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I have not read the paper in detail, as clearly as my opinion at present is unfavorable. (Reviewer, British Journal of Mathematical and Statistical Psychology, March 23, 1977).

Shortly after Wilson (1928) had brought the issue of factor indeterminacy to light 70 years ago, it became entangled with ideological and commercial interests which effectively immunized it against further rational argument. In (Steiger & Schönenmann, 1978) we reviewed the resulting bizarre history of this issue.

At present, there seems to be a resurgence of interest in the conceptual history of factor analysis. Within a few weeks, I received three related papers: (a) Maraun’s (1996a) target article; (b) a paper by Blinkhorn (1995) which focuses on Burt’s and Garnett’s roles in the early history of factor analysis but ignores the entire literature on factor indeterminacy; and (c) a review with a new slant of the history of factor indeterminacy by Lovie and Lovie (1995). While Maraun makes some effort to discuss technical issues in technical terms, the British authors still seem bound by the programmatic advice an editor of the British Journal of Mathematical and Statistical Psychology issued two decades ago:

It seems to me that far from cutting down your paper, perhaps down to the algebra as you more-or-less suggested in a recent letter, almost the opposite is required: less algebra, more analysis of the ways in which the issues have current importance (Levy, editorial communication, 1978).

In this spirit, Blinkhorn (1995) cautions his readers that the early literature contains “a good deal of tedious algebra” (p. 22), suggesting that he views this as a defect best to be avoided. Similarly, after describing “the ploy of adding extra tests as a way of reducing the indeterminacy of g [which] can be attributed to several players, although one of them (Wilson) was less convinced of its efficacy”, Lovie and Lovie (1995, p. 251) pointedly ignore Mulaik and McDonald (1978), who demonstrated convincingly the futility of this ploy, albeit by recourse to tedious algebra.
Some commentators, Maraun (1996b) included, wrestled with the question whether factors qualify as random variables. Maraun takes me to task for suggesting that factors may not be random variables, in a technical sense, because the relation from the test space (of dimension p, say) to the factor space (which by definition of the factor model has at least dimensions $p + 1 > p$) is not many-one and hence not a map, so that the composite relation from the probability space to the factor space cannot be a map either. Maraun (1996b) replies: “Schönemann (1996) too seems to forget that indeterminacy implies an infinity of possible maps from the test space to the factor space” (p. 605).

The problem I have with this solution is that an “infinity of maps” is not a map, but a set of maps. “Random variables”, on the other hand, are usually defined by a map, not by a set of maps, for example, in Feller (1950): “A function defined on a sample space is called a random variable” (p. 199; emphasis mine). Similarly, in Parzen (1962) we read: “An object X is said to be a random variable if (i) it is a real finite valued function defined on a sample description space ...” (p. 9).

This insistence on a map (function) may not be as gratuitous as it seems but rather intended to preclude precisely the kind of semantic muddle psychologists find themselves in with their indeterminate “random variables” (or, Maraun’s, 1996b, random variables defined by sets of maps). Similarly, I am also uncomfortable with the notion of “estimating” values on indeterminate random variables, such as factor scores: When statisticians discuss the linear model, they go to some length to spell out the constraints under which the expected value of the data vector and the design matrix define the regression weight vector uniquely. Only if these conditions are met do they call it “estimable”.

Thus, while I cannot get too exercised about such semantic problems, the early history of statistics, at least, may contain some salutory lessons in common sense for psychologists.

Turning now to my peer commentators, I gather both Bartholomew’s (1996) posterior moment position and Williams’ (1978) elimination method have already gone out of fashion. This neither surprises nor saddens me. I always thought of them primarily as stop gap measures designed for editors in need of excuses for rejecting papers containing tedious algebra, rather than as serious efforts to learn more about the implications of factor indeterminacy.

I was more intrigued by Professor McDonald’s (1996) contribution, because it promises to lend coherence to several abrupt changes in his previous arguments which, nevertheless, always converged on the same immutable conclusion that factor indeterminacy is nothing to worry about. From Guttman (1975/1994, p. 368) one learns that initially McDonald argued that the issue is “trivial”. This would have explained why it was
ignored for so long after Wilson had raised it in 1928 (For a dissenting view, cf., Schönenmann, 1978). However, McDonald soon replaced this early argument with the observation that the extent of the indeterminacy can be reduced by simply using a different index for measuring it (McDonald, 1974). From his first round commentary (McDonald, 1996, p. 593) we now learn that “as early as McDonald (1977) [he] abandoned the argument in Section 4 of McDonald (1974) against the legitimacy of Guttman’s interpretation of Kestelman’s alternative solutions, and specifically against his measure of indeterminacy”. The latest link in this chain of reasoning is his reintroduction of a powerful syllogism that can be traced back to Wechsler (1939):

If Spearman-Thurstone factor analysis should be “discarded for lack of determinacy”, we must also discard structural equation models with latent variables (obviously), and virtually all of classical test theory, — for example, classical true score theory, internal consistency reliability, including Cronbach’s alpha, and most of validity theory, since these developments rest explicitly or implicitly on the common factor model ... we cannot [even] save IRT, and it becomes necessary to close E.T.S., A.C.T., and all facilities whose work depends on item banking, test equating, computer adaptive testing, etcetera

It seems that almost the whole of the theory of psychological tests and measurements gets mortally bitten by the ASP (Alternative Solutions Positions). So what is wrong?” (McDonald, 1996, p. 597).

Formally, this argument against the ASP has the same structure as Wechsler’s (1939) celebrated

Proof of the Existence of $g$:

As to the existence of “$g$” there seems to be no possibility of doubt. Psychometrics, without it, loses its basic prop” (p. 8).

The syllogism has the general form:

| Major premise: | P implies Q |
| Minor premise: | Q is too horrible to contemplate |
| Conclusion: | P must be false |

While reminiscent of Pascal’s wager about the existence of God, it is actually more powerful. For example, it can be used to prove the existence of Easter Bunny:

As to the existence of Easter Bunny there seems to be no possibility of doubt. The candy industry, without it, loses its basic prop.
References


