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Untangling Cause, Necessity, Temporality, and Method: Response to Chambers' Method of Corresponding Regressions

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This paper argues that while Chambers' method of corresponding regressions offers an intriguing way of analyzing empirical data much remains to be done to make the mathematical, and thus, the statistical meaning of the procedure clear and intuitive. Chambers' theoretical justification of the method and the claim that it can in some sense validate formal cause explanations as alternatives to efficient cause, mechanistic ones is rejected. Chambers has misattributed the mechanistic cast of most contemporary psychological explanations to linear temporality rather than to necessity, and has preserved such necessity in the quality of asymmetry. The paper seeks to distinguish and clarify temporality, causality, and necessity in order to be more clear about the central theoretical problem Chambers identifies. It is further argued that the current theoretical issues facing the discipline likely cannot be resolved by methodological advances.

William Chambers' paper, "Inferring Formal Causation from Corresponding Regressions" (1991, this issue) is well done and quite thought-provoking. There is certainly justification for pursuing the insights offered in the paper. The proposed statistical procedure holds promise for extracting new information from data sets, and makes possible the formulating and answering of new questions. What is needed at this point, and what I still find lacking in the paper – and Chambers himself acknowledges this – is a discussion of the method of corresponding regressions that makes obvious just what sort of information the method extracts from data, and just what that information implies about the relationships among the variables captured in the data.

Our commonly used statistics such as correlation coefficients (*r*'s), *t*'s, and *F*'s have an intuitive appeal because the sorts of information in data that give rise to them, their way of extracting that information mathematically, and thus the meaning they convey about data sets can be made quite clear with

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a modicum of conceptual effort and a close examination of the calculations on which they are based. This same sort of intuitive justification for the use and meaning of corresponding correlations and regressions will, I expect, be necessary before use of the technique becames widespread. I am hopeful that this rationale will be forthcoming.

While I find myself in sympathy with the theoretical and philosophical point of view Chambers presents as justification for the importance of his statistical method, I also find his analysis unsatisfactory and conceptually lacking at a number of points. I will concentrate my remarks on these points. My concern is that Chambers fails to make clear the nature of certain concepts that are very important to his arguments. His justification for his method, and thus the contribution he perceives it to make, are based on misconceptions about the nature of causes, causal necessity, temporality, and the relation of methods to theoretical explanations. I should emphasize that while I feel Chambers' theoretical justification for his own methods fails, the methods are interesting and potentially interesting on other grounds, and I am thus overall supportive of his effort.

Causes, Temporality, and the Problem of Necessity

Chambers devotes a considerable part of his paper to a refutation of efficient cause explanations in psychology, suggesting that although they are irrefutably the dominant explanations, they are inadequate because they preclude human agency, and thus fail as accounts for much of human activity. While I share his concern, I believe Chambers miscasts the fundamental problem with efficient causality. Chambers quite accurately points out that some sort of linear temporality is implicit in any efficient cause account of behavior. He mistakenly assumes, however, that this temporality, or "sequentializing" of human action is the essential component of the mechanistic cast of efficient cause accounts.

Mechanism in contemporary efficient cause accounts of behavior arises from assumptions of Aristotlean *necessity* (the notion that a state or event cannot be otherwise than it is), rather than from assumptions of temporal sequence. Efficient cause theories tend to become mechanistic not because of their inherent temporality, but because of an added assumpton of necessity made by theorists. For example, the fact that my anger toward my son always precedes his getting angry with me, does not necessarily imply any mechanistic forces at work in our relationship, nor does it even imply an efficient cause. Formal cause explanations (or, indeed, any others) can be offered for behaviors which are temporally sequential. Indeed, since all human behavior is temporal (see Faulconer and Williams, 1985), any adequate theory of human behavior must deal with temporality.

