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Coming Full Circle in the History of Factor Indeterminacy

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

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

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

Factor analysis is a popular technology, because it has provided its users and developers with a number of tangible benefits. In the 1940's and 50's, simply performing a factor analysis was often sufficient to obtain a Ph.D. Factor analysis offers a rich field of technical problems, of wide-ranging difficulty, to be mined by researchers. These problems kept a whole generation of psychometricians gainfully employed. Factor analysis was, and remains, a "productive" technology. Factor indeterminacy represented an unwelcome challenge to this technology, and was simply shunted aside by Thurstone (1935, 1947) and his colleagues, who went on to establish a substantial academic empire with factor analysis as its foundation.

Over the years, factor analysts have offered many reassuring responses to indeterminacy. In 1929, Spearman pointed out that, if you had enough indicators for a factor, indeterminacy would still exist, but would be of little substantive concern, since the correlation between a factor and its regression estimate would be close to one. Unfortunately, often as the number of indicators increased, more factors were required to fit the data. Originally, Guttman (1955) had explored the notion of an "infinite domain" of variables fitting a finite number of factors, in order to establish various interesting connections between factor and component theory. Later, this idea was extended in the following way. If you observe p variables, you can always imagine they came from an infinite domain with the same factors. This "infinite domain" model is actually quite different from the p-variate model, and, if taken seriously, raises many difficult substantive questions. These latter questions were tedious to deal with, and, moreover, distracted from the greater glory of the factor analytic enterprise. The more convenient path was simply to use the *possibility* that indeterminacy was no serious problem in practice as a license for ignoring it entirely. Since the vast majority of practitioners only know what they read in textbooks, a collective amnesia, perhaps including ostracism of those who kept recalling the topic (unless they put the proper "positive" spin on things) soon developed.

One sees this type of historical development frequently these days as textbooks and computer software continue to play an important role in "filtering" innovative developments. A small amount of filtration at a key level can have an overwhelming "ripple effect" on statistical practice. (A classic example — Bartlett's test, outperformed by at least a half-dozen alternative statistics, is still the standard test of independence in major general-purpose software packages, and is virtually the only test ever reported.)

In my view, proponents of factor analysis have already been at the crossroads, and taken the wrong path, several times by filtering out some unpleasant theoretical realities. It seems we are, thanks to the vision and flexibility of MBR Editor Stanley Mulaik, at another such crossroads. Once again, these issues have been brought forth and clarified. The correct path seems clear. If we wish to continue using models with more latent than observed variables, we need to discuss and develop methods for the measurement and evaluation of factor indeterminacy, so that the problem is properly controlled. These methods, in turn, need to be implemented in standard statistical software. Some voices seem to be suggesting an alternative path, by subtly suggesting that the problem *need not be severe*, or has been effectively "managed" in the past. Unfortunately, history suggests that such voices will be taken as a license to ignore the problem entirely, and so I respond to them in the pages that follow.

Latent Traits and the Possibility of Being Slightly More Specific

After reading Maraun (1996a), it seems natural to interpret McDonald's (1996) commentary as a concatenation of the *infinite behavior domain* and *scientific usefulness* arguments, mixed with a powerful appeal to orthodoxy and a substantial dose of vagueness. The appeal to orthodoxy is rather blatant. To discard factor analysis, McDonald suggests, would be unthinkable, because to do so would mean calling into question IRT theory and, heaven forbid, ETS and ACT! Having herded the psychometric traditionalists into his corner with this (scare?) tactic, McDonald then regales them with reminiscences about his practical experiences with a (unidimensional?) 95 item Likert scale. He seems to have settled on the view that (a) we should never have taken the p-variate factor model seriously in the first place, since it is really only an approximation to the more elegant and appropriate *infinite domain* position, and (b) since Guttman conjectured "ten or fifteen variables approximate an infinity of them," little harm is done using the p-variate model in practical applications where the ratio of p to m (the number of common factors) is large.

