X}}$, and this set is of infinite cardinality.

There can, however, be no question as to the importance Guttman places on the case in which a set of manifest variates described by the linear factor model is but a sample from a population of infinity of similar variates. He asserts that any proper item theory must be about such a universe of content: "In developing a psychological theory, any set of n observed variables to be factored will ordinarily be regarded as but a sample from an infinitely large universe of content" (p.76); "Neither N nor n should ordinarily be regarded as finite at the outset in a fundamental theory for mental tests, say, or for other substantive areas of factor analysis" (p.79). He observes that "though L and U^2 may be non-singular for all n, so that at least q+r factor scores are not perfectly predictable for any n...nevertheless *all* factor scores can possibly have their respective $\rho \rightarrow 1$ as $n \rightarrow \infty$" (p.75). Considering the limiting case in which the parameters of the model are a function of n, the number of manifest variates, he comments that "It seems safe to say that there has been considerable complacency about being able to ascribe particular scientific meaning to [the factor scores] on the basis of A_n, L_n, and $U^2{}_n$" (p.78); "If $\rho_n{}^*$ is low, it raises the question of what it is that is being estimated in the first place; instead of only *one* 'primary trait' there are many widely different variables associated with a given profile of loadings. If the scientific usefulness of factor analysis is to rest on its finding factor scores that can serve as meaningful reference axes, surely these reference axes should be defined by the analysis as being distinct from alternative axes" (p.79).

Guttman observes that, contrary to popular belief, "No matter how well an A_n satisfies a given criterion of rotation, and no matter how constant some of its rows may be from test battery to test battery, a particular set of meaningful reference axes is still not defined if ρ_n does not tend to unity for these axes" (p.79). That is, regardless of whether a given linear factor representation satisfies simple structure criteria, or whether invariance properties obtain, indeterminacy is problematic unless ρ^* is close to unity. He asks whether "...*in principle* a given universe of data admits of an approximately determinate set of common- and deviant-factors...", and replies that "The few instances of recorded ρ_n noted above are not very encouraging for the hypothesis of determinacy" (p.79). In other words, merely letting the number of manifest variates become large is *not* sufficient for determinacy in the limit, and the possibility of determinacy in the limit is not the same thing as obtained determinacy in the limit. Determinacy in the limit is an hypothesis that, in practice, seems rarely to be true. Summing up, he calls the Spearman-Thurstone factor theory a "neo-faculty" theory of psychology, and states that "Unless the factors or 'faculties' are pinned down, they can be used neither in theory nor in practice; hence, if determinacy is unobtainable from the observed universe of [the manifest variates], a correlation analysis of R_n by itself is not adequate for the Spearman-Thurstone type of theory...the Spearman-Thurstone approach may have to be discarded for lack of determinacy of its factor scores" (p.79).

Guttman (1957, *Psychometrika*)

This paper on the relation between the communality problem and multiple correlation, begins with Guttman claiming that "Solutions to the communality problem and of the problem of *meaning* of common and unique factors have been shown previously to depend intimately on certain relations with ordinary multiple correlation" (p.147). His emphasis on the word *meaning* indicates, once again, that unlike so many latent variable modelers, he understands that the existence of the CA does not solve the conceptual issues attendant to the concept *factor* or *latent variate*. In this paper, his aim is "To make these basic propositions more accessible" (p.147). They include the following:

i) Let there be a set of p manifest variates. If ρ_j is the population multiple correlation coefficient of the jth variate on the remaining (p-1), and if h^2_j is the communality of the jth variate, then $\rho_j^2 \leq h^2_j$ (j=1,2,...,p).

ii) Since, from the inequality of (i), $\frac{(1-h^2{}_j)}{(1-\rho_j^{\,2})} \leq 1$, (j=1,2,...,p), it follows that

$\frac{1}{p}\sum_{j=1}^{p}\frac{(1-h^2{}_j)}{(1-\rho_j^{\,2})} \geq 1 - \frac{m}{p}$, in which m is the number of common factors of the manifest variates, and p is the number of linear factor representable variates. It follows then that *if* the common factor model continues to describe the manifest variates as the ratio $\frac{m}{p}$ approaches zero, and this, it must be noted, is an unlikely happening, then it must be the case that, as $p\rightarrow\infty$, $\rho_j^2 \rightarrow h^2_j$.

iii) Let $\delta^2_j = h^2_j - \rho_j^2$. This is the variance of the difference between the jth unique factor scores and the errors of linear prediction of the jth manifest variate by the remaining (p-1) variates. If ρ_j^2 is close to h^2_j, then δ^2_j is close to zero, and the jth unique factor scores must be essentially equal to the jth anti-image scores from image analysis.

iv) The quantity $\frac{(1-h^2{}_j)}{(1-\rho_j^{\,2})}$ is equal to r_j^2, the squared multiple correlation of the jth unique factor on the p manifest variates. Hence, a large value of δ^2_j means that r_j^2 will tend to be small, and the jth unique factor will be poorly determined by the manifest variates. That is, the jth unique (and common) factor will be highly indeterminate.

v) "The wide-spread practice of trying to name or attach meaning to factors merely by studying factor *loadings* is clearly suspect if the same loadings can be derived equally well from radically different sets of factors *scores*" (p.149)

Heermann (1964, *Psychometrika*)

In this paper, Heermann presents an enlightening geometrical analysis of indeterminacy. Unfortunately, the paper also contains a number of contradictory conclusions in regard the nature of indeterminacy. Heermann begins by distinguishing between rotational and factor-score indeterminacy. Echoing Guttman's comments, he notes that there exists an erroneous belief that associated with each factor pattern is a unique set of factor scores (p.372), but then claims that a consequence of indeterminacy is the inability to "...calculate exact factor scores from the test measures" (p.372). He was perfectly well aware of the formulas of Guttman, Wilson, Piaggio, and Kestelman which, if employed, yield "exact factor scores." Here, and elsewhere in the paper, Heermann's efforts to describe indeterminacy are compromised by his apparently unintentional invokation of the Central Account. Later, he states that "...the most obvious consequence of indeterminacy is that it prevents us from developing unique factor measures; as a result, factor analysis does not seem to be very useful in describing the individual subject" (p.379). Fair enough. But he then concludes that it still may be useful for the study of covariance structures since "..description of the individual subject is not necessarily a major objective of factor analysis" (1964, p.379), supporting this conclusion with a strange and ambiguous quote from Thurstone: "The individual subjects are examined, not for the purpose of learning something about them individually, but rather for the purpose of discovering the underlying factors" (1947, p.325). This is self-contradictory because the indeterminacy property which "prevents us from developing unique factors" militates against the aim of "discovering the underlying factors". This is because: i) common factors to $\underline{\mathbf{X}}$, constructed in accord with (4.4) and (4.6), are not "underlying"; ii) the fact that the cardinality of set *C*, which contains common factors to $\underline{\mathbf{X}}$, is infinite implies, contra Thurstone, that the concept *common factor to* $\underline{\boldsymbol{X}}$ does not signify a single thing in need of discovery.

The paper continues in this vein, with Heermann acknowledging the mathematics of indeterminacy, and yet seemingly unable to resist the pull of the CA. He states, for example, that "Without doubt, the generality and scientific utility of the factor model would be enhanced if some meaigful method could be found to render factor scores determinate" (1964, p.380), and yet rejects component analysis as an alternative on the grounds that components "...are always contained in the test space, and cannot be expected to represent anything which goes beyond the original measures" (1964, p.380). But what does he believe *is* represented by linear factor analysis beyond the manifest variates? While one could reasonably argue that the symbol $\underline{\mathbf{X}}$ does "stand for" the phenomena represented by the manifest variates by virtue of the fact that the scores assumed by $\underline{\mathbf{X}}$ are assigned to the objects under study on the basis of rules, the symbol $\boldsymbol{\theta}$ is not linked by any such rule of score production to any natural phenomenon of interest. Specifically, it is not the case that the investigator *knows* of a phenomenon, signified by a concept "φ", prior to a factor analysis, and then employs the symbol $\boldsymbol{\theta}$ to represent it in the equations of the model. The symbol $\boldsymbol{\theta}$ is, rather, a place-holder for any random variate that satisfies the requirements laid down in the linear factor model for common factor-hood. Moreover, variates $\boldsymbol{\theta}_i=\underline{\Lambda}'\Sigma^{-1}\underline{\mathbf{X}}-w^{1/2}\mathbf{s}_i$ constructed so as to meet these requirements (i.e., those constructed in accord with (4.4) and (4.6)) contain only one source of "information" beyond the manifest variates. This additional information is carried by the $\mathbf{s}_i$ component, a random quantity that is arbitrary save for mild moment constraints. Heermann's claim seems to rest not on mathematics, but on his belief in the CA.

Heermann (1966, *Psychometrika*)

Heermann begins this effort by observing that "Over the past 30 years, a handful of investigators have noted and studied a perplexing property of the Thurstone-Spearman factor model: the indeterminacy of factor scores. In essence indeterminacy of factor scores means that infinitely many sets of scores can be constructed which satisfy all the properties of factor scores for a given factor matrix" (p.539). The objective of the current paper is to "derive a matrix expression for the limits of "rotational indeterminacy" from Ledermann's result..." (p.539). After achieving this objective, Heermann investigates the following question: "If factor axes are not uniquely positioned in their n+r space, can factors be uniquely related to external measures, i.e., measures which were not included in the analysis" (p.541). This is a topic later taken up by in a series of papers Steiger and Schonemann. Heermann's examination of this issue pertains to Dwyer extension analysis, and his conclusion is that "...in general, factor loadings for external variables are indeterminate, that is, these factor loadings can be represented by infinitely many values" (p.542). His recommendation: "This is not to say that we should abandon our efforts to relate factors to external measures, only that we should view the results of these efforts more realistically" (p.543). It is not clear why he resists abandoning a practice which he has just shown to be illogical. Nor does explain what a more "realistic" interpretation would look like.

Schonemann and Wang (1972, *Psychometrika*)

Schonemann and Wang begin by distinguishing between the "transformation approach" of Thomson and Ledermann, and the "construction approach" of Piaggio, Kestelman, and Guttman. Their stated aim is to "...employ the "construction approach" of Piaggio and Guttman to investigate the factor indeterminacy issue in the fallible case, i.e., when the model does not fit exactly" (p.62). They offer the following as a working definition of "factor scores": "...two sets of numbers which satisfy all the strictures of the factor model in the same sample once C, the covariance matrix estimate, satisfies them" (p.62). Guttman's minimum correlation measure is applied to a large number of published factor analyses, the conclusion being that many of these analyses yielded representations that were severely indeterminate. They comment that "If one dislikes score indeterminacies, one should probably look for other models which do not contain any, e.g., models which define the latent variables as linear combinations of the (possibly rescaled) observed variables ("Component analysis", which contains "Principal component analysis" as a special case). Once one embarks on the factor model, one has to live with this indeterminacy. To call another set of numbers which satisfies none of the strictures of the model (e.g., the so-called "regression estimates") "factor scores"...does not strike us as very rationale" (p.67).

Regarding statements to the effect that "factor scores cannot be computed directly, they can only be estimated", Schonemann and Wang state that "They evidently mean hardly anything as long as we are not told in clear and unambiguous terms what is meant by "factor scores" (as distinct from "factor score estimates"). Upon checking, one finds that the exact meaning of the term is a closely guarded secret. There are good reasons for not defining it: if by "factor scores" one means...observations on random variables, then "factor scores" cannot be *defined* uniquely for the simple reason that the underlying random variables, the "factors", cannot be defined uniquely...This, in turn, raises the question what, exactly, it is that is being estimated when "factor scores" are "estimated by the regression method [e.g., Lawley and Maxwell, 1963, p.89]" (p.88). It will be argued in Part II of the book that Schonemann and Wang's opposition is to the Central Account.

McDonald (1972, *Some uncertainties about factor indeterminacy*)

This reviewed, but unpublished, manuscript was the precursor to a 1974 Pychometrika article, and was perhaps the first effort fully committed to recasting indeterminacy as a trivial matter. McDonald begins by noting the recent publication of Schonemann (1971), and the impending publication of Schonemann and Wang (1972): "Regrettably, Schonemann's papers serve to show that the issue is not dead" (p.1). Because the indeterminacy issue had not yet been resolved, it is interesting that McDonald considers the publication of Schonemann's article to be regrettable. McDonald's intention in writing the paper can be discerned from the paper's abstract:

> It is argued that the position adopted by Guttman and by Schonemann and Wang is mistaken, and that the results on factor indeterminacy have no serious implications for the factor model. In other words:-
>
> The work of a number of "man(n)s"
> Isn't quite in accord with their plans
> The results that seem strong
> Are vacuous or wrong,
> Factor scoring is under no bans.

As this paper reveals, it is quite possible that McDonald is as creative in the generation of psychometric locutions as he is with limericks. After reviewing the recent conclusions of Guttman and Schonemann in regard factor indeterminacy, McDonald concludes that "If we apply Schonemann's argument to classical true score theory, we note that the value one-half serves as a cutting point on the scale of reliability coefficients...We might seem forced to say that a measure whose reliability is below one-half is not measuring anything!" (p.10). In his opinion, this constitutes "…a reductio ad absurdum of the factor indeterminacy issue" (p.10).

McDonald then turns to the laying down of a number of definitions:

Formal factors: In the notation of the current work, a set of random variates that satisfy equations (2.9)-(2.10) (McDonald, 1972, p.12). According to McDonald, this is a synonym of *true factor scores* (McDonald, 1972, p.13).

Kestelman factors: In terms of the current work, any set of random variates constructed according to (4.4)-(4.6).

Component: A linear combination of the set of manifest variates, say, $\underline{\mathbf{v}}=\underline{b}'\underline{\mathbf{X}}$. Traditional examples of factor-score estimators, e.g., the "regression estimator" and the "weighted least-square (Bartlett) estimator", are, by this definition, components (p.12).

These definitions are employed in the hope of drawing certain conclusions about the implications of the indeterminacy property. In Section 3, for example, McDonald (1972) considers the implications of indeterminacy for traditional factor-score estimation procedures. His concern is with the conclusions drawn by Guttman and Schonemann to the effect that "indeterminacy raises the question of what, exactly, is being estimated", and that the sentence "factor scores cannot be computed, but only estimated" is vacuous (pp.11-12). McDonald attempts to rebut these views: "…the covariance matrices of the components with formal common factors, and their regression curves on the formal common factors, are invariant under Ledermann transformations. This would seem to be all that is required to justify all practical uses of the traditional "estimates"; "…it does not follow, as implied by Schonemann and Wang's discussion, that the traditional "estimates" of factor scores cannot be used because of "doubt" as to what is estimated" (p.13).

McDonald's point here is simply that, if $\underline{\mathbf{X}}$ is ulcf representable, then the squared-correlation between any component variate, $\underline{b}'\underline{\mathbf{X}}$, and any common factor, $\boldsymbol{\theta}_i$, contained in *C* is equal to

$$\frac{C(\underline{b}'\underline{\mathbf{X}},\boldsymbol{\theta}_i)^2}{V(\underline{b}'\underline{\mathbf{X}})V(\boldsymbol{\theta}_i)}=\frac{C(\underline{b}'\underline{\mathbf{X}},\underline{\Lambda}'\Sigma^{-1}\underline{\mathbf{X}}+w^{\frac{1}{2}}\mathbf{s}_i)^2}{\underline{b}'\Sigma\underline{b}}=\frac{(\underline{b}'\underline{\Lambda})^2}{\underline{b}'\Sigma\underline{b}},$$

while the regression of $\underline{b}'\underline{\mathbf{X}}$ on any $\boldsymbol{\theta}_i$ contained in set *C* is equal to

$$\hat{\boldsymbol{\theta}}=E(\underline{b}'\underline{\mathbf{X}}|\boldsymbol{\theta}_i=\theta_i)=\underline{\Lambda}'\underline{b}\theta_i.$$

But it is difficult to see any force in this observation, for if there did not exist these invariance properties, there would be no indeterminacy property. It is precisely because, when $\underline{\mathbf{X}}$ is ulcf representable, there exists a *set C* of infinite cardinality, whose elements each have precisely the joint moments with $\underline{\mathbf{X}}$ required of a common factor to $\underline{\mathbf{X}}$, that the linear factor model is indeterminate. On the other hand, it is true that there is nothing inherently wrong with the various "factor estimators" that have been offered, as long as they are viewed (properly) as predictors of each of the elements of *C*. There are really two issues that must be addressed in regard the notion of "factor score estimation". First, this notion is understood in terms of the CA tenet that what is to be estimated are scores with respect a single unique property/attribute (cause) of the phenomena represented by the $\mathbf{X}_j$. Whether this is a tenable belief is open to question. Second, it is of questionable reasoning to suggest that a common-factor predictor is *needed*, when realizations on the common factors to $\underline{\mathbf{X}}$, i.e., the $\boldsymbol{\theta}_i$, can be taken. But to acknowledge the latter fact is to do irreparable damage to the Central Account, for one is then, in effect, admitting that common factors to $\underline{\mathbf{X}}$ are constructions. To put this another way, to insist upon the need for factor score *estimation* is to assert the Central Account.

At this point in the proceedings, McDonald introduces another pair of definitions, these designed to distinguish between sub-types of Kestelman factors:

Nontrivial Kestelman factors: These are factors constructed in accord with (4.4)-(4.6), for which

$$E(\boldsymbol{\theta}\mathbf{s}_i)=0 \text{ and } E(\mathbf{s}_i|\boldsymbol{\theta}=\theta_o)=0\ \forall\ \theta_o.$$

A paraphrase of McDonald's rationale for this definition is as follows: i) The common factor, $\boldsymbol{\theta}$, is unobservable, so that one cannot take realizations on it; ii) In order to generate Kestelman factors, one randomly draws a number from some population of numbers (i.e., takes a realization on $\mathbf{s}_i$) and arbitrarily associates it with a realization $\underline{x}$ of $\underline{\mathbf{X}}$ (i.e., for a particular individual drawn); iii) One cannot choose $\mathbf{s}_i$ in such a way that $E(\boldsymbol{\theta}\mathbf{s}_i)$ and $E(\mathbf{s}_i|\boldsymbol{\theta}=\theta_o)$ are particular non-null values. In fact, "We can only draw them from a space of statistically independent random variables. In effect, $\mathbf{s}_i$ is a random variate on a distinct sample space, which is put into correspondence with the sample space of $\underline{\mathbf{X}}$ only by arbitrary association on the part of the investigator" (McDonald, 1972, p.15). This is how one achieves the two properties, $E(\boldsymbol{\theta} \mathbf{s}_i)=0$ and $E(\mathbf{s}_i|\boldsymbol{\theta}=\theta_o)=0 \ \forall \ \theta_o$, of "Nontrivial Kestelman factors".

