Beware the Illusion of Technique[1]

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Although the technique of corresponding regressions proposed and illustrated by Chambers might very well constitute an important and highly useful methodological development, this commentary draws attention to the fact that explanation is, finally, a theoretical endeavor. Thus, the absence (up to now) of the technique of corresponding regressions cannot properly be viewed as responsible for the heretofore prevailing bias among psychological researchers in favor of material- and efficient-cause explanations, nor can future applications of the technique be expected, in and of themselves, to dissolve that bias. Methodological advances do not — and should not — settle theoretical issues. To hold otherwise is to fall prey to that very theory-method confound Chambers himself — quite properly — deplores.

By certain lights, the development by Chambers (1991, this issue) of what he terms the method of corresponding regressions is a rather impressive accomplishment. Through a careful consideration of the nature of correlational research evidence, and of the nature of the knowledge embedded therein, he has discerned that under various circumstances where causal relationships in fact exist between factors brought under correlational investigation, certain patterns (most importantly the *asymmetry of variances* within corresponding mid-ranges of the variables, and the resulting *polarization of correlations* across the ranges of a true dependent variable but not across the ranges of true independent variables) can logically be expected to obtain in certain segments of the data. At one level, my impression of Chambers' insights in this regard is altogether favorable. It is clearly the work of one who is altogether serious, uncommonly thorough and meticulous, appropriately — and equally uncommonly — modest in his claims. Viewed this way, his contribution may be seen as a workmanlike attempt to interest others in the possibilities afforded

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[1]My apologies to William Barrett, who used this phrase to entitle his marvelous book published in 1979 in Garden City, New York, by Anchor Press/Doubleday.

by the sort of analysis he recommends, and as an invitation to others to join him in the effort to further refine the approach and gain answers to questions that are left hanging. All of this I can only enthusiastically applaud, and for these reasons alone I regard Chambers' article as worthy of readers' attention.

I do, however, have serious reservations about Chambers' inclination to regard his methodological contribution as a breakthrough by means of which investigators would now be able to discern formal — and final — cause relationships in empirical findings where, heretofore, only more mechanistic (i.e., material and/or efficient) causes could be seen. It is this issue on which I will train the rest of my remarks.

Citing Rychlak (1981), Chambers notes on the first page of his article that "empirical researchers tend to confound theoretical models of causation with methods of interferring independent variables (IV) and dependent variables (DV)" (p. 49). Though I understand and fully appreciate the point toward which Chambers is driving here, there is an unfortunate vagueness of terminology which, in my view, misdirects the discussion right from the start.

In the first place, correlational research does not, strictly speaking, involve independent and dependent variables. The former refer to variables the levels of which are manipulated/determined by the investigator — i.e., *independent* of what the subjects do. Correlational research, by its very nature, involves no such manipulation. In fact, correlational methodology is typically implemented *because* practical and/or ethical considerations preclude such manipulation. Under such circumstances, that which would otherwise be regarded as an independent variable is designated the *predictor*, and that which would otherwise be regarded as a dependent variable is designated the *criterion*.

Nor is the point here merely pedantic, for I think that this terminological confusion has led directly to Chambers' rather curious allusion to "empirical researchers . . . *inferring* independent variables and dependent variables" (p. 49). Since an independent variable is one designated for manipulation by the experimenter, it is difficult for me to imagine how an investogator could ever find him/herself in the position of having to *infer* same. The investigator *knows* — no need to infer — which variable is the independent variable because the latter *is not* an independent variable until the investigator *makes* it one. The same holds, if only by default, for the dependent variable.

Perhaps what Chambers intended to say in the passage quoted above is that in correlational research, the investigator is often in the position of inferring which of two correlated variables is *cause* and which is *effect*. But while discussion of "cause" and "effect" is properly regarded as *theoretical* discourse, discussion of independent and dependent variables falls into the domain of *methodological* discourse. Thus, by alluding to the latter when he (must have) meant the former, Chambers himself confounds theory and method, and thus

falls prey to the very danger about which Rychlak warns in (among other places) the publication Chambers cites.

Confusion is only deepened in the very next paragraph of Chambers' article, which opens with a reference not to the *confound* but to the *mismatch* that has occurred between theory and method. The use of this sort of language betrays the view that there are certain methods of research design and statistical analysis of data that "go with" or are suited to the search for certain types (e.g., material/efficient) of causal relations, and other such methods that "go with" or are suited to the search for other types (e.g., formal/final) of causation. This is a theory-method *confound* (not "mismatch") *par excellence*. While the design of empirical investigations and the statistical analysis of data are *methodological* endeavors, causal explanation is, finally, a *theoretical* matter. To suggest, therefore, that one requires certain principles of research design and/or statistical analysis techniques *in order to* detect certain kinds of causal relations is to blur the theory-method distinction and in turn to perpetuate the very confusion Rychlak has sought to eradicate.