While I find there are substantial commonalities between McDonald's (1996) views and mine, I feel duty-bound to allege that, in an important sense, it seems McDonald is trying to have his cake and eat it, too. Thoroughly reformed from his early misadventures (McDonald, 1974) as a defender of the p-variate model, he still seems, by virtue of what he *doesn't* say, to be casting a sly wink in the direction of the traditionalists, in effect playing to both galleries at once. First, he offers no concrete suggestions about how often the one obtains an "approximate" infinity in factor analytic practice, or how one should assess this notion empirically. Second, he persistently ignores the fact that "factors" in an infinite domain are also components. As a consequence, he ignores the subtantial connection between his own current position ("behavior domain theory cannot be used as a general, vague, thoughtless 'response' to the 'problem' of factor indeterminacy", McDonald, 1996, p. 599) and that advanced by Schönemann and Wang (1972), who argued that indeterminacy indices should be examined by computing indeterminacy indices, *and actually computed them for a number of published studies*. On the other hand, he admits (laudably) that many applications of common factor analysis and structural equation modeling are "misdirected," because they have insufficient indicators (i.e., determinacy indices are poor). When discussing his 95 item (unidimensional?) Likert scale, and the use of 19 and 40 item subsets, he again refers, obliquely, to what amounts to the measurement of factor indeterminacy. It seems McDonald has arrived (without realizing it?) at the view that the factor worth "measuring" is a component (i.e., a factor with an infinite number of indicators), and good measurement requires a low indeterminacy index. This is a view I have supported for a long time.

I'll join McDonald (1996) himself in congratulating him on his flexibility, and add that I too do not think we necessarily have to *discard* factor analysis or IRT theory, or ETS as a consequence of factor indeterminacy. I hasten to add that we cannot afford to ignore factor indeterminacy when pondering either. We need to embrace all the algebraic facts, take a more sophisticated view of what our models actually imply, and discuss, in more detail, how (and how seriously) we test them with data. McDonald nods in the direction of infinite domain theory. Fine — but to stop there is simply a cop-out. As more of an invitation than a criticism, I find myself requesting, "Will you ever be *specific* about your plans for testing and evaluating the infinite domain model?" In other words, if the *infinite domain* model is the one we are really talking about, and is not simply a sophisticated diversionary tactic, we should be talking about testing this theory. It is difficult to see how we can move in this direction when basic philosophical and algebraic issues are never even discussed in textbooks and computer manuals. An issue ignored is an issue unresolved.

A Matter of History

In 1975, Peter Schönemann and I completed our first draft of *A History of Factor Indeterminacy* (Steiger & Schönemann, 1978), and sent it to a few colleagues for comments. The article reviewed the entire history of factor indeterminacy beginning with the now-famous exchanges between E.B. Wilson and Charles Spearman. Between 1928 and 1939, more than a dozen articles by Wilson and Spearman (for references, see Steiger, 1979, and Lovie & Lovie, 1995) had probed the foundations of factor analysis, and non-uniqueness problems were at the heart of the debate. Yet major texts by Harman (1960, 1967), Gorsuch (1974), and others never mentioned this literature. The unanswered question was, and remains, "Why not?"

Spearman and Wilson were undeniably important figures, and in 1975 their exchange was simply missing from all current accounts of the history of factor analysis. Schönemann and I, though hardly naive about the more controversial aspects of our article (Steiger & Schönemann, 1978), hoped that it would fill an obvious void, would quickly become a popular resource for future writers on factor analysis, and would receive an enthusiastic reception. Our optimism was short-lived. The article provoked a substantial amount of controversy, and only surfaced 3 years later (Steiger & Schönemann, 1978; Steiger, 1979), after running an astonishing gauntlet of editorial pruning and ideological prejudice. (As I discuss below, the article is still being criticized, with thinly disguised ad hominems, more for its "implied" motivation than for its actual content.) By the time our questions were allowed to appear, a number of "answers" had already found their way into print.

It is easy to see why some people might be annoyed by *A History of Factor Indeterminacy* (Steiger & Schönemann, 1978). The article was heretical in some places, and asked embarrassing questions, such as: How *could* textbooks such as Harman's (1960, 1967) *Modern Factor Analysis* review the history of factor analysis in the 1920's and 1930's without ever mentioning the Spearman-Wilson exchanges, or, for that matter, factor indeterminacy itself? The question remains a difficult one to answer, although one need not be implying conspiratorial motivations in asking it. For example, a similar question in 1996 might be, "How can writers of allegedly comprehensive textbooks on statistics fail to ever discuss non-robustness of correlational tests?" This is a serious question, deserves a serious answer, and need not imply any conspiracy to protect classic correlational methods.

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. As inflammatory as it seemed in 1975, the article that eventually surfaced as Steiger and Schönemann (1978) seems less so today, which may be some indication of its success. The key messages were rendered very clearly, and have held up remarkably well, as Maraun (1996a, 1996b) has demonstrated:

1. The ASP position was a legitimate one, yet had been ignored, possibly even suppressed, by a generation of writers on factor analysis. The non-uniqueness problems of the factor model were never described clearly in textbooks (with a major exception being Mulaik, 1972).