Trivial Kestelman factors: These are factors constructed according to (4.4)-(4.6), but for the case in which realizations on $\boldsymbol{\theta}$ *can* be taken, so that the common factor, as well as $\underline{\mathbf{X}}$, is "known."

In the case of a trivial Kestelman factor, $\mathbf{s}_i$ would then be given as $\mathbf{s}_i=w^{1/2}\underline{\mathbf{X}}-w^{-1/2}\underline{\Lambda}'\Sigma^{-1}(\underline{\mathbf{X}}-\underline{\Lambda}\boldsymbol{\theta})$, from which it follows that

$$E(\boldsymbol{\theta}_i \mathbf{s}_i)=w^{1/2} \text{ and } E(\mathbf{s}_i|\boldsymbol{\theta}_i=\theta_o)=w^{1/2}\theta_o \ \forall \ \theta_o$$

But obviously, there would then be no need to generate Kestelman factors of any type, because, under this scenario, $\boldsymbol{\theta}$ is known! But, in fact, $\boldsymbol{\theta}$ is "unobservable", and, hence, "unknown", and so it follows that trivial Kestelman factors cannot be constructed.

On the basis of these definitions, McDonald deduces the following: "If the formal factors in a population are unknown, they are not included among the (non-trivial) Kestelman factors that we can actually construct. If the formal factors were known, we could then, but only trivially, represent them as Kestelman factors" (McDonald, 1972, p.15). After a little correlation algebra, McDonald seems to establish that the correlation between the "unknown formal factor" and each "non-trivial Kestelman factor" is equal to $\underline{\Lambda}'\Sigma^{-1}\underline{\Lambda}=\frac{(\lambda_{*1}-1)}{\lambda_{*1}}$, while the correlation of the former with the "regression estimator" is equal to $\frac{(\lambda_{*1}-1)^{\frac{1}{2}}}{\lambda_{*1}^{\frac{1}{2}}}$. It then appears that McDonald has proven that the "formal factor" is better predicted by the regression estimator than by a non-trivial Kestelman factor: "This is, after all, not surprising. If we add arbitrariness to ignorance we should get an increment of error, not of knowledge." (McDonald, 1972, p.16). Furthermore, "...it is necessary to emphasize that the unknown formal factors...can indeed be expressed as Kestelman factors, but it is only possible to so express them through the tautology to which [the constructions] reduce when we choose $\underline{\mathbf{s}}$ as a function of given $\boldsymbol{\xi}$, as opposed to generating $\underline{\mathbf{s}}$ as an independent random vector. To the writer, failure to recognize just this point is the fundamental error that underlies the position adopted by Guttman and his followers on factor indeterminacy." (p.17)

To begin, the definition McDonald offers for his concept *non-trivial Kestelman factor* seems to rest on an error. In particular, since variates signified by the concept *common factors to* $\underline{\boldsymbol{X}}$ are expressible as $\boldsymbol{\theta}_i=\underline{\Lambda}'\Sigma^{-1}\underline{\mathbf{X}}+w^{1/2}\mathbf{s}_i$ (Theorem 1, Chapter IV), and $E\mathbf{s}_i^2=1$, it follows that $E(\boldsymbol{\theta}_i\mathbf{s}_i)=w^{1/2}$ which contradicts McDonald's requirement of "non-trivial Kestelman factors" that $E(\boldsymbol{\theta}\mathbf{s}_i)=0$. Whatever be a non-trivial Kestelman factor it cannot be a sub-type of the factors refered to in the equations of the linear factor model. However, this point is of secondary concern. What is important, here, is to observe McDonald's exertions in attempting to deny that common factors to $\underline{\mathbf{X}}$ are constructed random variates. To do this: i) He retains a special symbol ($\underline{\boldsymbol{\xi}}$ in his notation) for what he calls a "formal factor", a single, "true", unobservable entity signified by a concept whose definition rests in some mysterious way on ingredients lying beyond the mathematical restrictions imposed by the model; ii) He assigns to the "Kestelman factors", i.e., the variates constructed in accord with the model specified mathematical requirements that are constitutive for common factor-hood, a distinct symbol (**x** in his notation); iii) At no point in his writing does he call the latter simply common factors to **X**, even though they are definitionally so. In McDonald's view, the "Kestelman factors" are not *real* common factors to **X**, apparently because the common factor model detects unknown, unobservable sources, and the Kestelman factors are neither unknown, nor unobservable (recall: "If the formal factors in a population are unknown, they are not included among the (non-trivial) Kestelman factors that we can actually construct").

With this definitional manoeuvering, McDonald is able to acknowledge the mathematical existence of the constructions, but protect the essential ingredients of the Central Account, i.e., that employment of the factor model allows for the detection of *something else*, namely an unobservable property/attribute (cause) of the phenomena represented by **X**. But what, then, are these unobservable "something elses" that are not constructed in accord with the mathematical restrictions imposed by the model? By way of explanation, McDonald offers up a significant portion of the causal picture of the Central Account: "To take the common factor model seriously is to suppose that in some sense, to some degree of approximation, it serves to describe a portion of the real world...it may be suggested that among the formal factors that in principle are "always fully known", there is one set which actually gave rise to the data in hand. That specific set is not, however, included among those Kestelman factors that have been defined above to be non-trivial. According to the above, we can better approximate the set which gave rise to the data, by components than by non-trivial Kestelman factors. On the other hand, it would be an outright contradiction to declare that more than one set of formal factors, or that all possible sets, gave rise to the one set of data in hand, and that all of these may be computed with the use of random numbers generated by the investigator" (p.18). So here one sees emerge the core of the CA, i.e., the view that when a set of variates is described by the linear factor model, the model describes a state of affairs, to wit, the relationship between the phenomena represented by the manifest variates and their detected causal source, the latter represented in the model by the symbol $\boldsymbol{\theta}$.

McDonald finds himself having to insist that the comments of Guttman and Schonemann are trivial because, regardless of the very definition of *common factor to **X*** offered by the model (recall Theorem 1, Chapter IV), he is committed to an ürbild, the CA, in which the concept *common factor to **X*** designates a single, unobservable causal source of the phenomena represented by the manifest variates. The constructions, then, are clearly beside the point, for they have no place in the CA: "If, in the first place, we are willing to regard the factor model as describing an aspect of the real world whereby unobserved processes give rise to "observable" random variables, it is then a contradiction to suppose that those processes are not unique. It is another contradition to suppose that they are known. It is a third contradiction to suppose that they are generated in many ways as non-trivial Kestelman factors, which are functions in random numbers that have nothing to do with the portion of the world being studied. In short, most accounts of the fundamental factor model use the non-mathematical qualifier "unobservable" to describe the common factor scores. It is not yet proven that it is philosophically naive to do this." (McDonald, 1972, p.18)

McDonald will come to embrace an "abstractive property" portrayal of the referent of *latent variate to **X***, and, in later publications, will attempt to distance himself from the stance he adopted in this 1972 paper: "There is no ground for Dr. Maraun's claim that McDonald (1974) was seduced by a metaphor, or conflated concepts....(I agree that others have, for example, conflated common factors and common causes. There is no instance where I have done such a thing." (McDonald, 1996a, p.595); "...in early drafts of the 1974 article [*Some uncertainties*...] I pressed the argument that the behavior domain determined the "one" factor in question, thereby defending Guttman against Guttman, as well as Schonemann" (McDonald, 1996a, p.594). It will be argued, however, that McDonald's 1974 article, and the later "abstractive property" conception, are not the grand departures McDonald has portrayed them to be. In particular, these later positions *do* rest on an array of conflations closely related to those present in his 1972 statement of the CAC, and are simply more subtle assertions of the Central Account. McDonald 1974, for example, attempts to defend the key thesis that "*common factor to* ***X*** cannot designate a set of constructed random variates because it designates a single, unique constituent of natural reality", on the grounds that this thesis is a consequence of the probability foundations of the factor model. As will be seen, Bartholomew (1981) attempts a similar strategy to protect the Central Account. And, while the final sections of McDonald (1972) do, indeed, contain certain arguments pertaining to the behaviour domain idea, they constitute a decidedly minor component in the support McDonald offers for his claim that "The fact that two alternative formal factors...are possibly orthogonal has merely trivial implications" (pp.23-24). In truth, McDonald's many accounts of latent variable modeling contain many and varied lines of defense of the CA, and not all of these lines of defence are even reconcilable.

McDonald (1974, *Psychometrika*)

The stated aim of this paper is "...to show that common factors are not subject to indeterminacy to the extent that has been claimed, because the measure of indeterminacy that has been adopted is ill-founded" (p.203). Its publication, and the earlier acceptance of McDonald (1972) for publication, were controversial, and resulted in an inquiry into the role played by the editor of Psychometrika at that time (see Schonemann, 1978). The paper itself is truly fascinating, one of the first published attempts to put down the "indeterminacy uprising" led by Schonemann, Steiger, and Guttman. The paper opens with sections that review the common factor model, factor score estimation, and the mathematics of indeterminacy. There then follows a section on the relation between the construction and transformation approaches to the construction of factors to **X**, in which are derived necessary and sufficient conditions for two distinct factors to **X** (i.e., random vectors constructed according to (4.4)-(4.6)) to be linearly related. However, things become genuinely interesting in section five of the paper, *The meaning and measurement of factor indeterminacy*.

In this section, McDonald begins by stating that "No one denies that factors are indeterminate. Surely no one could. Given the factor pattern and unique loadings for a population, and the observed scores of an individual or a group of any size from that population, clearly we cannot compute the common factor scores of the individual or group as a one-valued function of the observed scores. However, disagreement is possible over the meaning of such indeterminacy and over the assessment of its extent" (p.213). His disagreement can be seen as organized in terms of three arguments.

Argument 1

McDonald's first argument is that Guttman's minimum correlation measure is an inappropriate measure of the indeterminacy inherent to a particular representation. He claims that the majority of psychometricians prior to Guttman employed the coefficient of nondetermination ($1\text{-}R_{\theta.\underline{X}}^2$), or variants thereof, to quantify the indeterminacy inherent to a factor representation. He suggests that Camp (1932) paints an unreasonably bleak picture with his observation that if the average correlation of a set of tests is equal to .25, it would take 297 such tests to yield a coefficient of non-determination of .01: "Our concern is diminished when we note that Camp has set a standard for factor validity to which very few social scientists would aspire" (McDonald, 1974, p.214). According to McDonald, "Guttman [1955] departed from the precedent set by the early work...introducing the minimum correlation between alternative factors as a measure of the extent of determinacy. He gave no rationale for this departure" (p.214). Furthermore, "At least the same degree of plausibility pertains to the use of the squared multiple correlation of a factor with the observations, or the equivalent measures used by Spearman, Holzinger, Camp, Piaggio, and Thomson. The commonsense argument would be that we ordinarily measure statistical determinacy in these ways so it is natural to do so in this context" (p.215).

This first argument is then simply a restatement of Spearman's "error of prediction" argument. McDonald follows Spearman in blurring the distinction between error of prediction and indeterminacy by describing the former as an issue of "statistical determinacy". He scoffs at Camp's recomendation as if Camp were describing a standard instance of prediction, in which case it would indeed have been unreasonable to insist upon a co-efficient of nondetermination of .01. However, the indeterminacy property of the linear factor model is an entirely different kind of issue, there being no criterion variate, scores on which can be produced prior to analysis. Instead, what constitutes a common factor to $\underline{\mathbf{X}}$ is specified by the model itself. And it so happens that, according to this specification, when $\underline{\mathbf{X}}$ is described by the linear factor model, the set *C* of common factors to $\underline{\mathbf{X}}$ contains an infinity of constructed random variates (each, by definition, a common factor to $\underline{\mathbf{X}}$). This is why a very small coefficient of non-determination, hence, a very large value of Guttman's ρ^*, is essential. Without such a value, the common factors to $\underline{\mathbf{X}}$ can have widely varying empirical properties (e.g., correlations with external variates). As did Spearman, McDonald portrays indeterminacy as an issue of prediction, because this interpretation squares with the CA, and McDonald is committed to the CA.

Argument 2

McDonald, however, is not satisfied with a simple appeal to past figures of authority. He states that "To settle this question requires a deeper analysis of the concept of unobservable random variables. It turns out that it is necessary to consider the very foundations of the model, and that great care is needed if we are to describe these foundations without contradictions" (p.215). The basics, with Feller (1957) referenced, are presented: i) The sample space of the common fator model is usually a population of subjects; ii) a scalar random variate is a scalar function defined on the sample space. It is a rule that associates a number with every sample point; iii) If a set of random variates are defined on the same sample space, they have a joint distribution. He then presents a definition of *observable (unobservable) random variate*:

> Let $\boldsymbol{\tau}$ be a vector defined on a given outcome space(sample space). Then $\boldsymbol{\tau}$ is *observable* if (a) we can know the value $\boldsymbol{\tau}=\mathbf{t}$ of any observation drawn from the outcome space, or, (b) we have a rule for drawing an observation at random from a subspace of the outcome space in which $\boldsymbol{\tau}=\mathbf{t}$, where t is any fixed value, known

or not, in the domain of $\boldsymbol{\tau}$. It is unobservable only if it not observable, as defined (p.216).

According to this definition, a treatment mean is observable because the experimenter has knowledge of the rule that randomly assigns experimental units to that treatment. A true score is unobservable because a researcher cannot replicate observations and so cannot obtain a random variate whose mean is the true score. As McDonald notes, "On this convention, a true value is observable if we can replicate, as in some physical measurement, and unobservable if we cannot, as in most psychological measurement" (p.216). What then is being asserted when it is said that common and unique factors are unobservable? According to McDonald, merely that "...we do not have a rule enabling us to draw a random sample of subjects that have possibly different values of...the "observed" scores, but have the just one fixed value of [the common factor]" (p.216).

As is evident in the following quotes, McDonald attempts to deduce from the preceeding a special "uniqueness" property of unobservable common factors (symbolized in his notation as ξ). He states: "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. The contradiction would be plain. The same contradiction is contained in any attempt to say that different values of ξ can be associated ("at the same time", is understood thoughout this discussion) with one subject. Disguised forms of this contradiction, much less glaring, may be hidden in the looser expressions one might be tempted to employ. Thus, if we say that different factor scores are "equally correct", the question is, what can this mean? We cannot say that a given subject, with given $\boldsymbol{\eta}$, has more than one value of ξ" (p.217); "By definition an individual's common factor scores are unique, in the sense that he is not at more than one point in the outcome space of elementary events on which the specification of the model ultimately rests. Although his common factor scores are unique in this sense, they are not determined by his observable scores. We estimate them because of this fact, not in spite of it" (p.217); "...statements from Guttman...and echoed by Schonemann and Wang, might be taken to imply that factors are, in some very special sense, not uniquely defined. It is made to seem that we cannot estimate them because of "the question of what it is that is being estimated in the first place." The only special sense that seems available is the one we here reject as inconsistent with the foundations of mathematical statistics, namely that the factor scores of individuals are not unique, i.e., that the points of the outcome space are not points" (p.217); "It is not here being denied that given $\boldsymbol{\eta}$ we can contemplate random variables ξ^+ and ξ^- that are possible solutions of the equations of the model, i.e., possible values of the unobservable common factor scores coordinate ξ, and that hence we can compute the correlation between these possible solutions. Certainly ξ^+ and ξ^- are defined as functions of $\boldsymbol{\eta}$ and $\boldsymbol{s}$, hence they are two random variables defined on the same sample space. But if they are regarded as alternative solutions of one system of equations, i.e., as possible distinct values of one variable ξ, then the only prediction of one of these from the other, as indicated above, is the logical disjunction of mutually exclusive events. If an individual is at ξ^+, he is not at ξ^-. That is all there is to it" (p.218); "In effect, then, the correlation between extreme alternatives combines the acceptable notion of non-unique solutions of equations, with the acceptable notion of statistical prediction, into an unacceptable measure, the correlation between logically disjoint solutions of those equations" (p.218).

Guttman (1975) (see later) has pointed out that this line of argument confuses the concepts of *sample space* and *range* of a random variate. But, more essentially, McDonald tries to say that because there is a symbol ξ which stands for common factors to $\underline{\mathbf{X}}$ in the equations of the linear factor model, it must be the case, *according to probability theory*, that an individual possesses only one score with respect the common factor to $\underline{\mathbf{X}}$, and this score, because the common factor is unobservable, is unknown. In fact, nothing of the sort follows from probability theory. In particular, there is nothing in probability theory that settles the reference of a symbol occurring in the equations of a statistical model. Probability theory provides a calculus for the description of the behaviour of measureable functions (random variates), but it can say nothing about what is signified by the concept *common factor to* $\underline{\boldsymbol{X}}$, nor the relationship between this concept and the symbol ξ, nor the meaning of the scores that comprise the distribution of this random variate. Because neither ordinary language, nor the technical language of psychology, provides a criterion of employment for the technical concept *common factor to* $\underline{\boldsymbol{X}}$, it is the equations and distributional specifications of the linear factor model itself that settles the issue. From Theorems 1 and 2 of Chapter IV of the current work, it so happens that ξ stands for each of the members of set *C*, because *C* contains random variates constructed according to (4.4)-(4.6), and (4.4)-(4.6) is a recipe for the construction of random variates which satisfy all of the requirements constitutive of common factor-hood. Hence, when $\underline{\mathbf{X}}$ is ulcf representable, an infinity of functions (random variates), each a common factor to $\underline{\mathbf{X}}$, is defined on the sample space. These being distinct functions, one is perfectly free to compute the correlation between any pair, and, as it turns out, these correlations will not, for finite p, be equal to unity. When McDonald states that "...we can contemplate random variables ξ^+ and ξ^- that are...possible *values* [italics added] of the unobservable common factor scores coordinate ξ...", he is confusing the situation of distinct realizations of a single random variate with that of distinct random variates, in this case ξ^+ *and* ξ^-. The symbol ξ stands for the set of common factors contained in *C*, among them, pairs with minimum correlation ρ^*. McDonald conflates these ideas, purposefully or not, because he refuses to betray that which is essential to the Central Account, namely that concept *common factor to* $\underline{\boldsymbol{X}}$ refers to a single, unobservable property/attribute (causal source) "existing" in nature.

