

## MAXCOV Pseudotaxonicity

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In commenting on Meehl (April 1995), Miller (May 1996) is correct, of course, that nonlinearity can theoretically produce a MAXCOV “peak” in a nontaxonic latent structure. My generalization about flat nontaxonic graphs holds well for Monte Carlo runs, where the individual indicator triads  $x_i$ ,  $y_i$ , and  $z_i$  are generated by the classical (Thurstone) psychometric model, assigning random Gaussian deviates on a latent quantitative factor and multiplying by factor loadings that are constant over the factor range. I hope less mathematically astute readers assumed this, given my unqualified use of the Pearson  $r$  and the term *factorial*, but I should have stated it explicitly (as was done in the cited MAMBAC article [Meehl & Yonce, 1994]). I am grateful to Miller for the public clarification; the inventor of a procedure should not wait for it to get a black eye from misapplication in empirical contexts where necessary auxiliary conjectures are violated. One is left with the following empirical questions: How large must these nontaxonic effects be to mislead, and how common are such effects in the real world of psychopathology?

The MAXCOV-HITMAX procedure involves a moderator effect (causal imputations and latent structure aside), as it asks whether the “output” indicator relation  $cov_{yz}$  or  $r_{yz}$  is a statistical function of “input” indicator  $x$ . This purely descriptive relation can sometimes be formally attributable to nonlinearity (Lubinski & Humphreys, 1990) and yields a cusp at the high end, rather than a maximum (“hump”) in the midregion. Another “peak”-generator, despite linearity of all three regressions, could be a marked regional heteroscedasticity, the increased variances of  $y$  and  $z$  in an  $x$ -region yielding larger  $yz$  correlation. Fortunately, an investigator worried about these nontaxonic possibilities can subject them to direct statistical test.

Miller (1996) believes that a single MAXCOV hump is “virtually meaningless.” (I opine that is too strong, but here is not a convenient forum to debate it.) Fortunately, one is never confined to a single graph. Using three or more quantitative indicators, as I prefer, we have at least three MAXCOV graphs to look at. Can a latent unimodal factor  $\nu$  (no taxa, but the factor loadings change over the  $\nu$ -range) produce three large, clear maxima? No, it cannot. A large value of the  $yz$  covariance in a small  $x$ -region (we use .25 *SD* interval widths in our Monte Carlo runs) requires large  $\nu$  factor loadings in the hump region on output indicators  $x$  and  $y$ . When one then takes  $y$  as input, the large loading greatly constricts the latent range  $\Delta\nu$ , preventing a large  $xz$  covariance as output, and similarly for the  $cov_{xy}$  graph using  $z$  as input. For maxima small enough to escape this numerical contradiction—a parametric problem awaiting investigation—we must rely mainly on consistency tests, as Miller says. When only one quantitative indicator (usually psychometric) is available, pairs of scale items can be used as output to yield  $\binom{n}{2}$  graphs,

although I agree with Miller that the dichotomous indicator variant of MAXCOV needs further study. Because an algebraic bound is set on  $\phi_{ij}$  by differing splits on the marginals ( $p_i \neq p_j$ ), could an empirical correlation between dispersion of item difficulties and

difficulty level (determined by input variable location) be a dangerous source of artefactual taxonic hump or cusp? If so, is the  $\phi$  coefficient or its numerator ( $p_{ij} - p_i p_j$ ) safer? This important question awaits analytic, Monte Carlo, and empirical investigation.

I also invoke the logicians' total evidence rule. There have been forty years of research since Ghiselli (1956) and Saunders (1956) called attention to moderator variables, and it turns out that quantitative (nontaxonic) moderators are extremely hard to come by, not robust, and of small magnitude. "It is possible that moderators are as fragile and elusive as that other will-o-the-wisp, the suppressor variable" (Ghiselli, 1972, p. 278). In the present problem, there are the further strange requirements that (except for cusps) a strong moderator effect be nonmonotone and similarly located on each indicator. Apart from multiple consistency tests, here I would optimistically rely on Einstein's dictum, "Raffiniert ist der Herrgott, aber boshaft ist er nicht." Less theologically stated as, Nature is not trying to make a Dutch book against us.

I appreciate Miller's (1996) concern about random fluctuations in a (single) graph being misinterpretable on mere inspection, but inventing an adequate algorithm for taxon-detection is easier said than done. As base rates move from  $P = .50$  to  $P = .10$ , the MAXCOV graph (see Meehl, 1995, Figure 6) will be best fitted by different polynomials, and the change is continuous. I conjecture that routinely fitting a quartic and specifying joint ranges for the four coefficients will work but investigating this has low priority since multiple-graph panels are inspectionally quite unmistakable, despite one or two individual curves showing deviant hills and valleys (cf. graphs in Meehl & Yonce, 1996). Ninety panels were sorted perfectly by three psychologists and two nonpsychologists, a total of 450 errorless judgments. I also take comfort from the extent to which astronomers, physicists, chemists, and geologists appraise clear-cut quantitative relations by inspection.

After several weeks of E-mail exchanges, Miller and I seem to be in substantial agreement on the main issue, although one statement in Miller's (1996) comment puzzles me. He writes, "The supportive argument for MAXCOV with continuous variables is based not on mathematics, but on how awkward it is to explain consistent MAXCOV results without reference to taxa" (p. 555). Of course. If intended as indicating a defect in taxometrics, contrasting it unfavorably with other psychostatistical methods, this sentence is an epistemological mistake. All strict deductive inference in empirical science concerning theoretical entities is in the theory-to-fact direction, never conversely, as logicians and philosophers of science agree. Typically, irreversibility in the formalism is apparent because variables and parameters occur in the mathematical statement of the theoretical postulates that do not occur in the observational statements. For example, in the MAXCOV formalism the base rate parameter  $P$  does not appear in any of the statistics. A factor loading  $a_{ij}$  or a communality  $h_i^2$  is not a number found explicitly in a correlation matrix; when obtained by operations on the latter, the algorithm rests on the postulated factor equation, a substantive conjecture. The theoretical terms of an interpretive text (Meehl, 1990a, p. 109; 1990b, pp. 3-5) cannot be explicitly defined in observational terms, as they have "surplus meaning" (Feigl, 1950; MacCorquodale & Meehl, 1948; Reichenbach, 1938). In rare situations where the mathematical derivation chain is reversible, it is always because a theoretical frame ("implicitly defining" the theoretical constructs) is being surreptitiously presupposed. Miller's "awkwardness" of competing interpretations is simply the philosopher's "inference to best explanation," the

scientist's policy of not attributing empirical orderliness to what philosopher Wesley Salmon calls "a strange coincidence" (cf. Meehl, 1990a). There is nothing special about taxometrics in this respect.

My position is as follows: (a) Mathematically, deviations from the classical psychometric model can produce a single pseudotaxonic MAXCOV graph but not panels of several clearly taxonic graphs that also satisfy multiple consistency tests; (b) the quantitative impact of (a) needs analytical and Monte Carlo investigation, especially as to latent configurations where the consistency tests are insufficiently discriminating; and (c) the frequency of such "dangerous" latent structures in the parameter space of research domains is an empirical question, to which neither Miller, nor I, nor anyone else yet knows the answer. Given the epistemic situation (a)–(b)–(c), one's subjective probability will depend partly on the Bayesian prior assigned to the existence of genuine taxa in psychopathology.

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