2. There had been a rich history of controversy about the foundations of factor analysis, marked by a series of exchanges between Charles Spearman and E. B. Wilson beginning in 1928 (see Wilson, 1928). This history was important and interesting, and should have merited at least a paragraph of discussion in any serious history of factor analysis.

3. When the American psychometric group led by Thurstone took over the development of factor analysis, they made a point of never mentioning any of this rich history. This third fact was certainly no accident. Wolfle (1940), writing a historical review of factor analysis, ignored the rich exchanges between Spearman and Wilson, despite the fact that they had occurred very recently and Wolfle obviously knew they occurred because he cited the articles in his references! If, as keepers of the factor analytic flame so often insisted, factor indeterminacy was not a problem at all, and had been "resolved" by Spearman and Wilson, why was its algebra (and the alleged resolution) never presented clearly and unambiguously? The contrast between Thurstone's (1947) prominent treatment of the rotational indeterminacy issue and his failure to even discuss the factor indeterminacy issue was striking.

Although the articles pointed out the obvious mistakes in many of the arguments dismissing indeterminacy, the main conclusion of the article was *not*, as is often reputed, that factor indeterminacy *necessarily* had fatal consequences for factor analysis. Rather, it was that science progresses best when core issues are dealt with directly, rather than being shunted aside in favor of more "productive" busywork. This was stated at the conclusion: "The practical consequences of factor indeterminacy for the modern user are minor, compared with the negative impact the problem has had on the field of psychometrics. (Indeed, Wilson and Worcester, 1939, argued that factor analysis could continue to provide some useful information in the face of indeterminacy.) ... thousands ... were never told about factor indeterminacy ... A science can progress only if its practitioners are willing to confront crucial and difficult theoretical questions head on ..." (Steiger & Schönemann, 1978, p. 174-175).

In retrospect, this conclusion seems fair enough. What negative consequences would have ensued if modern software actually calculated and reported indeterminacy indices, so that researchers only reported determinate and measurable factors? What negative consequences would have resulted if users of factor analysis actually *knew* about the logical problems at its foundations, and made an informed decision with the aid of that knowledge?

Some writers seem to confuse dismissal of a problem with its "management." I believe the distinction is crucial, and is the primary lesson that the history of factor indeterminacy has to teach us. This belief motivates my extended comments in the following sections.

Recasting More than Spearman and Wilson

Recently, we have learned a bit more about the history of factor indeterminacy. To mark the 50th anniversary of the death of Spearman, Lovie and Lovie (1995) published a historical account of the extensive private correspondence on indeterminacy between Charles Spearman and E. B. Wilson. Lovie and Lovie reviewed dozens of letters held in the British Psychological Society and Harvard University archives. Their article fills in some details about the origin of Wilson's interest in indeterminacy, and the way the relationship between the men unfolded, especially in the period from 1927-1933.

The most reasonable evaluation of Lovie and Lovie (1995) would seem to be that it augments, in an interesting but relatively non-mathematical way, the more mathematically-oriented history presented by Steiger and Schönemann (1978). Lovie and Lovie seem determined to raise the status of their article by characterizing it as at conflict with Steiger and Schönemann. They introduce their article with a number of comments and conclusions attacking Schönemann and myself. The comments are negative but strangely evasive, condescending and dismissive yet almost completely nonspecific. Consequently, it is a commentary that is difficult to respond to (as I suppose was its intention). However, since I strongly dispute their fundamental characterization of my work, and since the evidence they present offers only ambiguous support for their conclusions about Spearman and Wilson as well, some response on several points is warranted.

First, there is a curious lack of differentiation. Lovie and Lovie (1995) lump three articles (Steiger, 1979; Steiger & Schönemann, 1978; Schönemann, 1981) together, referring throughout to the work of "Schönemann and Steiger." Yet even a casual reading reveals substantial differences in tone and content between the three articles. For the record, (a) Steiger and Schönemann (1978) was written by the two of us, while I was a student at Purdue under Schönemann's direction; (b) Steiger (1979) was originally co-authored, then re-written by me, under extensive editorializing by the staff of *Psychometrika* (the article appeared in print nearly 3 years after submission), after I left Purdue. Schönemann felt the editorial changes were too extensive for his taste and declined co-authorship credit; (c) Schönemann (1981) was written by Schönemann, with no contribution by me. These differences are clearly reflected in the content of the articles, a fact which seems to have escaped the notice of Lovie and Lovie.