Argument 3

The third argument offered by McDonald, beginning on p.218, is, essentially, a re-packaging of the "formal factors" line of reasoning from his 1972 paper. It runs as follows:

i) The constructed factors are observable while the common factors to $\underline{\mathbf{X}}$ are unobservable. In McDonald's words, "Given an observable random vector $\boldsymbol{\eta}$, an investigator may choose an arbitrary random vector **s** by the same sampling rule that he has for $\boldsymbol{\eta}$, and hence obtain a possible solution **x**, **z**...That is, each time he draws an "examinee" at random and measures $\boldsymbol{\eta}$, he draws an **s** at random by means of some random device *and assigns it* to *that* "examinee". Such a possible solution is *observable*, since the investigator has a rule for drawing **s**, and so the solution computed is a function of observable random variables $\boldsymbol{\eta}$ and **s**...On the other hand, we say that the common factor vector ξ in the model is unobservable..." (p.219).

ii) Within the set of variates constructed as per (4.4)-(4.6) lies a variate "corresponding to" the real, unobservable common factor. In McDonald's words, "In principle, by the same argument, we can imagine the investigator arbitrarily choosing indefinitely many random vectors **s**, until all possible solutions have been generated, so that , in a sense, the unobservable factor vector $\boldsymbol{\xi}$ must be somewhere among all the observable vectors **x**. But because we lack a sampling rule for it, we do not know which one it is" (p.220).

iii) The correlation between the "regression estimator" and the unobservable common factor is larger than that between any observable (constructed) common factor to $\underline{\mathbf{X}}$ and *the* unobservable common factor. In McDonald's words: "...If we attempt to construct an observable counterpart of a factor, the correlation between this and the factor is equal to the square of the multiple correlation of the factor on the observed variables. Such an observable counterpart would not be as good as the regression estimate in determining a factor" (pp.220-221)

iv) Therefore, "...we repeat the conclusion reached earlier that it is proper to express factor indeterminacy in terms of the irreducible uncertainty as to the (unique but unobservable) location of an individual in common factor space." (p.220).

In (i), McDonald attempts here to establish cleavage between the factors constructed as per (4.4) and (4.6) (these definitionally factors to $\underline{\mathbf{X}}$) and an unobservable property/attribute (causal source) of the manifest variates whose existence is asserted in the CA. He does this by providing a definition of *unobservability*, and then simply asserting that the linear factor model is *about* a single unobservable common factor to $\underline{\mathbf{X}}$. Since the CA is the received account of latent variable modeling, few of his readers would have questioned how the linear factor model, a set of equations and a few distributional specifications, could step beyond the definition *it* provides of *common factor to* $\underline{\boldsymbol{X}}$, and access mysterious unobservable entities. Regardless, McDonald supports this manoeuver by avoiding calling the variates constructed as per (4.4)-(4.6) common factors to $\underline{\mathbf{X}}$, and by symbolizing the constructed common factors as **x**, while reserving the symbol ξ, which appears in the model equations, to stand for the unobservable common factor refered to in the Central Account.

McDonald cannot deny the mathematics of indeterminacy, notably the fact that the construction formula (4.4)-(4.6) yields random variates with properties that are necessary and sufficient for common-factor hood. However, the CA claims that the factor model detects an "existing" property/attribute (causal source), and, as he argues elsewhere, a constructed random variate cannot be any such thing. Hence, in (ii) he tacitly asserts that, *in addition* to the constructed common factors to $\underline{\mathbf{X}}$, there is still a single existing, but unobservable, referent of *common factor to* $\underline{\boldsymbol{X}}$. He maintains this illusion in (iii) by calling the variates constructed according to (4.4)-(4.6) "observable counterparts" of a factor, he, once again, attempts to deny that these constructed random variates, which are, *by definition*, factors to $\underline{\mathbf{X}}$, are factors to $\underline{\mathbf{X}}$. Conclusion (iv) does not follow from anything inherent to probability theory, and, in fact, is in direct conflict with the definition of *common factor to* $\underline{\boldsymbol{X}}$ provided by the model (under which $\underline{\mathbf{X}}$ has an infinity of common factors if it has any at all). McDonald's argument is based on little in the way of mathematics or statistics, but rather on his tacit commitment to the CA.

McDonald (1975, *Psychometrika*)

McDonald begins this effort by noting that, while there are in existence satisfactory methods for the estimation of the parameters of the linear factor model, there has arisen interest in image theory as a possible corrective for "...alleged theoretical defects of the common factor model" (p.137). He observes that "...factor analysis is unfavourably contrasted with image theory in respect of at least two main properties. Firstly, it is asserted that common factors are indeterminate, whereas image scores are entirely determinate. A variant on this statement is that in common factor analysis the common part of a variable lacks clear definition, while in image analysis it is clearly defined" (pp. 137-138). McDonald then sets off to develop a comparison between factor and image analysis. He does this by developing a set of axioms, and deducing the axiom set on which each "theory" is based. An axiomatic characterization of component theory appears in an Appendix.

This paper is fascinating because, in it, McDonald actually arrives at the water, but simply refuses to drink. He describes linear factor theory, image theory, and component theory as each based on a decomposition of the form $\underline{\mathbf{z}}=\underline{\mathbf{v}}+\underline{\mathbf{w}}$, in which $\underline{\mathbf{v}}$ is called by him the "main part" and $\underline{\mathbf{w}}$ the "deviate part". His axiomatic account then very nicely illustrates that there is no sense in speaking as if one or another of these "theories" is, in absolute terms, "better" or "worse" than the others. Common factors, partial images, and components, the $\underline{\mathbf{v}}$'s in the model equations, simply answer to different optimality requirements, these requirements spelled out in terms of McDonald's axiom sets. In particular, McDonald shows that in the case of decomposition $\underline{\mathbf{z}}=\underline{\mathbf{c}}+\underline{\mathbf{e}}$, if one insists that $E(\underline{\mathbf{ce}}')$ be a null matrix, $E(\underline{\mathbf{ze}}')$ be a diagonal matrix, and $E(\underline{\mathbf{cc}}')$ be of rank $r<n$, then one cannot have it that $\underline{\mathbf{c}}$ is a linear transformation of $\underline{\mathbf{z}}$. That is, the fact that the common and unique factors of a ulcf representation can't be expressed as linear transformations of the manifest variates is a cost of the requirements unambiguously imposed by the model on any variate that can rightly be called a factor to $\underline{\mathbf{z}}$.

However, rather than take the logical final step and admit that his axiomatic statement of the linear factor model is "construction talk", i.e., that it spells out a recipe for the construction of random variates that are factors to a $\underline{\mathbf{z}}$ that is described by the model, and that the recipe is nothing other than construction formula (4.4)-(4.6), McDonald sacrifices logic for the protection of the Central Account. He does this by next equating the indeterminacy property of ulcf representations, i.e., the fact that the cardinality of C is infinite, with the alleged unobservability of the factors to $\underline{\mathbf{X}}$: "By Theorem 2...$\mathbf{e}$ cannot be expressed as a linear transformation of $\mathbf{z}$, hence, of course, $\mathbf{c}$ cannot be so expressed either. This means that given $\mathbf{z}$, the parts $\mathbf{c}$ and $\mathbf{e}$ are jointly indeterminate (under linear theory). (It is traditional to describe them as "unobservable")" (1975, p.141). The taking of the indeterminacy property as equivalent to unobservabilty is a new wrinkle to McDonald's writing on the subject, and certainly does not square with the definition of unobservability he provided in his 1974 paper (or any of his other attempts to render trivial indeterminacy and its implications). However, it might have been enough to satisfy the incurious reader as to why McDonald does not provide construction formulas for $\underline{\mathbf{c}}$ and $\underline{\mathbf{e}}$, but does provide such formulas for partial images and principal components. Regardless, the property of unobservability has no bearing on whether one *variate* is a linear transformation of a set of *variates*. Common factors are not linear tranformations of manifest variates because no there exists no such transformation that satisfies the requirements imposed by the model of common factor-hood. If there existed a transformation that did satisfy these requirements then common factors would be tranformations, and McDonald would have discussed this fact in detail.

Imagine that it *were* the case that, given the axiom set of linear factor analysis, $\underline{\mathbf{c}}$ *could* be expressed as a linear transformation of $\underline{\mathbf{z}}$. McDonald would then undoubtedly have provided the construction formula for $\underline{\mathbf{c}}$, and, moreover, would, as he did for components and images, have taken this formula as *settling* the issue as to what the concept *common factor to* $\underline{\mathbf{z}}$ means. Why then does the mere fact that $\underline{\mathbf{c}}$ is *not* expressible as a linear transformation of $\underline{\mathbf{z}}$ lead him to conclude that the well-known construction formula for $\underline{\mathbf{c}}$ (i.e., (4.4)-(4.6)) no longer settles the issue as to what is meant by *common factor to* $\underline{\mathbf{z}}$? Why does he believe that unobservability must be brought into play to explain why random vectors which satisfy the ulcf axioms are not linear transformations of the manifest variates? Because McDonald professes to have provided a *complete* axiomatic characterization of factor theory, and employs an unobservability property of $\underline{\mathbf{c}}$ and $\underline{\mathbf{e}}$ to exclude from factor-hood constructions yielded by (4.4)-(4.6) (despite the fact that these constructions answer to the axioms *he* claims defines the linear factor model), why then does he not list unobservability as a defining axiom of the factor model? There is no logic to the choices McDonald makes in regard these issues. What is at work here is the standard adherence to the Central Account as the unwritten law to which his whole effort must square.

Green (1976, *Psychometrika*)

Green begins by opining that, in regard the indeterminacy issue, "The facts are somewhat obscured, sandwiched between elegant mathematics and argumentative long-winded conclusions" (p.263). His note "...attempts to clarify and explain the main facts and offers a moderate conclusion" (p.263). Importantly, the paper is coloured by various commitments to the Central Account. He begins, for example, by stating the falsehood that "...the factor scores cannot be obtained" (p.263), even though he was evidently well aware of formulas (4.4)-(4.6) by which factors to $\underline{\mathbf{X}}$ can be produced. According to Green, components are linear composites of "real variables", while common and unique factors are hypothetical. He does not explain what he means by "real", nor "hypothetical", nor, given that they can be produced by (4.4)-(4.6), how factors can be said to be hypothetical in any sense. He claims wrongly that "Recent computer packages, notably SPSS...will produce factor scores on request" (p.263). In fact, computer packages produce so-called "factor score estimates", which are, in reality, various types of predictors. He correctly characterizes McDonald (1974) as arguing that "...by doing a factor analysis, we are accepting the common factor model and thus implicitly positing that one of the plethora is really the one true score vector, thus putting us into the standard regression situation" (p.264). He calls this view "...unconvincing and unnecessary" (p. 264). He claims that "...the vector of factor score estimates is estimating the entire inifinite set of possible factor score vectors" (p.264). It is not clear why he calls these constructed random variates "possible factors", when, later in the paper, he states that "It is unnecessary to assume that one of the infinite set of admissible factor scores is true, as all are estimated equally well by the factor score estimates" (p.265). He claims that the squared multiple correlation of the manifest variates with the common factor is "...the appropriate measure of factor score indeterminacy", and that Guttman's minimum correlation "...while true, is grossly unrepresentative" (p.264). He does not explain what he means by "unrepresentative". According to Green, "A better index is the expected correlation between two different score vectors" (p.264), i.e., $E(\rho(\boldsymbol{\theta}_i,\boldsymbol{\theta}_j))$ defined over *C*, which, he claims correctly, but without proof, is equal to the squared multiple correlation between the manifest variates and the common factor.

Guttman (1975, Unpublished, submitted for publication to *Psychometrika*)

In this paper, "The Green-McDonald Proof of the Nonexistence of Factor Analysis", Guttman provides a tongue-in cheek indictment of McDonald (1974). The aim of the paper is to show that the core arguments of McDonald (1974) are flawed. In the abstract, he explains that "Green and McDonald have pointed out that the underdeterminacy of factor scores has "been thought to have disturbing consequences for the common factor model". To help quell disturbing thoughts in this regard, they have invoked three logical principles which in effect prove that factor analysis does not exist". In a bibliographical note, he explains that "The editor of Psychometrika was an active collaborator in the development of the published G-M arguments, so it would be amiss not to add his name to these contributions, as is done in the present paper" (p.12).[2]

Guttman begins by noting that "Factor analysis has had to cope with at least three varieties of indeterminacy: communalities, rotations, and factor scores. By indeterminacy is meant that there is more than one solution to the factor problem as originally stated. Traditionally, removal of indeterminacy has been attempted by suggesting further limitations on the problem which pick out a unique solution. In practice, until now, suggestions have been put forward towards resolving the predicament only for the first two varieties: communalities and rotations. For some hitherto unknown reason, the scientific literature has not yet suggested a concrete way of delimiting a unique solution to the third variety of indeterminacy, namely that of factor scores. This hiatus was indeed baffling until the recent work of Green and McDonald [McDonald 1974]" (p.1). According to Guttman, the flawed reasoning of McDonald (1974) can be traced to McDonald's employment of what Guttman sarcastically calls "G-M logical principles":

G-M Principle 1: If b implies c, and if b is false, then c is false.

G-M Principle 2: If u does not imply v, but the proof of u is incomplete, then v is true.

G-M Principle 3: If the domain of P and range X of variable have no elements in common, then the elements of X are in P.

In Guttman's view, the main argument of McDonald (1974) begins on page 213, and rests chiefly on Principle 1, while Principle 2 is used to relate the first part of the paper to the second. Principle 3 "...is not entirely essential to the main argument...But is used heavily in the last portion of the paper to establish logical and empirical conclusions...the most striking of these...the Green-McDonald law of constant correlation. Largely by means of Principle 3, with assistance from Principle 1, they show that a certain kind of correlation...must be invariant in size over space and time, that is, independently of environment. This is not unlike Planck's constant, the constant speed of light, and other basic constants of physical science, and may be the first such an invariant quantity to have been deduced outside of the physical sciences" (p.3).

After a brief overview of the linear factor model, Guttman comments wryly that: "While there are many solutions to the factor scoring problem, there is a well-known general mathematical formula for calculating each and every one of them [Guttman, 1955]...Why, then, is there no computer program for calculating at least a few of these solutions, or even one of them?...Green and McDonald give a profound answer to this last question: a computer program is impossible...They show that "if we attempt to construct an observable counterpart of a factor, the correlation between this and the factor is equal to the square of the multiple correlation of the factor on the observed variables" (pp. 5-6). According to Guttman, McDonald (1974)

2 See the summary of Schonemann (1978) for more on this episode of alleged questionable editorial conduct.

unwittingly establishes that "...if we use the known mathematical formulas for calculating the scores y_{kp} for $p \in P$ on the kth common factor, then this will not yield the scores y_{kp} but only something whose correlation with these scores is $\rho^2_{y_k}$, where ρ_{y_k} is the multiple correlation coefficient of the y_k on the n x_i" (Guttman, 1975, p.6). Guttman concludes that "Here is a rather remarkable result. No matter how our programmers may try, if they follow the instructions of the mathematical equations--which are clear and must always yield a solution--the electronic computer will balk and will always give something which correlates only $\rho^2_{y_k}$ with what is desired. Indeed, all computer solutions must yield precisley this correlation for each data problem for each factor analyst in the world. This is the Green-McDonald law of constant correlation..." (p.6).

Guttman next turns to unpacking McDonald's definition of unobservable random variate. To recall, this definition is as follows:

> Let $\boldsymbol{\tau}$ be a vector defined on a given outcome space(sample space). Then $\boldsymbol{\tau}$ is *observable* if (a) we can know the value $\boldsymbol{\tau=t}$ of any observation drawn from the outcome space, or, (b) we have a rule for drawing an observation at random from a subspace of the outcome space in which $\boldsymbol{\tau=t}$, where t is any fixed value, known or not, in the domain of $\boldsymbol{\tau}$. It is unobservable only if it not observable, as defined (p.216).

Guttman's analysis of this definition is as follows:

i) The first sentence of the definition states that τ is a random variate since "..."defined" there means there is a rule uniquely relating a value of τ to each point of the outcome space" (Guttman, p.8)

ii) Condition (a) of the definition states precisely the same thing as the first sentence, since it too "...says there is a rule uniquely relating a value of τ to each point of the outcome space" (Guttman, 1975, p.8).

iii) Since condition (a) is sufficient for τ to be observable, and since every random variate satisfies condition (a), there follows: <u>Proposition 1</u>. Every random variable is observable.

iv) A second sufficient condition for τ to be observable is condition (b).

v) McDonald (1974, p.216) may be paraphrased as: "Let us consider, then, what is being asserted when we say that common factors (and hence unique factors) are unobservable. It is sufficient to say that we do not have [condition (b)]" (Guttman, 1975, p.9).

vi) Hence, "...by Principle 1, *not* satisfying (b) is sufficient for a random variable to *not* be observable. After giving their interpretation of condition (b), Green and McDonald go on to show that factor random variables do not actually satisfy (b) in their sense" (Guttman, 1975, p.9). There follows then <u>Proposition 2</u>: Factor random variables are always unobservable.

vii) From Propositions 1 and 2 there then follows Proposition 3: (Green-McDonald). Factor random variables are always both observable and unobservable.

viii) According to the law of contradiction of formal logic, Proposition 3 yields: Lemma 1 (Green-McDonald) Factor random variables do not exist.

ix) Since McDonald states that "Before we consider estimating factor scores, it must be shown that there exists something to be estimated" (McDonald, 1974, p.206), it follows that factor analysis cannot exist without common and unique factors.

x) Hence, there follows Theorem 1: (Green-McDonald) Factor analysis does not exist.