Temporal sequencing is not the problematic element in efficient cause accounts. In fact, modern structuralist theories are but one manifestation of accounts which retain necessity and mechanism, without temporality. Chambers seems to be moving toward such a structuralist position in suggesting that "structural asymmetries" are essential for causality. It is true that such asymmetries are essential for necessity, and for classical Humean causality. Chambers seeks to defend such asymmetry. In fact, it is central to the method of corresponding regressions. However, the contradiction in his position is that he tries to remedy mechanism – as he sees it in efficient cause explanations – by relying on a model which preserves the essential element of mechanism, i.e., necessity, as manifest in causal asymmetry, thus preserving Humean causality, and inviting mechanistic explanations. Chambers contends that asymmetry is necessary for formal causality. It is only necessary for a formal causality that wants to preserve all the essential features of Humean causality, and thus retain a mechanistic and anti-telic cast.

The Relation of Methods to Causes and Theories

The major problem Chambers attempts to address with his method of corresponding regressions concerns the pervading mechanism and reliance on efficient causality found in contemporary psychological theories. He sees quite accurately that the development of telic theories, those which invoke formal and final causes, has lagged seriously behind that of material and efficient cause accounts. Chambers attributes this lag, mistakenly, I believe, to a lack of statistical methods that are able to somehow illuminate formal causality. This attribution belies a sort of naive realism. Chambers seems to imply that causes are somehow "out there," waiting to be discovered, or uncovered (another manifestation of the structuralist flavor of his work), and that special methods are required to accomplish this.

The alternative position, which I find more compelling, holds that causes have no such ontological status – their only existence is in the explanations a theorist may offer. In other words, causes have their being only in explanatory language, and never in the world we study, in data sets derived from our measurements of that world, nor in the methods with which we choose to examine the data. What is needed from this latter perspective then, is not a new method of analyzing data, but a more sophisticated, or at least more telic way of talking about them. A philosophical problem – psychologists' explanatory predilections – cannot be remedied by statistical methods. At best a statistical method, such as corresponding regressions, might facilitate formal and final cause language. Chambers, however, has misattributed the problem when he suggests that our research methods are responsible for material and efficient cause theories. (See Rychlak, 1981, for an excellent treatment of the independence of theory and method.)

Chambers suggests that current research methods are based on mechanistic assumptions. If this is true at all, it is only so in a purely empirical sense, i.e., most psychologists who design studies also think mechanistically. There is nothing inherent in our current research methods, however, which demands or even presupposes the validity of any mechanistic assumptions. Statistical methods are especially innocent of mechanistic assumptions. All statistical methods look only (and innocently) at patterns of numerical relationship, demanding no commitment to any sort of causal account.

If one defines, as has Chambers, efficient causality purely in terms of temporal sequencing, then there is some validity to the claim that so-called experimental research designs are different from so-called correlational research designs. Experimental designs are marked by an actual manipulation of events at one point in time (T1) and a measurement of subsequent events (T2) manifest in the dependent variable. Since such designs employ a sequence of events and try to attribute causality, they are manifestly "efficient causal" in form. Correlational designs, because they involve no such necessary sequencing of events can be more readily defended as "formal causal."

Further analysis makes it clear, however, that the advantage generally afforded to experimental designs is not that they allow one to make simple causal statements (since formal and final cause accounts can be offered to account for experimental results), but that they allow one to make Humean causal statements (i.e., statements of necessity). It is not the temporal sequencing of events that makes such statements possible – although this is a necessary component – but, rather the control over extraneous, potentially influential variables. Such control is not necessarily unique to experimental as opposed to correlational designs, but simply more common.

Chambers' contention that experimental designs are somehow inherently efficient causal, or that they somehow lead us to efficient cause theories seems unfounded, because he bases this assessment on the temporal features of the designs, and overlooks the issue of control. Since ultimate control is not possible, mechanistic, deterministic, efficient cause accounts are never necessitated by experimental designs. Since experimental designs simply set out conditions under which observations will be made, and because the temporal sequencing of events does not necessarily lead to mechanistic efficient causal accounts, I feel Chambers' contention is insupportable, and instead it simply perpetuates a misunderstanding of traditional design issues.