Lovie and Lovie (1995) then offer several rather unflattering juxtappositions of their work and ours. The main point seems to be not to clarify history, but to characterize us as unnecessarily nasty and given to an oversimplified "adversarial" view of scientific relations. They assert:

1. The Schönmann-Steiger treatment of the Wilson-Spearman exchanges is "seamless, unproblematic, and partisan," while Lovie and Lovie (1995) "reveals the usual mixture of misunderstanding, negotiation around entrenched views, and slow and painful change which characterizes all scientific endeavor." (p. 238).

2. "Schönemann and Steiger's work has strongly implied ... that Wilson had demolished [factor analysis] in 1928 by unequivocally demonstrating its fundamental indeterminacy ... We shall argue, contrary to Schönemann and Steiger, that Wilson's main aim was to rescue Spearman from what he considered to be the ill-thought-out consequences of the mathematics ... Thus, on the basis of the extant evidence, the relationship between the two was more cooperative than antagonistic ... the ... episode actually illustrates the socially negotiated nature of science, rather than the more adversarial model implied by Schönemann and Steiger." (p. 238).

The alert reader can see immediately that basic facts are not being debated, only matters of motivation and interpretation. Moreover, most readers are well aware how much important scientific work has been fueled by bitter competition, so the central contention of Lovie and Lovie (1995) seems somehow Pollyanna-ish. Wilson was an academic figure of considerable importance, and he was challenging the foundation of Spearman's lifetime accomplishments.

Responding to the Allegation 1 above is difficult, since the meaning of "seamless and unproblematic" is itself indeterminate in this context. Certainly, we alluded to problems the men discussed, and differences of opinion between them. We quoted some statements that certainly reflected antagonism and entrenched views, as well as a shifting of views by both Spearman and Wilson. If our account seemed "seamless," perhaps it was because (a) we were covering a lengthy history of which the Spearman-Wilson debate was only a small (but important) part, and (b) we were operating under space limitations. The description of my articles as "partisan" is a rather transparent ad hominem. The articles had a point of view, and in that sense all history is "partisan." In my view, Lovie and Lovie (1995) present an account that is as "partisan" as mine.

Point 2 above is an indefensible misreading of our articles, and the authors evidently know it, as they provide no quotes and fall back on the terms "strongly implied" and "implied" at points where one might have expected quotes to appear. Schönemann and I had no knowledge of the personal relations between Spearman and Wilson, made no attempt to characterize the psychodynamics between them, and restricted ourselves to a reasonably accurate interpretation of the way the published record unfolded. With two notable omissions (Spearman, 1931, 1932), one of which was also missed by Lovie and Lovie (1995), I think we succeeded. Moreover, even a casual reading of Steiger and Schönemann (1978) reveals that we had a rather sympathetic view of Spearman's stature as a scientist, and that the key point for us was not *who won* the debate between Spearman and Wilson, but rather that there was a rich history which was subsequently ignored to the point of being lost.

With this in mind, it is difficult to imagine why Lovie and Lovie (1995) might think their enriched account of the Spearman-Wilson exchange contradicts either Steiger and Schönemann (1978) or Steiger (1979), or weakens our case. That the exchange between Wilson and Spearman was far more extensive than we had imagined only deepens the mystery of Wolfle's (1940) omission.

As of this writing, Patricia Lovie, citing copyright considerations, was unwilling to provide me with copies of the Harvard University archive letters she quoted from. However, it appears that the evidence cited in Lovie and Lovie (1995), as well as some information they did not cite, calls into question just how cooperative Spearman and Wilson actually were. Many of the fundamental asssertions of Lovie and Lovie are contradicted by evidence in their own article.

Friends or Adversaries, Or Somewhere In Between?

Lovie and Lovie (1995) criticize "Schönemann and Steiger" for an unnecessarily "adversarial" view of the relationship between Spearman and Wilson. As I pointed out above, this critique is a straw man, as we in fact made no conjectures about the nature of the private relationship between the men. Moreover, the evidence presented by Lovie and Lovie suggests little more than the typical *detente* that often results when two strong academic figures work through a conflict. To transmute it into a friendly relationship is a reinterpretation that is at odds with a number of facts.