With regard the "law of constant correlation", Guttman claims that the reasoning of McDonald (1974) can be unpacked as follows:

i) The random variates of factor analysis all have the same domain, *P*, containing the population of individuals under study.

ii) Domain *P* has no values in common with the ranges of the random variates, observed or unobserved.

iii) McDonald states that "By definition, an individual's common factor scores are unique, in the sense that he is not simultaneously at more than one point of the outcome space of elementary events on which the specification of the model ultimately rests" (McDonald, 1974, p.217). According to Guttman, McDonald is apparently claiming that "...the outcome space *P* of individuals has further elementary events, different from individuals, and these must be a unique set of common factor scores" (Guttman, 1975, p.10). This conclusion is reached by taking Principle 3 and applying it to condition (b), by which it follows that values of the range of τ are in the domain , *P*, since τ has no elements in common with *P*.

iv) Guttman (1975, p.11) then paraphrases McDonald as claiming that "Lack of a rule for selecting the factor scores in the outcome space [that is, lack of condition (b)] leads to the "irreducible uncertainty as to where an individual is located in the outcome space" [ibid.]", and sarcastically concludes that "...a psychometric parallel to Heisenberg's uncertainty principle seems to have been established" (p.11)

v) Thus, what Green and McDonald have established is "...a particular consequence of uncertainty, namely the necessary invariance of the $\rho^2_{y_k}$ in light of Principle 3. This explains the untoward behavior of electronic computers" (p.11)

In the bibliographical note, Guttman states that "It may be of interest to put on record that some earlier, unpublished work of Green and McDonald established a much weaker variant of Theorem 1. A ms. version preceding McDonald [1974] discussed "trivial" and "nontrivial" factors instead of "observable" and "unobservable" random variables, and led to the (unpublished) Green-McDonald theorem that factor analysis is trivial. This version was accepted for publication by Psychometrika several years ago, until triviality of the theorem was noticed. The ms. was then withdrawn from publication, in the hope that nontrivial results could be established. The published 1974 version does contain much improved proofs, yielding the stronger Theorem 1 of nonexistence and the added feature of the law of invariant correlation" (p.12). The unpublished work to which Guttman refers, is, of course, McDonald (1972).

Mulaik (1976, *Psychometrika*)

In this paper, Mulaik attempts to correct McDonald (1974) in regard the latter's misconstrual of Guttman's measure of indeterminacy. He also explores the issue of a distribution of the set of correlations defined on all pairs of common factors to $\underline{\mathbf{X}}$ (i.e., all pairs of members of *C*). The article also provides a strong and fascinating statement of what is, herein, called the causal picture of the Central Account. Mulaik begins with an example that is intended to illuminate the nature of the indeterminacy property of the linear factor model. He states: "The idea of Guttman's measure can be illustrated with a simple example...Let *X* be a variable with an observed unit variance, having a correlation of ρ=.60 with another unit variance variable *Y*. The variable *Y* must be identified just on the basis of this information. Obviously, this is impossible; there is an uncountable infinity of variables that might be the variable *Y*...There can be but one variable *Y*. But because *Y* is unobserved, it is impossible to tell without additional information which of the vectors...represents *Y*" (p.250). There are, however, a number of ambiguities inherent to this example, chief among them the sense Mulaik intends for *identified*. For Mulaik to give us credit for "identifying" *Y*, must we be able to *list* the *Y*-scores of those individuals whose *X*-scores we possess (i.e., must we be able to extensionally identify *Y*)?[3] Or is it that we must be able to *name* *Y* (i.e., provide the name of a concept that is presumed to signify the *Y* scores). Based on other points he raises, and the fact that Mulaik provides no guidance as to how identification in the former sense could even be attempted, it would seem that the latter sense is intended.

The scenario that Mulaik, then, seems to have in mind is akin to the following:

> A variate, **X**, whose scores are signified by the concept *self-esteem* has the following correlations with variates $\mathbf{Y}_1$ (signified by *dominance*), $\mathbf{Y}_2$ (signified by *agreeableness*), and $\mathbf{Y}_3$ (signified by *sociability*): .67, .45, and .33. I provide you with the set of correlations, but keep from you the identity of variate **X**, i.e., knowledge as to the concept that signifies the scores that comprise the distribution of **X**. You must, using only your knowledge of the correlations and the identities of $\mathbf{Y}_1$ to $\mathbf{Y}_3$, "identify" **X** (i.e., deduce that its scores are signified by the concept *self-esteem*).

Now, what Mulaik intends to assert by likening the factor analytic context to a scenario such as this is made clear on page 252 (Mulaik, 1976), and can be paraphrased as follows:

i. There can be but one referent of the concept *common factor to* $\underline{\boldsymbol{X}}$;

ii. This referent is signified by an ordinary language concept whose name should properly replace the generic label *common factor to* $\underline{\boldsymbol{X}}$;

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3 Rozeboom (1988) discusses a number of the various senses of *identified* that arise in multivariate practice.

iii. However, the identity of this concept is unknown to the factor analyst;

iv. The identity of the concept must be figured out on the basis of the profile of correlations between the referent and various other variates;

v. Because, when a finite number of other variates are considered, many variates will have roughly the same profile of correlations as the referent of *common factor to* $\underline{\boldsymbol{X}}$, there will exist an unresolvable uncertainty in regard the identity of this referent, i.e., in regard the ordinary language concept that signifies it.

To square this interpretation of indeterminacy with the mathematics of indeterminacy, Mulaik: a) reinterprets the variates constructed according to (4.4) and (4.6) as "possible interpretations" of *the* single, particular (but unknown) thing he presumes is the referent of *common factor to* $\underline{\boldsymbol{X}}$. That is, according to Mulaik, these variates are the bases for guesses as to the identity of a concept that signifies the single, existing consituent of natural reality he takes to be the referent of *common factor to* $\underline{\boldsymbol{X}}$; b. reinterprets the indeterminate component, $\mathbf{I}_{\boldsymbol{\theta}(i)}$, as reflecting the *uncertainty* of the researcher in regard the identity of this presumed existing single referent.

Mulaik's interpretation of the indeterminacy property of the linear factor model will not, however, fly unless he is able to provide something more in regard the definition of the concept *common factor to* $\underline{\boldsymbol{X}}$ than that provided by the model itself. For, as will be recalled, the definition provided by the model (see Theorem 1, Chapter IV) admits of an infinity of (constructed) common factors to $\underline{\mathbf{X}}$. Without something more, Mulaik, therefore, has no rational basis for presuming that the referent of *common factor to* $\underline{\boldsymbol{X}}$ is a single thing, nor, certainly, that this single thing is a constituent of natural reality, nor that it is signified by an ordinary language concept.

The additional ingredients Mulaik has in mind are contained in the CAC. According to Mulaik, *the* referent of *common factor to* $\underline{\boldsymbol{X}}$ is the thing that actually generated the phenomena represented by the manifest variates. Thus he states: "To use Guttman's measure of indeterminacy in factor analysis, we need not assume that the factors generating the data on the observed variables are simultaneously all sets of variables satisfying [the construction formulas]. But our uncertainty as to which set of variables actually are the factors that generated the data must necessarily extend to all variables satisfying [the construction formulas] in the space in which our uncertainty resides" (p.252); "The problem of indeterminacy in factor analysis ultimately relates to the problem of interpreting the meaning of the factors obtained in the analysis. For example, suppose one researcher, after considering the potential causes that could have produced a common factor in a factor analysis of some variables, decides he has isolated that causal variable. He then establishes a set of empirical operations that will measure that causal factor, and finds the correlations of this measure with the original variables in the study. If the correlations match the correlations between the original variables and the factor in question, can he conclude that he has isolated the correct causal factor?" (p.252); "The resolution of controversy over the interpretation of factors will not come about from an inspection of factor scores or correlations of candidate variables with the original variables, but will become apparent after a more careful analysis of the causal network among the variables in question. Such analysis, performed outside of the factor analysis or in later factor analytic studies with additional observed variables, should allow us to rule out some potential candidate variables for the interpretation of the factors that otherwise satisfy (2) or (3) respectively" (p.254); "Guttman's measure of indeterminacy is useful only insofar as it dramatizes the perils of performing factor analyses with little thought as to the causal relationships expected among the variables studies. This will occur most frequently in exploratory factor analyses. In confirmatory factor analyses, indeterminacy is a less critical problem because presumably the researcher has given much thought to ruling out potentially conflicting causal explanations for his factors..." (p.254). And, of course, because, according to Mulaik, *the* single constituent of natural reality that is the referent of the concept *common factor to* ***X*** is the cause of the phenomena represented by the manifest variates, it cannot be a constructed random variate: "But no one could consider the variables in **x** generated in this way to be the common factor variables underlying the observed variables in **n**, because such artificial variables in **x** could not serve as causal explanations of the variables in **n**" (p.254).

Part II of the current work will be devoted to arguing that the CAC is a mythology. At present, the following issues, attendant to Mulaik's assertion of the CAC, should be noted:

i. Just as in the case of McDonald (1972), Mulaik does not offer any justification for his belief that the linear factor model has to do with the detection of causes of the phenomena represented by the variates contained in **X**. He provides no argument to sustain his implicit verdict that the linear factor model is a tool for the detection of constituents of natural reality that happen to be causes. But, furthermore, how can a *variate* be a cause of something? A variate is just a function. How can it be a cause in the sense that a force, material entity, or phenomenon can be the cause of something?

ii. Mulaik presumes that the referent of the concept *common factor to X* is a detected cause and that this cause "has a name", i.e., is necessarily signified by an ordinary language concept. Mulaik seems to suggest that a researcher can take this signification for granted, and turn to the task of "interpreting the factor", i.e., inferring *which* ordinary language concept, e.g., *self-esteem*, *dominance*, *intelligence*, is doing the signifying. But because, on the Central Account, the researcher cannot even potentially know the identity of this concept due to the unobservability of its referent, this amounts to the assertion that conceptual signification can hold independently of human conceptualization (human linguistic practices). That is, unbeknownst to any human (due to unobservability), the referent of *common factor to* ***X*** is signified by a concept from ordinary language (*dominance*, *self-esteem*, *creativity*, or some such concept). This is akin to claiming that the scores that comprise the distribution of the random variate common factor to **X** are *inherently* meaningful, the problem being merely that the researcher isn't privy to the concept doing the signifying, and amounts to the platonic or objectivist conception of meaning indigenous to the Central Account. Mulaik offers no justification to support his assertion of this key element of the Central Account and this is a tell-tale sign that an ürbild is in play.

iii. Mulaik wishes to recast the indeterminacy property, the fact that, when **X** is ulcf representable, there can be constructed an infinity of common factor to **X**, as the property that there exists "alternative and possibly contradictory interpretations for a given factor". On this account, the random variates contained in set *C* play the role of the alternative interpretations. But the elements of set *C* are random variates. Hence, Mulaik must explain how it makes sense to portray a random variate, merely a function, as an *interpretation* of something? To give an interpretation of φ involves the making of statements involving the concept "φ" that signifies φ, and for these statements to be comprehensible one must possess an understanding of the meaning of "φ". The meaning of concept "φ" is settled by the rules which govern its correct employment.

On the other hand, a function is not an element of ordinary language, let alone a set of statements involving the concept "φ" that signifies the thing φ to be interpreted.

iv. What is meant by *unobservable*? As in so many discussions of factor analysis, the meaning of this concept is left hanging, and this is, once again, a sign that an urbild is in play. That is, the Central Account, with its account of unobservability, is a presupposition of Mulaik's treatment of indeterminacy.

v. Mulaik presents examples of what might be called "ordinary causal cases". These cases involve putative causal relationships existing between phenomena of interest, these phenomena, as is evident in the fact that we can discuss them, create hypotheses involving them, etc., signified by concepts whose meanings we understand. Mulaik clearly sees factor analysis as having a place in the resolution of such "causal networks", and, converesly, an understanding of these networks as helping in the "interpretation" of the results of factor analyses. But he does not clear up how he sees this as taking place. For, if factor analysis has anything whatever to say about causal relations, it is not by way of "ordinary causal cases". For factor analysis does not involve an analysis of the causal dependency of a set of phenomena represented by the manifest variates, these phenomena signified by a set of p concepts, on a single phenomenon represented by a (p+1)th variate, φ, and signified by a *particular* concept "φ". The (p+1)th variate in a factor analysis is a latent variate, a variate which, at least prior to analysis, is not signified by any ordinary language concept. No rule is layed down to link this variate to a phenomenon of interest. At the least, Mulaik does not establish how conducting a factor analysis is akin to engaging in an exploration of whether, say, the cause of the phenomena represented by *these* eight "happiness variates" is the phenomenon represented by *this* self-esteem variate.

vi. Mulaik envisages the possibility of conducting analyses external to an initial factor analysis and using the results from these analyses to eliminate "candidate interpretations" and, possibly, to arrive at a correct take on the identity of the "true" common factor to $\underline{\mathbf{X}}$, that being the thing that *really* generated the phenomena represented by the manifest variates. But what, then, does he mean by *generated*? Is it enough to merely talk loosely about one's ideas regarding putative "causal networks" (I believe that "self esteem" causes "happiness") or must Mulaik provide a definition for this new (additional) sense of *causality*? He cannot, of course, look to the factor model to settle the meaning of the concept *generated*, because, even if it were true, as some believe, that the model defines what is meant by *causal source*, this definition, as has been shown, then admits of an infinity of such "causal sources", and, hence, is the *source* of the indeterminacy problem. That is, the latitude in the definition of *common factor to* $\underline{\boldsymbol{X}}$ provided by the model is precisely why Mulaik must consider stepping beyond what the model says.

vii. Not only does Mulaik reinterpret the existence of an infinity of random variates which are, definitionally, common factors to $\underline{\mathbf{X}}$, as the existence of an infinity of *interpretations* of *the* single causal source of $\underline{\mathbf{X}}$, but he goes on to equate indeterminacy with the existence in science of rival models. Thus he notes: "...controversy in genetics over the correct genetic model to account for phenotypic frequencies of various Rh antigens in the blood...R.A. Fisher proposed a genetic model involving three pairs of allelic genes linked closely together on a common chromosome...A.S. Wiener postulated a series of six alleles. Both models predicted the frequencies of observed phenotypes equally well" (p.253). But this comparison is questionable, for the genetic researchers Mulaik cites had resolved the important definitional issues inherent to their debate, and had moved on to arguing about the correct explanations of phenomena. On the other hand, rampant equivocation over key concepts such as *causal source*, *unobservability*, and *latency* has perpetually beclouded empirical work centering on the factor analysis of sets of variates. Even the definition of *common factor to* $\underline{\boldsymbol{X}}$ provided by the model itself has failed to satisfy workers in the field, which is why they, like Mulaik, attempt to bypass (4.4)-(4.6) in favour of the Central Account. To be guessing as to the identities of causal sources alleged to be identified through the empirical application of a model whose status as a detector of causal sources is wholly open to question, is not quite the same thing as "postulating a series of six alleles." The indeterminacy property is most certainly not equivalent to the existence of alternative theories or models, but, rather, is a consequence of the definition of *common factor to* $\underline{\boldsymbol{X}}$ supplied by the linear factor model. In Part II, the rampant confusion within the discipline of psychometrics over *concept*, *model*, *theory*, and other concepts essential to science will be analyzed in detail.

McDonald (1977, *British J. of Mathematical and Statistical Psychology*)

McDonald opens this paper with a brief explanation of factor analysis, and remarks that "We explain the correlations of the tests by their regressions on the factors, so we regard the factors as the attributes that the tests measure in common, and we interpret each common factor as the common attribute of those tests whose regression weights are large, in their regression on the factor. (It is necessary to make these remarks because, although they are the very axioms of common factor theory, they are sometimes forgotten.)" (p.165). Now, the interpretational convention he is describing is, essentially, the CAM. How it can be correctly said that the CAM is axiomatic for factor theory, McDonald does not explain. That is, he does not explain how he has ascertained that, when a particular $\underline{\mathbf{X}}$ is described by the linear factor model, the concept *common factor to* $\underline{\boldsymbol{X}}$ signifies an attribute of the phenomena represented by the $\mathbf{X}_j$, this attribute signified by an ordinary language concept. Certainly, it is far from clear that the normative employments of ordinary language attribute terms are as described by McDonald. Nor does he explain how this revelation squares with McDonald (1972), in which it was sworn that the "axioms" of factor theory implied that the linear factor model is a detector of causes, or McDonald (1974), in which factor scores were viewed as contained in the sample space on which the model is erected. It most certainly was not part of the axiom set McDonald (1975) offered as definitive of the linear factor model.

McDonald next observes that "In contrast to common factors, components are weighted linear combinations of a set of tests which may be chosen to have one of a number of optimal properties, but which cannot in general explain the observed correlations of tests, except approximately. Thus, as is well known, common factors and components are incompatible both with respect to their mathematical properties and the purposes for which they are intended" (p.165). According to McDonald (1977, p.165), "Since Spearman (1922) it has been known that we cannot determine the common factor score of each of a set of subjects, on the basis of a sample of tests of finite size." This claim is ambiguous, because it is not clear whether McDonald means to assert by his use of *determine* that common factors are not unique, or that realizations cannot be taken on common factor variates. The latter claim is, of course, fallacious, there being the common factors to $\underline{\mathbf{X}}$ constructed in accord with (4.4)-(4.6), on which realizations can most certainly be taken.

It is claimed by McDonald that "The indeterminacy of factor scores has been held by some writers (Guttman, 1955; Schonemann & Wang, 1972, for example) to create problems about the common factor model. A curious feature of these problems is that they do not seem to have any obvious bearing upon the use of the model in practice. Specifically, it has been shown that it is possible, for each of a large sample or population of subjects, to obtain infinitely many alternative sets of numbers having the properties of scores on a given common factor" (p.165). He observes that "This fact, essentially, has been claimed to render suspect 'the widespread practice of trying to name or attach meaning to factors merely by studying factor *loadings*' (Guttman, 1957a, p.149)" (p.165). However, "In order to recognize any force in this claim, not only do we need to know how to find at least two distinct possible factor variables, but also we need to know on what grounds we might interpret them differently. With the exception of one hint given by Mulaik (1976), which will be developed below, no such grounds have ever been given" (p.166).