Significantly, Chambers' own article offers a clear illustration of the point being here developed. In the later sections of his paper, Chambers describes two illustrative applications of the method of corresponding regressions to data drawn from physical science experiments. His point, of course, was to show that his method of corresponding regressions works as it should when the problem is to determine the causal determinants of certain physical phenomena (the level of water in a beaker in one experiment; the cycles to failure in silver-zinc batteries in the other). The findings reported by Chambers indicate that his method did indeed work as it should have, and this is all to the good *as testament to the utility of the method*. Surely, however, Chambers is not suggesting that his findings *require* us to view the determinants of water levels in beakers, or cycles to failure in silver-zinc batteries, in formal/final cause terms. Just as surely, therefore, Chambers must finally recognize that there is nothing at all in the implementation of the method of corresponding regressions — or, for that matter, any other data analysis technique — that settles the question of how one's results are to be causally interpreted. That is, and as Chambers' own examples clearly show, the use of the method of corresponding regressions does not compel us to inject formal/final cause notions into our theoretical account of the results. Nor, it should be stressed, could the failure to employ the method of corresponding regressions prevent an investigator from formulating explanations grounded in formal and/or final cause notions (see, e.g., Rychlak, 1988, where a wealth of experimental findings, none of which were generated by means of the method of corresponding regressions, have nevertheless been theoretically interpreted in formal/final cause terms). Precisely because explanation is a theoretical matter, the question of which of the Aristotelian causal concepts to invoke in any

given explanation exists entirely apart from, and remains wholly unsettled by, the particular method(s) used to generate the empirical findings to be explained.[2]

It is undoubtedly true, as Chambers (following Rychlak) implies, that mainstream scientific psychology has long been dominated by the view that those explanations are best which are couched in material- and/or efficient-cause terms. It is also true that striking parallels can be drawn between this highly mechanistic theoretical outlook and the mechanics of research design and statistical analysis. I think it a grave mistake, however, to view the latter as somehow a "cause" of the former. (Incidentally, what kind of cause would it be?) If prevailing methodological practices have not served to dislodge mechanistic theoretical outlooks, that is simply because there is nothing in the former that would empower them to do that job. Methodological practices are not *supposed* to dislodge — or embed — *any* particular theoretical outlook. To appreciate what is meant by what Rychlak has termed the theory-method confound is to appreciate just this point; that to allow one's choice from among alternative theoretical constructions of the outcome of experiments or other empirical investigations to be dictated by method is to *violate* the canons of sound scientific practice. In accepting this, however, one must also accept that no purely technical advance in the domain of data analysis could possibly impel or even empower us to formulate causal explanations of a sort heretofore impossible. That is, no methodological advancement could, in and of itself, force an investigator committed to material-efficient-cause theoretical interpretations of empirical findings to abandon same, and no investigator committed to formal-final-cause theoretical interpretations of empirical findings requires the method of corresponding regressions — or any other technical device — to justify that view.

None of this gainsays Chambers' efforts as a potentially important *methodological* contribution. His insights might well one day lead to greater sophistication, discipline-wide, in the conduct of correlational research, and that possibility is hardly something to scoff at. I think it of signal importance,

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[2]Yet another example of this point is provided by Chambers himself. On pp. 62–63, where Chambers is discussing the genetic disinheritance phenomenon, he notes that it "need not be a passive process, driven primarily by the limited capacity of a system to incorporate information. A person could actually choose to break a pattern." Now, of course, Chambers is correct in this assertion; a person *could* choose to break a pattern, and hence genetic inheritance *need* not be a passive process. The point to be stressed here is that it *could* be a passive process, and there is nothing in the method of corresponding regressions — whether used in combination with path analysis or not — that will settle this matter for us one way or another. The method of corresponding regressions might help us to *detect* in the data a break of the sort that would result from genetic disinheritance, but detecting a phenomenon and *explaining* it are two different things. It is in the latter context that causal concepts are invoked, and it is just this context that data analysis techniques do not penetrate. Causal explanation is a theoretical matter, and must be dealt with as such regardless of the method used to analyze the data.

however, to view a *methodological* contribution as *just that,* and to resist the temptation to see in any such contribution a breakthrough in the domain of causal explanation. Let us ever beware the illusion of technique.

References

Barrett, W. (1979). *The illusion of technique.* Garden City, New York: Anchor Press/Doubleday.

Chambers, W.V. (1991). Inferring formal causation from corresponding regressions. *Journal of Mind and Behavior, 12,* 49–70.

Rychlak, J.F. (1981). *A philosophy of science for personality theory* (2nd edition). Malabar, Florida: Krieger Publishing Company, Inc.

Rychlak, J.F. (1981). *The psychology of rigorous humanism* (2nd edition). New York: New York University Press.