To begin with, one must recognize that correspondence between the principals in an academic dispute frequently does not reflect the full range of their emotions toward each other. So one might expect the correspondence between two men as formidable as Spearman and Wilson to reflect a certain diplomacy and muting of hostility. The published articles show plenty of evidence of such diplomacy, typified by quotes in Steiger and Schönemann (1978, p. 147) and Lovie and Lovie (1995, p. 242). Perhaps, then, what appears to be mutual regard might simply be diplomatic posturing.

Lovie and Lovie (1995) try to deflect this potential criticism by noting that Wilson had, in the past, "been merciless in despatching opponents," (footnote 21, p. 244) and argue that, if he had wanted to, he would have been merciless with Spearman. Yet, one page later they detail how, when Spearman tried to entice Wilson into criticizing his enemy Pearson, Wilson demurred. As Lovie and Lovie describe it, "Wilson also hinted strongly, if obliquely, that he would not risk compromising his professional standing by *publicly* denouncing a major figure like Pearson..." (p. 245).

Clearly, then, Wilson had a diplomatic side. There is no mistaking the fact that he was highly aroused intellectually by the discovery of indeterminacy. Consider that, in 1927, Wilson was 48, at the peak of his powers, and that, during the next few years, he would become President of the American Statistical Association, make a fundamental contribution to statistical theory with the Wilson-Hilferty transformation (Wilson & Hilferty, 1931) and produce a number of other important articles. In the midst of this, he dropped everything to pursue factor indeterminacy. As Lovie and Lovie (1995) describe it, a colleague brought him a copy of Spearman's *The Abilities of Man* in late 1927, asking for help with the mathematics. Wilson sensed the mathematical difficulties in the factor model immediately, and got himself invited to dinner when Spearman visited Harvard around December 2, 1927. When Spearman and the assembled guests failed to see his point, Wilson spent the next two days drafting a letter for Spearman. When Spearman reacted with little interest to the letter, (Lovie & Lovie, 1995, p. 241), Wilson sprang into action immediately. Whether he was motivated solely by academic interest in indeterminacy, or whether he reacted partly out of annoyance with Spearman's dismissive attitude is an intriguing question.

In any case, it appears he got himself invited less than two weeks later (on December 16) to review *The Abilities of Man* for *Science*, a journal edited by his friend, James McKeen Cattell. On January 9, 1928, he remarked to a friend that he had done nothing but work on Spearman for two weeks. In other words, he started working around the clock the day after Christmas. Within a few weeks, he had completed, not only the devastating review for *Science,* but also a more sophisticated mathematical treatment which he published in the *Proceedings of the National Academy of Science*. Since Wilson ran this journal, there was probably no chance of rejection.

The picture that emerges is that of Wilson dropping everything to make sure his critique of Spearman will *not* be ignored. It is possible that Wilson was motivated solely by the fascinating intellectual aspects of indeterminacy. In any case, he certainly was energized! Besides writing the articles themselves, Wilson engaged in a flurry of correspondence with other individuals concerning his views on Spearman's work. Perhaps revealingly, Wilson did not write to Spearman to alert him to the impending review. Lovie and Lovie (1995) press the view that indeed Spearman did not even know the review existed until questioned about it by Wishart a month after it appeared. This may not be true. Spearman may have known about the review, and been struggling for a strategy for blunting its impact. If he did not know, it raises the interesting question of why Wilson never sent a copy of the text of the review directly to Spearman. Could it possibly be that Wilson was annoyed by Spearman, or was this simply the custom of the day?

Throughout their account, Lovie and Lovie (1995) describe many incidents where Spearman reacted as a man under attack. Spearman viewed many of his academic relations as adversarial, (e.g., "from Spearman's view, the strength of the enemy had been halved at a single stroke" Lovie & Lovie, 1995, p. 249), bristled strongly at any criticism, often characterized his opponents in very negative terms, and may well have antagonized Wilson. Interestingly, Lovie and Lovie never raise this prospect in their account.

Without a doubt, Wilson emerges from the Lovie and Lovie (1995) account with a substantially superior image to Spearman. However, I think that to characterize Wilson's "main aim" as wanting to "rescue Spearman" is a substantial overstatement. A more reasonable view is that he had several main aims, which certainly changed during the course of the exchanges, but he might well have been energized by more than a desire to "rescue Spearman" in the beginning, and he might well have been motivated by a desire to extricate himself from an unproductive morass by the end. Moreover, as we show in the next section, Spearman, given a very clear and open opportunity to acknowledge Wilson and indeterminacy, snubbed them both instead.