Two points should be noted: i.) McDonald misrepresents the issue of factor interpretation. In particular, the issue is not whether the practice of factor interpretation yields *consistent* interpretations ("we need to know on what grounds we might interpret them differently") in the face of indeterminacy, but, rather, whether the indeterminacy property implies that the practice itself is *illegitimate*. In particular, are factor analysts justified in viewing a factor interpretation as yielding a guess as to the identity of a detected cause (attribute)? Does this view of the use of the model make any sense? It will be argued in Chapter IX that the practice of factor (latent variate) interpretation, a component of the CA, is nonsense, it being predicated on a perverted account of conceptual signification; ii.) As he has done throughout his series of papers on indeterminacy, McDonald subtly denies that random variates constructed as in (4.4)-(4.6) are common factors to $\underline{\mathbf{X}}$, he, instead, characterizing them as merely "having the properties" of scores on an alleged single, true common factor to $\underline{\mathbf{X}}$. And, in the end, McDonald explicitly deprives such constructed common factors to $\underline{\mathbf{X}}$ of the status of common factor-hood: "Since such alternative variables cannot lend themselves to distinct interpretations, it still remains to be shown how their existence renders the interpretation of factor loadings in any way suspect. It is probably this lack of interpretable empirical content in the mathematical demonstration of factor score indeterminacy that has caused most users of factor analysis to ignore the argument as 'metaphysical' or 'philosophical' in the pejorative sense of those words" (p.166). McDonald's defense of the linear factor model is really a defense of the Central Account. But the defense he erects itself *presupposes* the Central Account.

The stated object of McDonald (1977) is to "...present some further mathematical properties of certain components that have been derived as 'estimators' of common factor scores, and from the behavior of these to suggest a resolution of the doubts about the common factor model that have, we might say, been rumoured to follow from factor indeterminacy" (p.166). McDonald (1977) shows that "...the regression components given by Schonemann and Wang (1976) as an alternative treatment of the common factor model are the same as the OWLS factor score estimators given by Anderson & Rubin (1956), and are related by a change of scale or at most a non-singular tranformation to the usual LS estimators" (p.173). But, in truth, the paper's chief contribution is the further development the behaviour domain position that McDonald has, by this time, adopted as the foundation of factor analysis (and which will be considered in detail in Chapter XV of the current work). Hence, McDonald states that "The implications of the results quoted so far are quite clear. Provided that the tests we employ in a common factor analysis are drawn from a well-defined domain of possible tests (a behaviour domain, or universe of content), and provided that it may be supposed that any common factor we are interested in has a correlation of unity with the entire set of tests in the infinite domain, then the common factor is determinate in the domain, and so we may, if we wish, consistently define it as the limit of the estimator $\hat{\mathbf{x}}$, obtained as we augment a given set of variables with further tests drawn from the defined behaviour domain" (p.173). McDonald claims that this reasoning dates back to Spearman (1933). He states that "In response, then, to a claim by Schonemann & Steiger (1976) that advocates of factor estimation have not given a clear rationale for their efforts, it suffices to say, as both Spearman and Guttman have already indicated, that factors may (at least in some cases) be uniquely defined by all the variables in a behaviour domain, and these are the quantities we estimate with a finite sample drawn from the domain" (p.173). This is the origin of McDonald's abstractive property position.

The essential element of the abstractive property position, that the concept *common factor to* $\underline{\boldsymbol{X}}$ signifies a property/attribute whose name is an ordinary language concept-term (CAM, but especially CA2), is evident in the strange sentence containing "...and provided that it may be supposed that any common factor we are interested in...". What criterion does McDonald believe is available to the researcher that would allow him to *identify* the factor of interest to him? Imagine that I am about to embark on a factor analysis, the manifest variates being the items of the Need for Cognition Scale (Cacioppo, Petty, & Kao, 1984). I am going to test the hypothesis H_o: $\Sigma=\underline{\Lambda\Lambda}'+\Psi$ against the alternative H_1: [Σ is any gramian matrix]. How shall I, according to McDonald, provide a criterion that fixes the sense of the concept *common factor to* $\underline{\boldsymbol{X}}$ so that it denotes a *particular* common factor to $\underline{\mathbf{X}}$ of interest to me? To answer "need for cognition", or with some other ordinary language concept, is to erect smoke and mirrors, for it is not at all clear how, in a given application, a concept such as *need for cognition* is related to the technical concept *common factor to* $\underline{\boldsymbol{X}}$. On the other hand, to answer "the common factor to $\underline{\mathbf{X}}$" is simply circular. The fact is, there do not exist ordinary language means of fixing the sense of the concept *common factor to* $\underline{\boldsymbol{X}}$ (The equations and distributional stipulations of the model *do* provide such a means given that they describe particular $\underline{\mathbf{X}}$). And, yet, without a criterion with which to distinguish the common factor of interest to *me*, from other common factors, there would exist no way to judge whether *it* has a correlation of unity with anything, let alone the infinite set of tests contained in the behaviour domain. The quote from McDonald presumes an inherent, extra-linguistic meaning for the scores that comprise the distributions of the common factors to $\underline{\mathbf{X}}$, the view being that the researcher must simply reveal, through the practice of factor interpretation, this inherent meaning as if it were a feature of natural reality.

Eventually, McDonald turns to distancing himself from the stance of McDonald (1974). He acknowledges that "...McDonald (1974) did not offer convincing reasons for regarding a factor variable as unique and unknown" (p.174), and, in a footnote, claims that "An argument based on behaviour domain theory was, unfortunately, removed from an earlier draft in response to a referee" (p.174). But, as was seen, McDonald (1974) (as did McDonald (1972)) contains quite the array of disparate arguments each designed to show that indeterminacy is trivial. It is hard to know then where the extirpated behaviour domain case would have fit into this flurry of arguments. Once again, complaints are registered about Guttman's measure of indeterminacy, the observation being that "It is the lower bound measure...that has caused questions to be raised about the common factor model. No one expects tests to be perfectly *reliable* [italics added]" (p.174). Not surprisingly, McDonald (1977) does not clarify the sense he assigns to the term *reliable*, nor then the role reliability should be seen as playing in regard factor indeterminacy.

His use of the term appears to be yet another attempt to subtly misrepresent the nature of the indeterminacy property. It is a very good attempt, in that it erroneously implies that Guttman insisted upon unreasonable standards of *reliability* (recall McDonald's analogous misrepresenting of Camp as insisting upon unreasonable standards of predictive efficacy).

Steiger and Schonemann (1978, in S. Shye, *Theory construction...*)

This is a very important paper, that, unfortunately, appeared in a relatively low-profile book. Steiger and Schonemann present what might be called a "frustrated" history of the factor indeterminacy debate. They provide a relatively exhaustive account of psychometric dialogue on the indeterminacy property, but the subtext is clearly along the lines of, "why hasn't the discipline of psychometrics given serious consideration to this key feature of the linear factor model." As Steiger and Schonemann state, "The history of factor indeterminacy is...rather uneven. Periods of great activity have alternated with periods of almost total neglect. Some writers have attached great significance to the issue. Others have dismissed it as trivial. Most have completely ignored it" (p.143). Steiger (1996, p.621) reports that "Not long after we began circulating *A History of Factor Indeterminacy*, we received feedback that many of our colleagues found the article threatening and annoying." This is hardly surprising, for the subject matter of the article is decidedly a threat to the Central Account.

The paper begins with a simple example clearly intended to strip the topic of foreboding mathematics and, hence, allow the applied researcher access to the issue. Among many important contributions are the following:

i) The article asks pointed questions about the omission of discussion of the indeterminacy property from key factor analytic texts such as Wolfle (1940), Thurstone (1947), and Harman (1960). Mulaik (1986) has portrayed the apparent obliviousness of Thurstone and disciples in regard the indeterminacy property as a simple case of their being too busy making history to take note of it. However, the omissions noted by Steiger and Schonemann, when considered alongside the various misrepresentations of the indeterminacy property when it *has* been given consideration, suggest that Steiger and Schonemann may be well justified in implying conspiracy.

ii) The pointing out of the fact that "Spearman was not always careful to acknowledge the distinction between proving the *compatibility* of data and mathematical system and proving the empirical *existence* of the mathematical system's constructs." (p.145) In the current work, this point will be paraphrased as a blurring of the distinction between establishing mathematically the existence of variates that satisfy the requirements for factor-hood as laid down in the equations of the linear factor model, and proving the existence of consitutents of natural reality that cause (are properties of) the phenomena represented by the manifest variates.

iii) The highlighting of Spearman's confusion, intentional or otherwise, over the distinction between the concepts of *unpredictability* and *indeterminacy* (p.146).

iv) The identification of E.B. Wilson as an important figure in the history of factor analysis.

v) The identification of many of the manoeuvers that have come to be employed within psychometrics to protect what is, in the present work, called the Central Account. For example, they identify the belief that, in addition to random variates constructed as per (4.4)-(4.6), there exists yet some other common factor to **X** that is the "true" common factor to **X**. This belief is, of course, a key element of the CA.

Schönemann (1978, *Society for Multivariate Experimental Psychology*)

The chief purpose of this talk, delivered at the 1978 meeting of the Society for Multivariate Experimental Psychology, was clearly to raise questions about the ethical conduct of the psychometrics community in regard its handling of the indeterminacy issue. Essentially, Schonemann's account implies the existence of some element of conspiracy within the psychometrics community to suppress and distort facts and implications with regard the indeterminacy property, thereby inhibiting the growth of wide-spread understanding of the threat indeterminacy posed to factor analytic practice. Schonemann reviews the early development of factor analysis, including Spearman's claims about its social policy implications, and Rummel's likening of it to quantum theory. He states that "During all this time most users were left unaware that they were driving, so to speak, a car overdue for recall. The underlying model has a built in indeterminacy which potentially threatens the very purpose for using it" (p.2). According to Schonemann, following Wilson's discovery of the indeterminacy property, there came, within psychometrics, a long period of "theoretical regress" in which "What had started out as a plausible and empirically supported model of human behavior, became more and more a mechanical "calculus of the social science" in the worst sense of the word" (p.2). Moreover, the work of Wilson and contemporaries on factor indeterminacy was omitted from virtually every factor analysis text, Schonemann citing twenty-two examples of omission from articles and texts published between 1935 and 1975.

He claims that during the post-Thurstonian period of "blind factor analysis", only sociologist Louis Guttman showed concern for the implications of indeterminacy to factor analytic practice, and asks "...how it could have happened that for 40 years a large body of earlier research, which might have contributed to a more enlightened attitude towards a widely used research method, disappeared without a trace from our journals and texts" (p.6). His tongue-in-cheek answer is that "So far, only one rational explanation has been offered: "...the results on factor indeterminacy (or their) consequences...are trivial" (AA, 1972, Item 25, p.2)" (p.6), the quotation, of course, from McDonald (1972). Using codes to stand for the names of the involved journals and psychometricians, Schonemann provides an account of the acceptance of McDonald's 1972 article for publication in Psychometrika by the editor, Professor Bert Green, and the subsequent investigation by the Psychometrik Society into allegations of editorial corruption made by both Schonemann and Guttman. This account can be summarized as follows:

i) The editor overruled technical criticisms raised by reviewers, and accepted McDonald's paper for publication on August 8, 1972, "...on grounds (1) such 'arguments are essentially philosophical, that is, there does not seem to be any technical issues' (2) 'the current version of (the) paper is very well written and very clear', and (3) although there was '...a critic. Two other reviewers recommended that your paper be published'." (Item D6)" (Schonemann, 1978, p.6)[4]

4 Itemized quotations cited by Schonemann are taken from the report of "The Ad Hoc Committee to Consider

ii) In fact: a) There *did* exist technical difficulties with the manuscript because "...after several additional revisions...the paper was eventually "withdrawn"" (Schonemann, 1978, p.6), to be later replaced by another version (*The Measurement of Factor Indeterminacy*, Psychometrika, 1974); b) "The paper was neither clear nor very well written. Even under the most sympathetic reinterpretation, certain 'serious faults of exposition ..give the appearance of error, and leave the intended meaning in doubt' (Final Report, 1978, p.12)" (Schonemann, 1978, p.6); c) There were actually two negative reviews (one by Schonemann and Wang and one by Guttman) and only one positive review; d) The single positive review was of questionable scholarly merit, as is evident in the reviewers statement that "...while reading I did not arrive at the idea that this ms. was a criticism of the work on factor indeterminacy until half way through...Personally, I reject the claims, or implied claims, that the factor models are worthless on the basis of the work on factor indeterminacy and find great difficulty in reading more such papers...When the purpose of this paper became clear to me my enthusiasm grew..." (Schonemann, 1978, p.6)

iii) The editor of Psychometrika was well aware that McDonald's article was an "attack inevitably...aimed at your [Schonemann and Wang's] work...knowing that it included some loaded words with the tone of debate" (p.7) and Schonemann questions why then he, Wang, and Guttman were not invited to contribute responses.

iv) When the editor would not clarify matters surrounding his acceptance of the McDonald paper, Schonemann addressed the editorial council of Psychometrika on December 7, 1972. For a full year the council failed to take any action. After a second prompting, the chairman of the council relayed to Schonemann the council's unanimous decision of December 18, 1973, which, in the opinion of Schonemann, was evasive. Schonemann was told that the McDonald (1972) manuscript had been withdrawn but, as he notes, he had inquired as to how it could have been accepted in the first place. Schonemann was also informed that measures had been taken to ensure that "appropriate freedom be provided for publication of conflicting viewpoints" (p.7). No details were provided by the council as to what those measures would consist in.

v) On July 18, 1976, Guttman requested in a letter to the Chairman that the editorial council suspend B. Green from being editor, and that an independent committee be struck to investigate the refereeing process at Psychometrika. Guttman recommended that the committee be made up of members from the Institute of Mathematical Statistics. The council responded promptly: "'In view of [Guttman's] eminence, the Council voted to appoint a Committee...and make a firm recommendation...to what actions the Council should take' (Final Report, 1978, p.3)". Unfortunately, the committee did not consist of professional mathematicians as per Guttman's suggestion, but three former presidents of the Psychometrik Society, two of whom "...had previously worked in areas directly affected by the indeterminacy problem" (Schonemann, 1978, p.7). One might also inquire as to why Guttman's eminence was such an important factor in the editorial council's decision to launch an investigation. One would think that, rather than Guttman's happiness or unhappiness, the council's concern would have been with ensuring that the editorial process at *Psychometrika* was indeed fair.

Certain Charges"

vi) The committee issued its report on June 1, 1978. With respect the editor's conduct, it concluded that "'We have found only a few lapses in the editor's judgment...and no evidence whatsoever of any impropriety...We also find much in his behavior that is praiseworthy' (Final Report, p.58)" (p.8)

vii) Schonemann reports on another unsettling incident involving the *British Journal of Mathematical and Statistical Psychology* (BJMSP), and its editor of the time, Philip Levy. Essentially, a paper of Schonemann and Steiger, eventually published in the *Bulletin of the Psychonomic Society* (1978), was originally submitted to BJMSP. After a prolonged period of inexplicable delays, an apparent difficulty in finding willing reviewers, and some very strange correspondence from the editor, this correspondence giving the impression that he was simply trying to find some justification for blocking publication, the paper was finally rejected twenty-two months after submission. Meanwhile, the same editor accepted for publication McDonald's *The indeterminacy of components and the definition of common factors* (1975), a paper in which McDonald, once again, takes issue with the work of Schonemann, Steiger, Wang, and Guttman, some of this work contained in the rejected paper.

Schonemann and Steiger (1978, *Bulletin of the Psychonomic Society*)

This article opens by noting that certain individuals, notably McDonald, have argued that the implications of indeterminacy for factor analytic practice are trivial. Schonemann and Steiger (1978, p.287) state that "Once one accepts the fact that the factors are not uniquely defined by the model, one faces the question of how the indeterminate increment that is needed to define them can possibly enhance our knowledge of other variables. In the past, opinions were divided on this question. Some have argued that factor analysis is superior to component analysis, which defines new variables simply as linear combinations of the observed variables, precisely because the factors, in contrast to components, "go beyond the test space." (p.287). The purpose of the article was "...to lay the ground for a rational study of...the relation between factor indeterminacy and external prediction...and...concerns the purpose of factor analysis as a scientific method" (p.287). The article provides theorems that establish that, for *any* criterion variate, there exists a set of common and unique factors to $\underline{\mathbf{X}}$ that predicts this criterion with squared multiple correlation equal to unity (see Theorem 6, Chapter IV). Such a result is obviously a threat to the Central Account because it portrays the referent of concept *common factor to* $\underline{\mathbf{X}}$ to be the elements of set C, a set of constructed random variates, and, in doing so, undermines the CA thesis that a single cause (property) has been detected.

Mulaik and McDonald (1978, *Psychometrika*)

This article was chiefly on the topic of the inifinite behaviour domain response to indeterminacy, a topic that will be taken up in Chapter XV of the current work. However, the article also contains a number of questionable characterizations of the facts of indeterminacy, the aim apparently being to defend certain of the core theses of the Central Account. Examples include the following:

i) Throughout the paper singular grammar is employed to describe the referent of the concept *common factor to* $\underline{\mathbf{X}}$, even though both Mulaik and McDonald were well aware that set C, which contains the common factors to $\underline{\mathbf{X}}$, has infinite cardinality.

ii) It is claimed that "Guttman [1956; 1957, pp.148-149] and Mulaik [1976] have implied that as a result of factor indeterminacy there might be varying and in some cases contradictory *interpretations* [italics added] for a given factor" (p.178). This, however, is a misportrayal of both indeterminacy and Guttman, the apparent aim being to protect the key CA tenet that the referent of the concept *common factor to* $\underline{\mathbf{X}}$ is a single property/attribute (causal source) which is signified by an ordinary language concept whose identity is unknown, and, hence, must be deduced ("interpreted"). Indeterminacy does not imply that there "might be contradictory interpretations for a given factor", but, rather, is the property of linear factor representations that, when $\underline{\mathbf{X}}$ is linear factor representable, there are an infinity of common factors to $\underline{\mathbf{X}}$ (random variates that satisfy the requirements for common factor-hood as stipulated by the model).[5] Second, Guttman did not suggest that there might be, in a given application, contradictory interpretations of *the* common factor, but, rather, questioned the *coherence* of the practice of "factor interpretation" (a practice which *presupposes* that the concept *common factor to* $\underline{\mathbf{X}}$ has but a single referent) in light of the indeterminacy property. Certainly, Mulaik and McDonald do not provide any guidance to the reader who might question how it makes sense to attempt to "interpret" a *variate*, a variate being merely a function.