Certainly statistical designs have no necessary connection to any theoretical or causal account. All statistical methods are based, rather, on what may be referred to as "formal cause" aspects of the data and research process. Statistical methods are methods for examining patterns of relationship among the numbers that constitute a data set. They do not rely on linear time to establish nor recognize such a relationship. And in fact they say nothing about

material causes, energies, necessity, nor purpose. If there is an inhibitory effect on the development of formal and final cause theories based on statistical models and designs, it is surely in the mind of researchers, and based on a misunderstanding of causality. Such an effect is not inherent in the nature of the world or the statistical models we use to study it. Certainly the introduction of a new statistical design cannot remedy the problem because the problem runs much deeper.

Problems with the Model of Corresponding Regressions

The method of corresponding regressions is based on asymmetry in relationships among variables. As I suggested earlier, however, this asymmetry is an essential assumption of traditional Humean causality, mechanism, and most species of efficient cause determinism. It seems unlikely that a method which must invoke asymmetry will deal a very effective blow to the sort of mechanistic account Chambers decries.

Further evidence that Chambers' account of asymmetry is problematic to his own purposes comes from his demonstrations of the method of corresponding regressions based on contrived data sets. Chambers constructs data sets which have built into them the property of asymmetry. Because the relations between the numbers of the data sets are entirely mathematically determined, the property of necessity is also built into them. In other words, because both asymmetry and necessity are built into the same data sets, whatever the analysis of the data may reveal can as well be attributed to mechanistic, "efficient-cause-like" necessity, as to non-mechanistic, "formal-cause-like" causality. Chambers obviously prefers the latter explanation, but his data do not support it, nor does conceptual analysis make the distinction clear.

When we try to apply Chambers' notion of asymmetry to social psychological, or interpersonal events it shows itself to be problematic in yet another sense. In order for two behavioral events to be properly asymmetrical, it must be the case that a change in one variable (X) causes a change in another variable (Y), but the first variable (X) is not affected by the second (Y). When we try to apply this criterion to an actual interpersonal situation, we encounter difficulty. Most interpersonal events have effects on both participants. If, for example, my anger toward my son (X) changes, such that it causes him to be angry (Y), it seems unrealistic to claim that his anger, in turn, will not influence my current or future anger. It would seem that his anger would have no effect on my anger only if his anger had not yet occurred. In other words, the relation of asymmetricality is possible only when there is temporal sequencing of the behavioral events. But Chambers rejects temporal sequencing as leading inexorably to mechanistic accounts. It seems problematic, then, that it should be necessary to account for asymmetricality. What may

conceptually "work" for remotely associated events (accounting for them by capitalizing on asymmetry) seems not to "work" for more immediately associated social events, unless we import back into the account of the events the sort of temporality Chambers rejects.

One additional observation is in order about the examples Chambers gives to illustrate the utility of the method of corresponding regressions. Chambers acknowledges that his examples do not establish the validity of the method. This is obviously so because of the logic involved in arguing from example. What we can conclude from Chambers' paper is that if certain mathematical relations do in fact exist in a data set, then the method of corresponding regressions will yield certain results. However, if on the basis of finding such results, after application of the method, we argue that this certain mathematical relationship must therefore exist in the data, then we have affirmed the consequent of the argument. Evidence from example will never validate the procedure.

A more subtle problem is that the only way to validate the method is to find an actual case of the sort of asymmetrical formal causality Chambers is trying to identify in order to test the model. There is, however, no independent means of identifying such a test case. If there were, the statistical model would be unnecessary. This line of argument means, as I suggested earlier, that there is no alternative to making the meaning of the method of corresponding regressions intuitively obvious for researchers who may use it. Even when this has been accomplished, the method will not be able to achieve the loftiest task Chambers has envisioned for it – the distinguishing of various sorts of causality. This latter task is still a philosophical/theoretical one, and will ever escape methodological attempts to deal with it. I am hopeful that Chambers' method is conceptually justified and well accepted for the contribution it may make on other, methodological grounds.

References

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

Faulconer, J.W., and Williams, R.N. (1985). Temporality in human action: An alternative to positivism and historicism. *American Psychologist, 42*, 1179–1188.

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