"Managing" Indeterminacy, Then and Now

Lovie and Lovie (1995) end their article on a note which I find genuinely cryptic. Dismissing indeterminacy as a serious problem they say "Indeed, *managed* indeterminacy has proved, in practice, no serious obstacle to factor analysis which has developed and extended with a vigour that has scarcely slackened over the last 60 years." Whether indeterminacy is a serious problem is debatable. But what could Lovie and Lovie possibly mean by "managed indeterminacy?" Indeterminacy has not been "managed" in practice at all. How many factor analysis textbooks have sections on the "evaluation and management" of factor indeterminacy? How many popular factor analysis computer programs print "indices of indeterminacy," and caution users against using "factor scores" for highly indeterminate factors? Clearly, in the sense of dealing with the problem in practice, there has been neither acknowledgement nor "management" of indeterminacy. Are Lovie and Lovie confused about the vital distinction between "managing" an issue and ignoring it, or is their final sentence some kind of insider's joke?

History provides some suggestive evidence of how factor indeterminacy has been "managed" in the public relations sense. For example, Wolfle (1940), Thurstone (1947), Harman (1960, 1967) and many others "managed" to avoid any detailed discussion of it.

Because of the many details revealed by Lovie and Lovie (1995), it has become increasingly clear that the first effective "manager" of factor indeterminacy (in the public relations sense) turns out to have been Spearman himself. Correspondence reveals, to a much greater extent than the published record, that throughout the Spearman-Wilson exchanges, Spearman seemed primarily concerned with deflecting criticism. Ultimately, he "managed" indeterminacy the same way Thurstone and his followers did — by failing to mention it in an important source. To see this, one need look no further than the second printing of *The Abilities of Man*. This second printing was released in 1932, long after Spearman was well aware of indeterminacy. On Spearman (1932), page vii, there is a "NOTE TO SECOND IMPRESSION." In this note, Spearman reviews important developments since the book first appeared in 1927. Spearman says, "Besides...indications of the utility of the book, a few lines may be said about its validity. How far has the evidence obtained in the short period since its original publication tended to verify this? We seem entitled to answer, In an extraordinary degree."

Spearman (1932) first dismisses some unnamed critics as having represented his work "in an untrue manner." He then goes on, on page viii, to praise a number of individuals whose "objective" criticism of his work has led to enhanced understanding. First, he praises Thorndike, noting "our school and that of Professor Thorndike, formerly regarded as antitheses to each other, have now entered into cordial collaboration." Next, he praises D.W. Brown, noting how "mutual misunderstanding has been converted into mutual appreciation."

Spearman (1932) reserves special praise for "yet another class of critics. It consists of those who for long years judiciously suspended their judgment awaiting patiently and impartially until the evidence should accumulate enough to warrant a definite decision. Such men are the real arbiters of science." This sounds like a perfect description of E.B. Wilson, does it not?

Spearman (1932) goes on, "Outstanding instances in the present case have been Dr. Myers and Professor Nunn," and cites work published in 1930-31 by these men. Wilson, however, is not cited.

Summing it all up, Spearman (1932) concludes "the changes needed in this impression would seem reducible to the following few. A revision of the paragraph on verbal group factors...The addition of a more effective procedure for measuring specific factors (p. xviii). And — with shame be it said — the correction of very numerous misprints."

Wilson is never mentioned. The revised printing made no mention of indeterminacy *anywhere*. Clearly, it would have been a simple matter to add a footnote to the relevant section of the mathematical appendix. The omission is both striking and revealing.

Where Do We Go From Here?

Maraun (1996a, 1996b) has demonstrated how many of the defenses of the p-variate common factor model were simply misguided. History has revealed how, on several occasions, sophisticated rationales have been advanced, and applauded, that suggest that the p-variate model is not *really* the factor model we are testing when we do "factor analysis," rather we are really testing a more sophisticated model implying sampling from a "behavior domain." On the other hand, history has also revealed that this more sophisticated model seems to fade into the background once critics of factor analysis have been silenced.

Once again, we seem to have come full circle in the history of factor indeterminacy. The question is, are we going to insist that the "infinite domain" model be thoroughly and carefully defined and tested, or are we going to allow it to be used merely as an excuse to ignore indeterminacy? At the very least, are we going to insist that reputable textbooks have clear discussions of factor indeterminacy, and that computer programs provide determinacy indices for "factors?" Or are we going to repeat the errors of the past?

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