It has, herein, been suggested that the line of reasoning that informs factor/latent variate interpretation is, in turn, informed by a very particular brand of metaphysical commitment that is a characteristic of the Central Account. The reasoning can be sketched as follows: i) The concept *common factor to* $\underline{\mathbf{X}}$ signifies a cause (property) of the phenomena represented by the set of manifest variates that happen to be described by the latent variable model in question; ii) This concept is merely a place holder for an ordinary language concept (perhaps *self-esteem*, *neuroticism*, or *dominance*) that is the true "name" of the cause (property) detected in the analysis. The researcher must deduce the ordinary language concept that is the true signifier of the detected cause (property); iii) The scores that comprise the distribution of the random variate $\boldsymbol{\theta}$ to $\underline{\mathbf{X}}$ are measurements with respect this detected cause (property). If the researcher has interpreted the cause (property) to be, say, dominance, i.e., to be signified by the ordinary language concept *dominance*, then he can speak of these scores as being dominance scores or dominance measurements. As will be discussed in detail in Part II, because it portrays the researcher as attempting to deduce post-hoc which ordinary language concept signifies the constituent of natural reality he believes he has detected, this picture necessarily asserts the coherence of the idea of conceptual signification that holds outside of human linguistic practices (essentially, platonic concept meaning).

Later in the paper, it is stated that "To be empirically meaningful (as opposed to only mathematically meaningful as an arbitrary mathematical construction) the numerical values of the factor random variable must represent an empirical ordering of outcomes of the sample space" (p.178); "...Any empirical variable having the same correlations with the observed variables as a factor variable represents a potential interpretation for the factor. Yet factor indeterminacy means that it is mathematically possible for more than one empirical variable to be found having the specified pattern of correlations with the observed variables as the factor in

5 It will be recalled that Mulaik also mischaracterizes the indeterminacy property by likening it to, at various points in his writing, the existence, in science, of a multiplicity of rival theories.

question" (p.178). Having tacitly asserted the truth of the CA thesis that when $\underline{\mathbf{X}}$ is ulcf representable, a single thing has been detected, Mulaik and McDonald (1978) follow Mulaik (1976) in denying that random variates constructed in accord with (4.4) and (4.6) are common factors to $\underline{\mathbf{X}}$. For, of course, that an $\underline{\mathbf{X}}$ could possess common factors that are constructed random variates runs counter to the claims of the Central Account. Their employment of "empirically meaningful" is ambiguous, but, once again, represents the metaphysical take on conceptual signification that is a hallmark of the CA.

iii) It is reported by Mulaik and McDonald (1978) that "The essence of the argument would be that while the interpretation of a factor is based on "factor loadings", ultimately the interpretation corresponds to a hypothesis about the existence and nature of a certain random variable defined on a probability space associated with the observed variables" (p.178).

But, if the issue is the existence of a random variate defined on a probability space, then Theorems 1 and 2 of Chapter IV establish that, if $\underline{\mathbf{X}}$ is ulcf representable, not only does one such random variate exist, but, in fact, an infinity of such variates do. In fact, the quote is an example of the unacknowledged passing back and forth between different senses of the concept *existence*. For the existence of a particular type of random variate, in a given context, does not mean the same thing, and is not established in the same way, as the existence of some particular constituent of natural reality.

v) "To find a determinate solution for an indeterminate factor simply means finding an observable variable having the properties of the factor relative to the *p* given variables" (p.178); "What is not generally recognized is that producing a "determinate solution" for an indeterminate factor does not necessarily resolve the factor indeterminacy problem, because determinate solutions are not necessarily unique" (p.178).

This is a misportrayal of the concept *factor indeterminacy*. In particular, a determinate solution is not equivalent to "finding an observable variable having the properties of the factor relative to the *p* given variables", nor to finding a function of the random variates that "has the properties of the factor". It is precisely because, when $\underline{\mathbf{X}}$ is ulcf representable, one can produce an infinity of variates that are definitionally common factors to $\underline{\mathbf{X}}$ (i.e., that *common factor to* $\underline{\boldsymbol{X}}$ lacks unique reference) that the representation is indeterminate. Conversely, a determinate ulcf representation is one in which the cardinality of *C* is unity. Whether or not one can produce an "observable variable having the properties of the factor" is irrelevant.

J. Williams (1978, *Psychometrika*)

This paper promised a "...rigorous definition for a factor analysis model and a complete solution of the factor score indeterminacy problems..." (1978, p.293). But, in fact, what Williams offers is not a solution to the indeterminacy problem, but an entirely different formulation of the linear factor model, a formulation that has a strong kinship to traditional variate (behaviour) domain treatments of the indeterminacy problem. Williams considers the sequence

$$\underline{\mathbf{X}}_p=\underline{\Lambda}_p\underline{\boldsymbol{\theta}}+\Psi_p^{1/2}\underline{\boldsymbol{\delta}}_p,$$

in which $\underline{\Lambda}_p$ is a p by r real matrix, $\Psi_p^{1/2}$ is a p by p nonsingular real matrix, $\underline{\boldsymbol{\theta}}$ is a random vector containing r common factors, $E(\underline{\mathbf{X}}_p)=\underline{0}$, $E(\underline{\boldsymbol{\delta}}_p)=\underline{0}$, $E(\boldsymbol{\theta})=0$, $E(\underline{\mathbf{X}}_p\boldsymbol{\theta})=\underline{\Lambda}_p$, and $E(\underline{\boldsymbol{\theta\theta}}')=I$. The subscript p denotes any selection of p elements from an infinite random sequence of manifest variates with finite variances. Williams' carefully distinguishes between the issues of existence of a given factor representation and the uniqueness of the common factor random variates. He states that a factor analysis model "exists" if the above conditions hold for every value of p, and the following "stability condition" holds: $|\underline{\Lambda}_{p+q}'\underline{\Lambda}_{p+q}-\underline{\Lambda}_p'\underline{\Lambda}_p| \neq 0$, in which the p variates in the second set are a subset of the p+q variates in the first. This idea, that each selection of p variates is nested in (is a subset of) each selection of p+q variates, q>0, is fundamental to Williams treatment (and also to the restatement of the variate domain position given in Part 3 of the current work).

The aim of Williams inquiry is to establish, given that a factor model exists, conditions under which there is a unique $\underline{\boldsymbol{\theta}}$, no solution for $\underline{\boldsymbol{\theta}}$, and multiple solutions for $\underline{\boldsymbol{\theta}}$. Even in his technical treatment of the determinacy in the limit scenario, he still cannot resist the pull of the Central Account. Thus, he describes the probability space as "...a predictive model for a process with uncertain workings..." (p.294), and provides, as an example, "...the multi-faceted interplay of biological, physical, and psychological systems which produces human beings who are confronted with and respond to admissible mixes of internal and external stimuli in a behavior mode we call personality" (p.294). He also mentions, in reference to models such as principal component analysis, that "...dimensionally dependent solutions have no meaning as factor scores because they depend on the variates selected for observation rather than the process that generated them..." (p.295). As is typical of discussions of latent variable modelling technology, Williams does not explain how his perfectly standard statistical treatment allows the equations of linear factor analysis to jump the fence and graze in the pastures of underlying, generating processes. It is not clear why he believes that a consideration of the asymptotic properties of selections of nested sets of variates produces a "...predictive model for a process with uncertain workings..."

McDonald and Mulaik (1979, *Psychological Bulletin*)

This paper, offered as a non-technical review of the indeterminacy issue, is far more a policy statement, an attempt to persuade psychologists of the correctness of the thinking of McDonald and Mulaik on the topic of indeterminacy, and, without actually saying so, rebut Guttman, Schonemann and Steiger. It even reports on a "developing consensus about the problem and its implications" (p.297), a portrayal which is likely to induce in the reader familiar with the indeterminacy debate feelings of incredulity. On the other hand, it provides an almost letter perfect telling of the CAM, the measurement picture of the Central Account.

Throughout the paper, McDonald and Mulaik enforce the key CAM tenet that the "concept *common factor to* $\underline{\boldsymbol{X}}$ has a single referent" by speaking of nothing but *the* factor of a set of variates. To support this commitment, they take random variates that satisfy all of the requirements laid down by the linear factor model as sufficient for common factor-hood, as "possible factors", or as "having the properties of *the* common factor", or as "behaving like scores on a given common factor". For example, in regard the indeterminacy property, their comments include the following: "Factor-score indeterminacy refers to the fact that the common and unique factor scores in the common factor model are not uniquely determined by the observed variables whose correlations they explain..." (p.297). "This fact has sometimes been taken to mean that one cannot obtain exact scores on a common factor...Such a view is not strictly correct, as it is quite possible to construct numbers that behave precisely like scores on a given factor" (p.297); "...there are still infinitely many random variables that can satisfy the conditions for being a possible factor variable...(p.298); "A necessary and sufficient condition for a random variable X^* (in standard score form) defined over the population P to have the properties of X..." (p.299); "If a standardized random variable X^* has the required correlation with $\boldsymbol{y}$, one says that it is a possible factor variable of $\boldsymbol{y}$" (p.299). Note, however, that if Mulaik and McDonald are to coherently claim that the elements of set C merely have the properties of the true, common factor to $\underline{\mathbf{X}}$, then they must possess some way of distinguishing these "possible factors" from the real thing. Hence, the true, common factor to $\underline{\mathbf{X}}$ must have some additional properties beyond those stipulated by the linear factor model.

This additional stuff is the stuff of the Central Account, whose tenets are subtly asserted throughtout the paper:

i) When a particular $\underline{\mathbf{X}}$ is linear factor representable, a property/attribute of the objects under study has been detected/discovered;

ii) this property/attribute, scores with respect to which comprise the distribution of $\boldsymbol{\theta}$ to $\underline{\mathbf{X}}$, is signified by an ordinary language concept;

iii) Because its referent is unobservable, the signifying concept is unknown to the researcher and, hence, he must employ estimated factor loadings to make an inference about which property/attribute has been discovered, i.e., which concept-name should properly replace the generic *common factor to* $\underline{\boldsymbol{X}}$.

Mulaik and McDonald, for example, explain that "In its broader implications...it concerns the inability of a finite set of observed variables in an exploratory factor analysis to determine unambiguously *what attribute of the individuals the factor variable represents* [italics added]. This is imortant, as one will see, because factor analysis has commonly been treated as a theory-generating device; that is, it has been treated as a device for the *post facto discovery of the psychological concepts* [italics added] that explain the correlations of the variables one has chosen to measure" (p.298). Note the fact that it is taken for granted that, when a particular $\underline{\mathbf{X}}$ is linear factor representable, a property/attribute of the phenomena represented by the $\mathbf{X}_j$ has been detected. The problem is taken to be that the researcher can't be sure *which* property/attribute has been detected, i.e., which ordinary language concept term denotes this detected property/attribute. An inference as to its identity must be made: "In applications of the model, one attempts to fit the hypothesis...to a sample correlation matrix. If the fit is acceptable, one interprets the general factor X as whatever attribute of the persons in P seems to be common to the tests" (p.299). Once again, this is equivalent to asserting the coherence of the claim that conceptual signification exists outside of the human linguistic practices that contain the concepts that are presumed to be doing the signifying (non-normative or platonic meaning). McDonald and Mulaik do not attempt an explanation as to how this makes sense.

Obviously, then, because, according to McDonald and Mulaik, the model detects existing, inherently meaningful, properties/attributes of phenomena under study, the concept *common factor to* $\underline{\boldsymbol{X}}$ certainly cannot signify a mere constructed random variate. Hence, something must be done about the set of random variates constructed in accord with (4.4) and

(4.6) that are, according to the requirements of the model, definitionally common factors to $\underline{\mathbf{X}}$. McDonald and Mulaik dispense with them in a manner analogous to Mulaik (1976). That is, the constructed factor to $\underline{\mathbf{X}}$ are portrayed not as *real* factors to $\underline{\mathbf{X}}$, but, rather, as reflecting the uncertainty attendant to the inference the researcher must make as to the identity of the *single* detected attribute/property. This is evident in the following series of quotes:

a) "...although such computational procedures do indeed yield mathematically admissible alternative factor variables, the solutions so obtained can hardly be regarded as measurements of empirical properties of persons in the population *P*. Measurement constitutes the assignment of numbers to objects in such a way as to represent empirical relationships by numerical relationships. It is hard to see how assigning artificially generated random numbers to persons in the population can represent empirical relationships (of order, say) among these persons. Thus these alternative factor variables lack empirical significance and as such cannot be regarded as lending themselves to distinct interpretations of the factor" (p.301).

b) "It turns out...that in exploratory factor analysis as it is usually employed, the range of possible mathematical constructions of a possible factor variable corresponds to a range of possible ambiguity in the interpretation of a common factor" (p.298);

c) "Mulaik (1976) rejected McDonald's second argument, claiming that Guttman's lower bound represents a measure of the extent of possible disagreement between two investigators about the nature of a factor. Mulaik's argument assumes that two investigators might actually find two distinct empirical measures, either of which has the properties of the common factor variable. Further, the empirical measures might be imperfectly correlated, and their test contents might give conflicting interpretations of the factor..." (p.301);

d) "Mulaik further argued that in typical applications factor analysts will have at most only a few alternative empirical interpretations of a factor..." (p.301)

McDonald and Mulaik (1979) correctly note that random variates constructed as per (4.4) plus (4.6) cannot yield realizations that are signified by ordinary language concepts. That is, constructed random variates lack the conceptual signification that imputes to their realizations the meaning the psychologist desires in his scores. But they provide no argument in support of the view of conceptual signification they wish to install, i.e., that of the Central Account.

Steiger (1979, *Psychometrika*)

This article involves a derivation of the range of the correlations between the elements of the set of common factors to $\underline{\mathbf{X}}$, elements of set *C*, and a (p+1)th variate **Y** (see Theorem 5, Chapter IV). Steiger distinguishes between principal components and common factors and concludes that "In many practical data analytic applications, this theoretical distinction may seem of minor consequence. On the other hand, as has been demonstrated here with the theory of external correlations, factor and component models may diverge sharply in some situations." (p.97)

Bartholomew (1981, *British J. of Mathematical and Statistical Psychology*)

This article has been taken by some as resolving the indeterminacy debate. Aitkin, for example, opines that Bartholomew's treatment "...lays to rest past arguments over the status of factor scores" (1985). Essentially, Bartholomew claims that indeterminacy has been "an issue" only because those who have seen it as an issue have understood neither the concept *random variate*, nor the structure of statistical models. To Bartholomew, "A *statistical model* is a statement about the joint distribution of a set of random variables. A *mathematical model* (in the restricted sense used here) is a set of equations relating real numbers" (1996, p.551). His definition of random variate is standard. He cites little of the work on factor analysis and indeterminacy from the psychometrics literature, and seems to view this work as primitive: "I suspect that history has much to answer for in our present confusion. Factor analysis is a sophisticated multivariate technique but it arose when statistics was in a primitive state with no adequate concept of a statistical/probability model. Its subsequent evolution left it largely untouched by modern statistical developments. For this, statisticians must take their share of the responsibility" (1996, p.554).

The gist of Bartholomew's treatment of indeterminacy arises from the general representation of latent variable models that he has helped to popularize (that of (2.1)), $f_{\underline{\mathbf{X}},\boldsymbol{\theta}}(\underline{\mathbf{X}}=\underline{x},\boldsymbol{\theta}=\theta) = f_{\underline{\mathbf{X}}}(\underline{\mathbf{X}}=\underline{x} \mid \boldsymbol{\theta}=\theta)f_{\boldsymbol{\theta}}(\boldsymbol{\theta}=\theta)$. This representation goes back to, at least, Anderson (1959) and McDonald (1962). As was reviewed in Chapter II, the linear factor model is specified by making certain particular choices in regard $f_{\underline{\mathbf{X}}}(\underline{\mathbf{X}}=\underline{x}|\boldsymbol{\theta}=\theta)$ and $f_{\boldsymbol{\theta}}(\boldsymbol{\theta}=\theta)$. Bartholomew's argument then runs as follows: i. Once $\underline{\mathbf{X}}$ has been observed, it can no longer be treated as a random vector, the act of observation converting it into a vector of real numbers; ii. $\boldsymbol{\theta}$ is still not observed, and cannot be observed because it is "unobservable"; iii. As a result of (ii), one can only make inferences about the distribution of $\boldsymbol{\theta}$ via knowledge of the conditional distribution of $\boldsymbol{\theta}$ given $\underline{\mathbf{X}}=\underline{x}$ (see section (2g); iv. This conditional distribution is not typically a point distribution, and this is the property that has been mislabeled as "indeterminacy". In his words: "..it is obvious that the indeterminacy is simply a reflection of the fact that **Y** [$\boldsymbol{\theta}$ in the present notation] is still a random vector after **X** [$\underline{\mathbf{X}}$ in the present notation] has been observed. Only if the posterior probability were to be concentrated on a single point (as in the principal components case) would the *y*s be determinate. To speak of indeterminacy as a 'problem' is thus to overlook the essentially random character of the quantities concerned." (1981, p.97).

Bartholomew's account, far from a cutting edge treatment of statistical models and, in particular, factor analysis, is somewhat superficial. Does Bartholomew really believe that to define *statistical model* as "...a statement about the joint distribution of a set of random variables..." actually settles issues pertaining to the rules of correspondence that link the terms of a given "model", and notably those mischievous terms called latent variates, with those "objects" for which they are to stand (if, in fact, they do stand for anything)? Does he feel that to distinguish between a random variate and its realizations establishes rules for the admission of objects into the population about which the model is supposed to speak? Does he believe that these issues of representation are made transparent because he can point out that, when $\underline{\mathbf{X}}$ and $\boldsymbol{\theta}$ have a joint distribution, $\boldsymbol{\theta}$ has a distribution conditional on $\underline{\mathbf{X}}$? Apparently so, for he states that "Since the only thing that distinguiushes *X* [$\boldsymbol{\theta}$] from **Y** [$\underline{\mathbf{X}}$] is its unobservability, the mere introduction of a latent variable into the model, hardly justifies a wholesale departure from statistical practice" (1996, p.553). Then again, he seems also to feel that it is self-evident that the linear factor analytic claim about "the joint distribution of a set of random variables" produces a tool that can be employed to detect causes.

