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Corresponding Regressions, Procedural Evidence, and the Dialectics of Substantive Theory, Metaphysics, and Methodology

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A defense of the method of corresponding regressions was presented. The confounding of formal cause metaphysics with efficient cause methodology was discussed and a rationale for a formal cause methodology was presented. Time-series simulations were used to illustrate the primacy of structural tautologies over temporal transformations. Conclusions supported the use of corresponding regressions as a means of inferring formal causality.

Lamiell's and Williams' responses (this issue) to my paper concerning corresponding regressions (this issue) were disappointing in that I had hoped to debate either the mathematical validity of the method or the merits of various technical refinements. Instead, my response will be largely a justification of the impact of method on substantive theories and a defense of the relevance of procedural evidence to empirical methodology.

Response to Lamiell

Rychlak (1988) defines two principal methods by which scientific theories have advanced. These include the method of coherence, which is based largely on mathematical and logical procedures that produce what is known as procedural evidence and by the method of correspondence, based primarily on observation. These methods are used to establish an equilibrium in the dialectical assimilation and accommodation of substantive theories, metaphysics, and methodologies to one another. The method of coherence is essentially an assessment of the logical consistency of the fundamental postulates and

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corollaries of a theory. There are numerous methods for assessing logical consistency. The merits, limitations, or evils of syllogisms, ad hominem arguments, equivocation and many other techniques have been argued. Similar efforts have arisen to facilitate the assessment of correspondences between substantive theories and phenomena by observational or empirical methods. The growth of science is rooted in the development of both procedural and empirical methods. It should be recognized, however, that procedural methods are incorporated directly in the development of empirical methods, since there is a logic to each method of observation.

Historically, there has been disagreement concerning the impact of procedural evidence on empirical methods. Some have argued that the scientist can simply "know" by observation and manipulation (Locke). Lamiell aligns himself with this group when he says

> The investigator knows – no need to infer – which variable is the independent variable because the latter isn't an independent variable until the investigator makes it one (through manipulation). (p. 72)

In opposition to these "empiricists," others (Kant) suggested that observations were really constructions and that, indeed, even manipulations are based on constructions that are supported by various forms of procedural evidence. The differences between Lamiell's perspective and my own perspective revolve about the role of procedural evidence in observation. These differences lead to radically different assumptions concerning the advance of scientific theory, just as they have across the history of science.

From the Kantian perspective, science concerns the adequacy of substantive theories as explanations of phenomena. Observation is necessary to validate substantive theory, while metaphysical theories are required to define the terms of both substantive and methodological theories. Furthermore, methodological and substantive theories keep metaphysical theories relevant to the phenomena addressed by each of the theories. In each of these interactions procedural methods concerning logical consistency are necessary. As a group, substantive theory, metaphysics and methodology work in a dynamic, dialectical process that channelizes scientific growth. When one of the members of this triad is inconsistent with the other, a state of disequilibrium emerges and adjustments are necessary for the dialectic to return to a dynamic balance. Sometimes substantive theory is due for revision, sometimes metaphysics require improvement and sometimes advancements in methodology are necessary. Advances in procedural evidence play a central role in concerting this group, since procedural evidence is used in the generation of substantive theories, metaphysics and empirical methodology. As a procedural method, the method of corresponding regressions is thus potentially one means, although not necessarily the only means, for the synthesis of formal

cause metaphysics with empirical methods. Bringing substantive theory into the new equilibrium would be a logical next step. Therefore, I disagree with the spirit of Lamiell's comment that to

> . . . allow one's choice from among alternative theoretical constructions of the outcome of experiments or other empirical investigation 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 empower us to formulate causal explanations of a sort heretofore impossible. (p. 74)

Lamiell is wrong because science requires accountability from substantive theories. If one method increases the parsimony of our substantive theories without doing violence to defensible metaphysics, then let the theory change. If this seems absurd, just think about where the biological sciences would be without the microscope – next time you visit your physician. Then the legitimacy of methods as equal partners in the scientific endeavor will seem a little less philistine.