Bartholomew asks why, given that distributional claims are made about *both* $\underline{\mathbf{X}}$ and $\boldsymbol{\theta}$, the fact that $\boldsymbol{\theta}$ is a latent variate should necessitate wholesale departures. Well, why indeed. To begin, the discipline of statistics has traditionally preferred the making of distributional pirouettes over the consideration of the difficult issues involved in using stochastics as components of scientific investigation. This has resulted in its inventing increasingly trivial senses of the concept *model*. In particular, Bartholomew does not establish that the "models" of which he speaks are, as they stand, models *of* anything. To offer the decomposition $f_{\underline{\mathbf{X}},\boldsymbol{\theta}}(\underline{\mathbf{X}}=\underline{x},\boldsymbol{\theta}=\theta) = f_{\underline{\mathbf{X}}}(\underline{\mathbf{X}} = \underline{x} \mid \boldsymbol{\theta} = \theta)f_{\boldsymbol{\theta}}(\boldsymbol{\theta} = \theta)$, even with the standard further restrictions of conditional normality, and uncorrelatedness, of $\underline{\mathbf{X}}$ given $\boldsymbol{\theta}$, and a claim about the distribution of $\boldsymbol{\theta}$, leaves the reader egregiously ignorant as to what this "model" can be *about*. No clue is given as to what the symbols $\underline{\mathbf{X}}$ and $\boldsymbol{\theta}$ appearing in the model equations, stand for. Does the fact that $\underline{\mathbf{X}}$ and $\boldsymbol{\theta}$ are assigned densities help clarify matters? Not in the least.

What would help clarify matters? In the first place, it would help to know that the scores that comprise the distributions of the $\mathbf{X}_j$ are produced by following rules $\{r_1,r_2,...,r_p\}$ that are known by the researcher prior to running the analysis, and, with respect a given application of the model, to be actually *given* these rules. To know the set of rules $\{r_1,r_2,...,r_p\}$ for the production of the scores that comprise the distributions of the manifest variates in large part settles the issue as to the meaning of the symbols $\mathbf{X}_j$, j=1..p, that appear in the model equations. The symbol $\mathbf{X}_j$ stands for the set (distribution) of scores that would be produced by applying r_j to each member of the population *P* under study, i.e., by taking as the argument of r_j, each member p_i of *P*: ($r_j(p_i)$, $p_i \in P$). The rules $\{r_1,r_2,...,r_p\}$define the events to which random vector $\underline{\mathbf{X}}$ refers. The fact that $\underline{\mathbf{X}}$ is *random* simply means that the events generated by application of $\{r_1,r_2,...,r_p\}$ to the members of *P* occur with probability that can be described by a particular density function. Just as importantly, it is the fact that he can provide such extra-statistical recipes for score production that allows the factor analyst to rest easy in the knowledge that each $\mathbf{X}_j$ *uniquely* represents. Simply put, the scores that comprise the distribution of, e.g., $\mathbf{X}_2$, were produced by application of r_2, and *not* of r_1, nor r_3. Does Bartholomew believe that deducing the distribution of $\underline{\mathbf{X}}$ via his factor analytic distributional representation settles such questions?

Now, let us turn to the symbol $\boldsymbol{\theta}$ which also appears in the model equations. In marked contrast to the $\mathbf{X}_j$, the researcher possesses no rule prior to analysis by which θ-scores can be produced. He does not lay down a rule of correspondence linking the symbol $\boldsymbol{\theta}$ to some constituent of natural reality that it is supposed to represent. This is why a factor analysis involves only data that are realizations on $\underline{\mathbf{X}}$. But in the absence of such rules: i) The meaning of the scores that comprise the distribution of $\boldsymbol{\theta}$ is left open to question. That is, it is not the least bit obvious what $\boldsymbol{\theta}$ stands for; ii) Unique reference of *common factor to* $\underline{\boldsymbol{X}}$ is not guaranteed. This concept signifies whatever $\boldsymbol{\theta}$ represents, but what $\boldsymbol{\theta}$ represents is not fixed by antecedently specified rules. Because, to grasp the rules of employment of a concept is to grasp its correct role in claims of signification, the status of the concept *common factor to* $\underline{\boldsymbol{X}}$ in regard issues of signification is not analogous to that of *manifest variate to* $\underline{\boldsymbol{X}}$.

Now, the bolding of the symbol $\boldsymbol{\theta}$ (in Bartholomew's treatment, the use of a capital letter) indicates that *whatever* $\boldsymbol{\theta}$ stands for must be distributed in a population *P*. Hence, with a few specifications, Bartholomew, or anyone else interested in so doing, can work out the distribution (in *P*) of whatever $\boldsymbol{\theta}$ stands for, conditional on $\underline{\mathbf{X}}=\underline{x}$. But being able to do so in no way resolves the fundamental issue of what, if anything, $\boldsymbol{\theta}$ stands for. In fact, in the absence of an antecedently specified rule according to which θ-scores can be produced, hence, in terms of which the meaning of the symbol $\boldsymbol{\theta}$ is settled, the symbol $\boldsymbol{\theta}$ in the equations of a factor model is simply a placeholder for any random variate which satisfies the requirements laid down by the model as sufficient for common factor-hood. And it so happens that, when $\underline{\mathbf{X}}$ is lf representable, these requirements are not sufficient to achieve a cardinality of unity of set *C*. As a result, an infinity of random variates are contained in set *C*, the solution set of common factors to $\underline{\mathbf{X}}$, and $\boldsymbol{\theta}$ stands for any of the elements of set *C* (contrary to what Bartholomew believes, employing the term "solution set" does not in any way commit one to a non-stochastic treatment: random variates can be the elements of a solution set).

Bartholomew's insistence that statistical theory settles the indeterminacy issue in favour of his posterior distribution characterization is mistaken. Statistical theory, a calculus for dealing with stochastic processes, has no more power to settle the meanings of the terms that populate stochastic models, than does calculus have the power to settle the meaning of the symbols in Newton's second law, F = ma. Despite what Bartholomew claims, the authority to which he turns to resolve these issues is *not* the foundations of probability theory, but rather the metaphysics of the Central Account. To put it another way, where in Feller or Loeve does Bartholomew find mention of his favoured terms *underlying*, *unobservable*, (causal) *influence*, and (causal) *determinant*? What he offers is the standard statistical two-step, making disparaging claims about how concern with the indeterminacy property represents a failure to grasp statistical theory, while employing a rich mixture of undefined extra-statistical terminology to sell a pretty picture. His implied statistical sophistication with regard this issue turns out to be nothing more than a deep seated belief that the factor model can be used to detect *the* causal source of the phenomena represented by a set of manifest variates, and, hence, that the concept *latent variate to* $\underline{\boldsymbol{X}}$ signifies just this single thing.

Bartholomew's presupposing of the CA is made clear in his discussion of the "unknown" prior distribution of $\boldsymbol{\theta}$: a) Why does he call this distribution *unknown*? Has he already discovered a particular constituent of natural reality, say, "theta-things", determined that there exists more than one such theta-thing, hence, that there is a population, *P*, of theta-things, thus deduced that theta-things have a distribution, and is now awaiting scientific progress to make known the form of this, currently unknown, distribution? To speak of κ as being unknown, it must be possible to *know* κ. Yet, far from having some class of objects existing in natural reality to which the concept *latent variate to* $\underline{\boldsymbol{X}}$ refers, let alone being in the position of coherently debating the distribution of these objects, Bartholomew doesn't even define what he means by prefered terms such as *unobservable* and *underlying*. It is always possible to allude to the existence in nature of, say, the grek, debate where one would find greks, presuming, of course, that they do exist, even start up a society for the advancement of the scientific study of greks. But unless there exist linguistic rules that fix the correct employment of the concept *grek*, all of this talk must remain nonsense; b) if the prior distribution of $\boldsymbol{\theta}$ is unknown and awaiting discovery, then it certainly is not the case that it "...is essentially arbitrary and...the choice may be made to suit our convenience" (1980, p.295), nor that "Since there is no "natural" scale in such cases we are at liberty to construct one to suit our convenience" (1980, p.296). The interesting feature of Bartholomew's discussion of indeterminacy is that, when cleansed of its CA induced metaphysical commitments, it is very much in keeping with the idea that a factor analysis is an attempt to construct a set of random variates that have certain, particular, optimal relations with a set of (manifest) input variates, a thesis taken up in Part 3 of the current work. And, in fact, Bartholomew spends a great deal of time advising on optimal choices in regard this process of construction. But, of course, if construction is truly the modus operandi of latent variable modeling, then the joint restrictions imposed by the model (equations and distributional specifications), rather than Bartholomew's superstitions, determine the cardinality of the set of constructed random variates that can rightly be called common factors to $\underline{\mathbf{X}}$. Bartholomew has it backwards: It is not the model which serves to "clarify the conceptual basis of the subject" (Bartholomew & Knott, 1999, p.24), it is the conceptual basis of the subject to which model employment must answer. He has chosen to rest the explanation for the terms of his models on the Central Account, and, hence, his account of indeterminacy must stand or fall as the Central Account stands or falls.

Schonemann and Haagen (1987, *Biometrics J.*)

This article shows that, as a result of the indeterminacy property, when $\underline{\mathbf{X}}$ is t-dimensional linear factor representable, there always exists a set of factors to $\underline{\mathbf{X}}$ (t common, and p unique) that will predict perfectly "...any criterion, including the dates of easter sunday..." (p.835). In other words, it illustrates the theory developed in Schonemann and Steiger (1978).

Rozeboom (1988, *British J. of Mathematical and Statistical Psychology*)

This is an important paper, because it appears to be the only consideration of indeterminacy that links the problem to "the ontology and semantics of scientific variates". In particular, the desire for the determinacy of common factors is diagnosed by Rozeboom as a longing for the alleged referent of *common factor to* $\underline{\boldsymbol{X}}$ to be *identified* in a very strong sense of the term, namely, knowledge as to the concept presumed to signify this referent. Thus, Rozeboom states: "...I shall submit that much past distress over factor indeterminacy has been an implicit desire for factors to be *identified* in an epistemic sense much stronger than unique specification, a sense we don't know how to cash out even for data variables." (p.209). But this is a desire that is at the very heart of latent variable modeling, and whose realization is presupposed in the Central Account and, in particular, the practice of "latent variate interpretation".

Rozeboom begins by arguing that the issue of indeterminacy is a much broader one than that which has been of concern in the classical literature on factor indeterminacy. He comments that "Precisely what is meant by describing a multivariate model as "identified" or, contrastingly, as "indeterminate" in some particular application is surprisingly problematic" (p.209). According to Rozeboom, "...we may say that [the common factor model] is (fully) "determinate" in some particular application with side constraints just in case its totality of imposed conditions provides identification of exactly one model solution. But the notion of "identifying" something, model solutions in particular, is obscure" (p.211). In first approximation, he offers that "...to identify an entity *s* is to communicate a name, description, or other denotative phrase that picks out this particular *s* as differentiated from all other things we regard as distinct from *s*. However, not all expressions that refer to the same *s* are equally acceptable as *identifications* thereof. To give quantitative examples, the description "Mean number of acorns colleceted per squirrel in Ohio last October" designates a specific number while leaving us egregiously ignorant as to its identity" (p.211), and that "Identification requires not merely individuating reference, but reference in whatever special way we intuitively require for greatest epistemic illumination" (p.211).

In regard the classical factor indeterminacy issue, "The point is this: On pain of dismissing the past factor-indeterminacy literature as foolish, we surely do not want to say that side conditions on (1) make the model "determinate" whenever they specify a unique solution. For we can always supplement our mathematical constraints by

> Moreover, $\langle \mathbf{A},\mathbf{M}_{FF},F\rangle$ is the particular solution of (1) that most closely aligns *F* with an *m*-tuple of *Z*'s causal sources.(Degree of "alignment" here can be made precise as, say, the *F*-axes' mean correlation with the source variables to which they are respectively matched)...Although our understanding of causality is still primordial...there can be little doubt that any tuple *Z* of data variables does in fact have causal sources which, moreover, comprise just a vanishingly small subset of the variables with which *Z* is jointly distributed" (p.211).

That is, unique reference for concept *common factor to* $\underline{\mathbf{X}}$ could be guaranteed by insisting that it designates *the* variate in *C* that is, in fact, *the* causal source of $\underline{\mathbf{X}}$. As with McDonald and Mulaik, Rozeboom provides no definition of *causal source* as distinct from what the model provides, no argument as to why he believes a *variate* can be a cause, nor why the set *C* must contain the presumed cause of $\underline{\mathbf{X}}$.

However, what Rozeboom claims next is the key point in a consideration of the Central Account. In regard this potential solution to the indeterminacy problem, he states: "Yet this does little to allay traditional *angst* over factor indeterminacy" (p.211). For even if the imposition of such an additional causality rider *did*, in fact, pick out just one element from *C*, this "...neither *identifies* that *F* nor gives any clue to how its identity might be found" (p.211). To paraphrase, even if it were possible, on the basis of a factor analysis, to isolate just one random variate (or one vector of real coefficients, depending on the model) as *the* common factor to $\underline{\mathbf{X}}$, nothing would make evident the concept-name that is presumed to signify the scores that comprise the distribution of this variate. Nor, for that matter, is it obvious that it even makes sense to presume that the variate is signified by a concept. The Central Account, and, certainly, McDonald and Mulaik, claims that when $\underline{\mathbf{X}}$ is lf representable, a property/attribute (causal source) of the phenomena under study has been detected, this property/attribute (causal source) *has* an identity (is signified by an ordinary language concept), and that it is just a matter of making an inference as to this identity. Faith in these presumptions may well be misplaced.

Rozeboom next turns to a general consideration of principles involved in the identification of variables. He states that "To identify any particular solution of [the common factor model], we must designate its $\langle \mathbf{A},\mathbf{M}_{FF},F\rangle$ by expressions of whatever canonical forms we have judged to be most useful for dealing with entities of these kinds. Happily, coefficient and moment matrices present no puzzles in this regard, insomuch as intuition insists that the canonical form for identifying a finite array of numbers is listing for each element thereof a symbol in standard numeric notation which designates that number...*But we have no canonical forms of expression for identifying variables, nor any theory of what should go into one*" (p.212). Rozeboom challenges his audience "...to test it yourself by contemplating how, when preparing an empirical research report, you would attempt to identify your study's data variables. Simply publishing your observed score matrix would accomplish little, for that tells nothing about *what* the variables are on which those numbers are scale values. More informative is for you to describe the procedures that elicited this output from your sample subjects in a way that defines how scores on these very same variables are to be obtained for other subjects in whatever population your study is construed to sample. Yet however exhaustively you spell out your procedures-and in practice we seldom manage to say much-it will always be possible to detail them further in conflicting ways..." (p.212). He concludes that "Our failure either to articulate a reasoned methodology for identifying variables-*any* variables- or to establish some praxis of doing this effectively has seriously impeded psychology's development as a hard science...and is undoubtedly the most important of factor indeterminacy's neglected facets" (p.212). This is an important point. It raises the basic question as to how the scores that comprise the distribution of a variate gain their meaning, if, in fact, they are at all "meaningful." Once again, McDonald and Mulaik, for example, believe that the scores that comprise the distribution of a common factor to $\underline{\mathbf{X}}$ are, as it were, inherently meaningful, they being signified by some concept, but, as a result of unobservability, this concept is unknown to the researcher. While this extra-linguistic conceptual signification is promised by the CA, it is far from clear how the notion of non-linguistic signification could possibly make sense.

Rozeboom next offers a technical standard of individuation for variables. He states that "Ontologically, a "variable" over a population P is a contrast-class of properties (attributes, features, characteristics) that are mutually exclusive and jointly exhaustive over P- i.e., any individual that satisfies the conditions for belonging to P necessarily has one and only one property in this class" (pp.212-213). However, "...when a variable is numerically scaled...it defines a function mapping each member of its domain P into a number that represents on this scale that individual's particular property in this contrast-class. Accordingly, we shall stipulate that mathematically, in a sense that philosophers characterize as "extensional", a (numerically scaled, extensional) variable over population P simply *is* a function that maps each member of its domain into one particular number. Then if x and y are both variables over P, they are moreover the *same* variable just in case they are identical as functions, i.e., iff they have the same value for the same argument everywhere in P" (p.213). This definition "...enables us in principle-never mind feasibility in practice-to *identify* extensional variables Z over a population P by numerically listing the Z-defining score matrix in P" (p.213). Finally, "...given attainable knowledge (or suppositions) κ about variables Z and F, notably a solution for all or part of $\langle \mathbf{A}, \mathbf{M}_{FF} \rangle$...we can say that F is (extensionally) "identifiable" from Z given κ whenever, from any numerically identified value $\mathbf{z}$ of Z, we can effectively compute (up to rounding error) a numerically identified score vector $\mathbf{f}$ such that if κ is true, $\mathbf{f}$ is the one and only vector of scores on F compatible for a member of P with score-vector $\mathbf{z}$ on Z. This is a *relative* identifiability of F from Z given κ indifferent to whether we ever in fact numerically identify the values of Z for any P-members" (p.213).

The next part of the paper is an analysis of a variety of brands of indeterminacy that arise in the context of the linear factor model, including that of classical indeterminacy. In the conclusion to the paper, Rozeboom asks what the results imply for multivariate practice. His answer is "not much directly." However, "...it redirects concern for factor indeterminacy from its narrow and-let us be honest- inconsequential classic **AF** focus into a perspective far more consilient with recent multivariate advances" (p.223). His final comments on classical factor score indeterminacy are intriguing: "Unlike the other cases examined here, variety **AM** has no relevance for modeling practice insomuch as we never have use for a determinate choice of factor scores at any stage of model fitting. So why, when we are given $\langle Z, \mathbf{M}_{ZZ} \rangle$ and have identified a distinguished $\mathbf{A}$ and $\mathbf{M}_o$ such that $\mathbf{M}_{ZZ} = \mathbf{A}\mathbf{M}_o\mathbf{A}'$, should we feel disturbed when L-ambiguity of $\mathbf{A}$ admits a multiplicity of F_i for which $\langle Z = \mathbf{A}F_i, \mathbf{M}_{F_iF_i} = \mathbf{M}_o \rangle$? If we simply wished to pick out a specific F_i in this solution-range without much caring which one we get, we could easily close out the indeterminacy by an arbitrary stipulation of E_i...Whereas if some of these F_i seem more selection-worthy than others, it is again straightforward in principle to search out the optimal one if we can devise some computable measure τ on score matrices in the **MF** solution range such that $\tau(F_i)$ appraises the merit of selection F_i...I submit that the real problem here has little if anything to do with **AM**-indeterminacy of factors construed extensionally as number-valued functions on whatever population we take to be at issue. We *do* intuit that some score matrices in the **AM** solution-range are more meritorious than others, yet have little notion of how to distinguish them from their less worthy brethren by a computable τ on $\{F_i\}$. But such a τ would be of little use even if, contrary to all likelihood, we could operationalize it. For what we are seeking here is the factor solution specified without identification by some causal criterion...And what we want to learn is not so much F_i-scores in **AM** solution-range most closely aligned with scores in *P* on causal sources of *Z* as the non-extensional nature of these causal variables- precisely what score matrices fail to tell us about the contrast-classes of properties on which they list numerical scale values. (If you did know scores in *P* on causal sources *F* of *Z*, but nothing else about *F* save statistics entailed by the $<Z,F>$ distribution in *P*, what good would this information do you?)" (p.225). Here, Rozeboom spells it out. Not only does there not exist a causal criterion, distinct from what factor analysis itself offers, by which a single most-meritorious variate could be singled out, but a score-matrix (construction formula for random variates) says nothing about the *identity* of the concept that is presumed, perhaps incorrectly, to signify the scores contained within such a matrix (produced as realizations on these random variates). Nor do the variety of popular interpretational aids based on moments from the joint distribution of $\underline{\mathbf{X}}$ and $\boldsymbol{\theta}$ ("statistics entailed by the $<Z,F>$ distribution in *P*") or the conditional distribution of $\boldsymbol{\theta}$ given $\underline{\mathbf{X}}$. Yet, identification of the concept presumed to signify the scores comprising the distributions of factors to $\underline{\mathbf{X}}$ is precisely what the latent variable modeller desires, and what the CA tells him is possible.

Vittadini (1989, *Multivariate Behavioral Research*)

In this paper, Vittadini proves that LISREL representations possess the same indeterminacy property as linear factor representations. He employs Guttman's ρ^* to quantify the indeterminacy inherent to such representations, and expresses this measure in terms of the eigenvalues of the matrices of the LISREL covariance structure. He also derives formulas for the construction of variates that are latent variates to $\underline{\mathbf{X}}$ under the LISREL model.

Mulaik, Steiger, Schonemann, Bentler (1990, *Multivariate Behavioral Research*)

These articles were part of a commentary on Velicer and Jackson's 1990 *Multivariate Behavioral Research* article, the latter a comparison of factor- and principal component analysis, and contained a number of comments on the topic of indeterminacy, several of which were noted in Chapter V. We now complete the account.

i. Schonemann (1990, p.48). "...by the conventional definition of a *random variable* as a map of a probability space into a real (product) space Re^n (e.g., Feller, 1966, p.4) factors, for example, *g*, are not even random variables. Because the relation from the probability space to *g* is a composite of a many-one relation (from the probability space to the test space) and a many-many relation (from the test space to the factor space), it is many-many and hence not a map."

McDonald (1996) also makes this claim. But the claim is founded on the same misunderstanding that undermines Bartholomew's work, namely that the symbol $\boldsymbol{\theta}$ in the equations of the linear factor model *must* have unique reference. But there exists no justifiable *must*, for a symbol does not settle its own reference. In fact, $\boldsymbol{\theta}$ stands for *each* of the elements of the *set C*, each of these elements being (definitionally) a common factor to $\underline{\mathbf{X}}$ and a random variate, i.e., a "map of a probability space into a real (product) space Re^n".

ii. Mulaik (1990). As was noted in Chapter III, Mulaik's response to Velicer and Jackson contained a clear statement of the CAC. However, Mulaik has thought deeply about the implications of the indeterminacy property, and his articles are rich in ideas. His response to Velicer and Jackson is no exception. And, yet, it will herein be argued that Mulaik misemploys key concepts such as *model*, *theory*, *concept*, and *hypothesis*, and this invalidates much of his commentary on indeterminacy. His greatest mistake is to not treat with care the individual case. His work is painted in large brush strokes, and very often generates illegitimate comparisons, category errors, and conflations. At the root of it all is a failure to grasp the key distinction between a conceptual and an empirical issue (A topic taken up in Chapter XII). The following are examples:

a) "On the other hand, if one believes that the indeterminacy of the common factor model is a fatal flaw to be avoided by using determinate models like component analysis, then one has not come to grips with the pervasive indeterminacy that exists thoughout science. As contemporary philosophers of science, such as Garrison (1986), point out, scientific concepts are not uniquely determined by the data of experience. Science is concerned with generalizing beyond the particulars of experience. But there is no unique way to do this in connection with a given set of particulars. Theoretical physics, for example, is continuously occupied with differing speculations designed to synthesize the same set of diverse experimental data. All of these differing theoretical speculations may yield models that fit equally well the data already at hand, but in time some or all of these speculative models may be eliminated from further consideration by their inconsistency with new data obtained to test certain predictions derived from them." (1990, p.54)

By his opening comment that "if one believes that the indeterminacy of the common factor model is a fatal flaw to be avoided by using determinate models like component analysis, then one has not come to grips with the pervasive indeterminacy that exists thoughout science", it appears that Mulaik wishes to downplay the significance of the indeterminacy property of linear factor representations by equating it with a range of distinct indeterminacies that are endemic to scientific practice. It is as if he is saying, "how can one worry about the little old lady who took $5 from the bingo till, when the world is full of big-time embezzlers." This is unfortunate, because, even when correctly characterized, the consequences of the indeterminacy property of the linear factor model possibly *have* been overplayed. Certainly, there seems to be no point in making blanket assertions about whether or not to discard the model, when the most important issue arising from the indeterminacy issue is the truth or falsity of the Central Account. This issue must be addressed before any recommendations can be made in regard the employment of factor analysis.

But also, Mulaik's likening on the indeterminacy property of linear factor representations to the indeterminacy inherent to various scientific contexts is simply illegitimate. In particular, he provides several examples of "indeterminacy" without noting their profound differences, nor establishing that they have any relevance to the brand of indeterminacy that arises in linear factor representations. His examples also contain ambiguities and category errors. Perhaps his purpose is to obscure the facts. It is true that there exists within science, not to mention psychometrics (see, e.g., Rozeboom, 1988), many distinct brands of indeterminacy, some of a conceptual, and some of an empirical, nature. For example: i) The widely ramifying natures of the grammars of certain psychological concepts, e.g., *dominant*, generates local indeterminacies in the application of these concepts; ii) There is an indeterminacy in regard the solution to the equation 5+x=9; iii) If there is available no basis for prefering theory A to theory B, because each explains equally well the relevant facts as they are currently understood, then one might say that, with respect the available evidence, there exists a theoretical indeterminacy. Each of these is a distinct brand of indeterminacy, with its own special problems, and calling for its own tailored solutions.

Consider three of the examples provided by Mulaik:

> "scientific concepts not being uniquely determined by the data of experience"
>
> "non-uniqueness in generalizing from a given set of particulars to something more general"
>
> "uncertainty over the correctness of a number of distinct explanations of sets of facts already at hand"

Now, the first of Mulaik's examples contains a category error, for a concept is not determined, nonuniquely or otherwise, by the "data of experience". A concept's correct employment is, rather, fixed by linguistic rules laid down by humans. Moreover, if by "data of experience" Mulaik simply means experience, then one does not just "experience", one experiences hot and cold, the pressure of an exam situation, the beauty of a starry night on the prairie, and such experiencing, expressed in terms of concepts, is a gift of language. Such experiences presuppose a linguistic capacity. We can express our having had such experiences because language affords us with the tools to see them *as such*. To experience (at least in a manner that can be articulated) is a gift of language.

In the second of his examples, Mulaik invokes the notion of generalization. A generalization is a proposition about an A, based on knowledge of B, when B is seen as a part, or exemplar of, A. There may well be disagreement over how to generalize from B, because, e.g., nothing about A is entailed by knowledge of B. Whether this disagreement should be viewed as a case of indeterminacy is open to question. What is certain, however, is that issues regarding the making of generalizations rest on a consideration of the relationship between particular *facts* (distinct bodies of knowledge), while the indeterminacy property of the linear factor model does not. The indeterminacy property of linear factor representations is simply the fact that, when a particular $\underline{\mathbf{X}}$ is described by the linear factor model, the concept *common factor to* $\underline{\boldsymbol{X}}$ has an infinity of referents. This latitude in the reference of *common factor to* $\underline{\boldsymbol{X}}$ arises from the definition of the concept *common factor to* $\underline{\boldsymbol{X}}$, not from theoretical or interpretational concerns. The conflating of the latitude that exists in regard the interpretation of a set of facts (an issue that could conceivably be resolved by reference to empirical facts) and the latitude that arises as a result of the formulation of certain definitions (a conceptual issue) is a reoccurring feature of Mulaik's commentary on the indeterminacy issue, and a necessary consequence of his commitment to the Central Account. For his writing betrays a commitment to the view that factor analysis involves the detection of the property common to (cause of) phenomena under study, but that the "facts" possessed by the researcher to not allow him to know with certainty what precisely has been detected.

With regard the third example, while Mulaik is quite correct in his observation that distinct theories can account equally well, in certain senses, for a set of facts, this observation has no relevance to the indeterminacy property of linear factor representations. The indeterminacy property of linear factor representations, the fact that the cardinality of set *C* is infinite, is a result of the definition of the concept *common factor to* $\underline{\mathbf{X}}$ and has nothing to do with there being a multiplicity of theories each of which explains equally well some existing set of facts. The random variates in *C* are not theories, but, rather, answers to a call for variates possessing the properties, as specified by the linear factor model, required for common factor-hood. Mulaik is, here, confusing two very distinct brands of indeterminacy.

b) "Exploratory common factor analysis is at best one among many methods one might use intitially in the course of trying to formulate hypotheses about causal structures underlying the variables of a domain. The common factor model is but a template imposed upon the correlations among a set of variables to see what things would be like were the variation of these variables produced by variation in a set of common causal variables. But resolving the indeterminacy in assinging meaning to the common factors is no different from resolving the indeterminacy that first exists when one tries to see other things in the forms of clouds, tea leaves or Rorschach ink blots or make sense out of a novel situation: if there is to be a meaning seen at all therein, one must project or impose it." (p.55)

"In factor analysis this involves formulating an interpretation for the factors. The interpretation is a hypothesis put forth to account for the values of the factor structure and interfactor correlation coefficients obtained in a factor analysis of the observed variables. One identifies the common and unique factors of the analysis with variables in the world one believes have these same correlations among themselves and with the observed variables. But the indeterminacy of the common factor model means that variables in the world answering to this description are not uniquely determined by this description. So, whatever interpretation one gives to the factors need not be unique. Other researchers may form equally viable interpretations." (p.55).

Now, these quotes contain many ideas, including the interesting idea that a factor analysis is merely a starting point in the search for constituents of natural reality that are causes of the phenomena under study. This latter idea will be addressed in Part II. For now, the several problematic aspects of Mulaik's discussion are noted:

i) If factor analysis were merely a starting point in the search for constituents of natural reality that are causes of the phenomena under study, then there would be no reason for the dependency of latent variable modeling on the Central Account. To put this differently, given that one believes the Central Account, one believes that, when $\underline{\mathbf{X}}$ is linear factor representable, an *unobservable* entity has been detected, and one must turn to making an inference as to the concept that signifies it. Given commitment to such a picture, it would make little sense to then launch a program of research in an attempt to find some real constituent of natural reality that is the cause of the phenomena represented by the manifest variates. The Central Account excuses the researcher from engaging in such real science. Perhaps this is why latent variable modellers do not engage in the extra-factor analytic research Mulaik describes.

ii) There is no such thing as "variables in the world", unless one means by this expression, the variables employed by researchers in their research. But variates are certainly not constituents of natural reality. They are not discovered, but are, rather, created by humans. What is "in the world" are the various constituents of natural reality, including phenomena, entities, forces, etc., some of which are denoted by concepts contained within the various languages created and used by human beings. The identification of variates as objects of scientific investigation is unfortunate, and, as will later be argued, has generated a great deal of confusion. For one constructs a variate, a real valued function, to quantify objects under study with respect a phenomenon of interest. A variate is not a material entity, nor a process, nor a force, nor a phenomenon of any sort. In certain situations the construction process is relatively straight-forward, and in others not. For example, let us say that I am interested in whether "percentage fast-twitch muscle" is associated with "running time for 100m". If I measure the "percentage fast-twitch" and "running time (in seconds) for 100m" for each of one-hundred humans, and let the variate X stand for the former scores, and Y the latter, then I can justifiably view the X-scores as signified by "percentage fast-twitch muscle" and the Y-scores as signified by "running time (in seconds) for 100m". Furthermore, the correlation between the variates X and Y can reasonably be viewed as a quantification of the relationship between percentage fast-twitch muscle and running time (in seconds) for 100m. As long as these representations are reasonable, then I can employ these variates to find out about the phenomena of interest. However, it is the phenomena that are of interest and not the variates per se.

iii) "But there is a way to go on that takes something of value from performing the exploratory factor analysis. One can proceed conditional on a particular interpretation for the factors to formulate testable hypotheses involving the original observed variables and additional variables. For example, one might assert, "*This* variable (perhaps one points to it) in the world stands in the same relationship to the observed variables as does a common factor to the observed variables of this factor analysis." One must identify this putative common factor variable in some way *independently* of the factor analysis one has just performed...One must either come up with (a) some specific measured variable not incuded in the original analysis, or (b) define it as whatever is common to not just the original observed variables but to some other larger set of measureable variables that may include the original observed variables (and one should be fairly specific as to how one identifies this common factor, for example, "By the *g* factor of this analysis I shall provisionally mean *rule-inferring or analytic ability* as defined by Guttman" (p.56)

Mulaik intends the loadings estimated in a factor analysis, say $\underline{\Lambda}_o$, to play the role of criterion for the application of the concept *possible causal source of* $\underline{\boldsymbol{X}}$ in the extra-factor analytic research he envisions. But if the researcher were to come up with another "variable in the world", say variate $\mathbf{Y}$, whose scores are produced in accord with some rule r_y, and the joint distribution of $\underline{\mathbf{X}}$ and $\mathbf{Y}$ happened to have vector of correlations equal to $\underline{\Lambda}_o$, he has not yet generated any causal case in regard the phenomena represented by $\underline{\mathbf{X}}$. What kind of causal case *could* he generated? A material entity can have a causal impact on other material entities, but how does it make sense to say that a variate has had a causal impact on another? Mulaik does not clarify these issues.

iii. Bentler and Kano. This article includes an interesting theorem which proves that, under mild conditions, if a ulcf representation holds as $p \rightarrow \infty$, then the first principal component is asymptotically equivalent to the common factor, and the vector of factor loadings is equivalent to the first eigenvector. This finding will be discussed in detail in Chapter XV.

Lovie and Lovie (1995, *British J. of Mathematical and Statistical Psychology*)

The purpose of this article was to suggest that Steiger and Schonemann (1978) improperly characterized the Wilson-Spearman exchanges on indeterminacy. According to Lovie an Lovie the exchange was not "antagonistic" or "adversarial", but rather of a cooperative nature, it being an example of the "...negotiated nature of science..." (Lovie & Lovie, 1995, p.238). Based on their article, it is doubtful whether Lovie and Lovie (1995) understand the indeterminacy property well enough to offer probate on the history provided by Steiger and Schonemann (1978). An example of their unreadiness is their vague likening of the indeterminacy property to "...the inability of the factor-analytic methods of the day to extract an unambiguous structure from a given set of results" (Lovie & Lovie, 1995, p.237). It is manifestly unclear what they mean by "unambiguous structure". In any case, the case they offer illustrates, once again, that taking a middle-ground position does not necessarily move a discipline closer to understanding.

The dialogue between Wilson and Spearman is characterized by unfailing politeness. Wilson clearly views himself an interested applied mathematician, offering up his skills as an interested applied mathematician, for the betterment of factor theory. However, Spearman's repeated misconstruals of Wilson's insights can hardly be called an example of the "negotiated nature of science." Wilson employs a very dry humour in dealing with Spearman's infelicities, but, at various points in his writing, reveals his frustration over the imprecision inherent to Spearman's statement of his theory (and Kelley's statement of his theory), and to the evasiveness of his responses. Their exchanges are far less about cooperation, and far more about an honest attempt to understand (on the part of Wilson) met by mischaracterization in an effort to protect an infant version of the Central Account (on the part of Spearman).

Maraun (1996, *Multivariate Behavioral Research*) with commentaries by J. Steiger, D. Bartholomew, W. Rozeboom, P. Schonemann, S. Mulaik, and R. McDonald

This was the author's first attempt to articulate certain of the arguments that are the subject matter of *Myths and Confusions: Psychometrics and the latent variable model*.

Haagen and Oberhoffer (1999, *Metron*)

This paper begins with a review of the various identification problems inherent to the linear factor representation of a set of variates. The authors note the well known result that, given that $\underline{\mathbf{X}}$ is ulcf representable, there exist an infinity of factors to $\underline{\mathbf{X}}$, and these can be constructed as per (4.4)-(4.6). They carefully note that the arbitrary component of these constructions is "...not an error of measurement nor an error in variables, but...rather due to the model specification" (1999, p.38). This is precisely the point, in that the recipe that yields these constructions is exactly what is demanded in the linear factor models specification. The authors review Williams' (1978) "solution" to indeterminacy, which involved, not a solution to the problem that existed in the finite variates model, but a new model in which the observed variates are considered to be "...elements of an infinite dimensional space of variables which are determined by a finite number of common factors" (1999, p.38). They also consider Kano's treatment, which involves the addition of m arbitrary equations to bring to full rank the system of equations that relates the p variates to the (p+m) latent variates of the model. They note that Haagen (1991) has argued that such a treatment "...ends up in a tautological definition" (1999, p.38).

The treatment considered by Haagen and Oberhoffer involves a consideration of a new linear factor model in which each object under study can be observed not just a single time, but, rather, T times. Given that each objects vector of common factor scores is constant over replications, and that various other conditions hold, they establish that the indeterminacy will vanish as $T \to \infty$. They mention several ways in which the conditions of the model might be satisfied in practice, including the case in which there can be formed N different collections of T homogeneous objects.