In his commentary Lamiell suggests that I betray some hidden belief that certain methods are better suited for certain theories. For the record, let me make it very clear that I do believe this to be the case. If a method cannot reflect the metaphysical assumptions incorporated in the substantive theory, then data from the method will lead to the degeneration of the theory. There is probably no better example of how a mismatch between metaphysics and methodology can have a negative impact on substantive theory than the case of the equivocation surrounding the term "variable." To appreciate this issue one must first appreciate Rychlak's (1988) insight into the "double-duty" that mathematics plays in science and philosophy.

Mathematics plays a very important role in the development of procedural methods because math can be both a metaphysical and a methodological endeavor. As such, mathematics can be a valuable middle ground between the insights of metaphysics and the applications of empirical methods. As a metaphysical pursuit, mathematics reflects insights into the nature of highly abstract constructs, principles and proofs that are organized by the mathematician's reasoning "for the sake of" a set of axioms. This essentially introspective endeavor expresses and reveals the metaphysical nature of mathematical abstractions. On the other hand, mathematics tends to be learned by students in a purely extraspective fashion. When this occurs there is a tendency to ignore the metaphysical assumptions of math and to naively apply mathematical techniques in the service of demonstrative pursuits. There is nothing wrong with using mathematics for demonstrative purposes so long as the assumptions built into the math are respected. Unfortunately, the naive application of mathematical terms beyond the self-contained proofs and axioms of introspective math leads the extraspective reasoner to confuse metaphysical terms with phenomena to which the terms are applied. Having little insight

into the metaphysical basis of the mathematical terms, the student tends to base the meaning of the terms on observations. The consequent equivocation in terminology amounts to "putting the cart before the horse" and the rationalization required to give an appearance of coherence and correspondence leads to degeneration of the substantive theory. Something like this has happened in the use of the term "variable," which began as a mathematical term and is now used primarily as a term of empirical methodology.

To mathematicians a "variable" is capable of possessing many values across its range but there is nothing in the definition of "variable" to require the assumption that these values change. Indeed, the concept of "variable" does not require any reference to time or manipulation. Because a variable can be a vector (as in the simulations of the method of corresponding regressions) rather than a scalar, the values across the range of a variable do not have to change to be different from one another. With mathematical variables, the only thing that changes is the focus of attention of the mathematician. Thus the definition of a mathematical variable (metaphysics) becomes confounded with a method of observation (change in attention) when the mathematician assumes differences across a variable are based on changes. The very same confounding of method and metaphysics can occur when the empiricist claims to observe a variable change. There may or may not be changes occurring, since the observations may simply reflect differences across the array of a timeless variable.

Lamiell's comments simply add fuel to the confusion. In fact, he directly contributes to the confounding of method and metaphysics when he says that we only know that something is an independent variable by manipulating it. In extraspective fashion, Lamiell fails to understand that the terms independent variable and dependent variable are rooted in the metaphysical assumptions of introspective mathematics and not in assumptions of experimental methodology. He also fails to appreciate that these roots are directly grounded in the notion of a function, which has direct relevance to the rationale of corresponding regressions. Rychlak (1988) pointed out that mathematicians utilized the notion of deterministic relations between motionless variables when defining the nature of functions.

> Further evidence of the confusion of (formal-cause) theory with (efficient-cause) method can be seen in the altered interpretation of the statistical "function" that has taken place in our time. (p. 49)

Rychlak goes on to point out that the notion of function is a formal cause metaphysical term originating with Leibniz and that

> . . . when Dirichlet subsequently enlarged on the meanings of an independent and a dependent variable in this functional relationship, he in no way departed from formal- and final-cause phrasing. (Rychlak, 1988, p. 49)

Furthermore, Rychlak argues that

> When this mathematical conception (of a function) was analogized to the machinelike sequence of validation, in which an experimenter does lay his hands on an antecedent "variable" to (efficiently) cause a predicted effect on a consequent "variable" over time, the confusion between formal- and efficient-cause terminology was furthered. (1988, p. 50)

Thus Rychlak links the confounding of metaphysical theory and methodological theory with the mismatch between a formal-cause method of procedural evidence (mathematics) and an efficient-cause method of empirical evidence (manipulation). Lamiell's assumption that the inference of independent and dependent variable status is based on manipulation betrays his own misunderstanding of the theory/method confound. *The confusion rests in inappropriate correspondences, not simply in the attempt to draw a correspondence between theory and method.* Lamiell has confounded methaphysics and method and he has done this by an equivocation of the terminology that rightfully belongs to formal cause procedural methodology.

Historically, the equivocation of the terminology of functions, independent variables and dependent variables has rested in the need to make theories empirically accountable. However, since the theory of functions, as a formal-cause endeavor, does not solve the problem of asymmetrical relations at the procedural level, it cannot do so at the empirical level either. Nor can the use of manipulation as an empirical method solve the problem of functional ambiguity in the realm of formal causes. If, however, a procedural method could be developed that resolves the problem of the ambiguity of functions, without violating the metaphysical nature of functions, then there is no reason that a better match between mathematics and empirical methodology could not develop. What I have tried to do in developing the method of corresponding regressions is to provide just such a procedural method with the hope that it will match formal cause metaphysics with observations. Such a match can only facilitate the quest for correspondence between observations and substantive theories.

Response to Williams

Williams' objections (this issue) to the method of corresponding regressions primarily concern the notions of time and necessity. In his address of these issues Williams has misunderstood the potential of the method of corresponding regressions on several points, although I acknowledge that this is partially due to my failure to adequately elaborate my assumptions.

When Williams speaks of necessity he fails to explain what he means. From the perspective of the theory of corresponding regressions, necessity is based on tautology. Tautology is a purely logical concept of equivalence by defini-

tion. As a procedural method, the method of corresponding regressions does not rely on any particular sample (as suggested by Williams), although parameters of samples do influence the outcomes, not the rationale, of the method. In my simulations of the tautological relations between independent and dependent variables, I defined these variables as metaphysical entities, not as phenomena of observation. As such, I developed my arguments in the context of the kind of closed system required to speak of proofs. My arguments, therefore, come much closer to "proof" than do the manipulative demonstrations developed by efficient cause experimenters.

In contrast to the tautologically based inferences of the method of corresponding regressions, the inference of efficient causation has been based upon randomization and manipulation. The statistics used in efficient-cause inference do not incorporate proofs that facilitate formal-cause inference. Traditional statistics do not resolve the ambiguity of functions. Traditional statistics, in fact, tend to obfuscate the actual metaphysical basis of manipulative experimental inferences since people tend to be overly impressed by the complexity of the extraspective mathematics. The real metaphysical basis of manipulative inference is the assumption that previous variables may cause subsequent variables, but not vice versa, and that observed variables have precedence over unobserved variables in explanation. Randomization is used to render implausible the unobserved variables and statistical means are used to reflect differences in values of the presumed dependent variable following manipulation. Derivation of formal logical or mathematical proof of the validity of these efficient cause metaphysical assumptions would be very difficult, if not impossible. This is because the observations, from which these metaphysical assumptions arise, are not constructed in the context of a closed system. Arguing proof from such observations, in fact, leads to the logical fallacy of "affirming the consequent."

When Williams says that I am not presenting proof of the validity of my method, he fails to see that I am "grounding" necessity in tautology. My proof is procedural, not scientific. From a scientific perspective, one cannot prove anything absolutely. Science is a dialectical process and since the days of Aristotle the limitations of dialectics for generating absolute certainty have been understood. Unlike syllogisms, there are no proofs for dialectics. Scientific inference incorporates the proofs of procedural evidence but because science is an attempt to synthesize the products of fundamentally different epistemological perspectives – substantive theory, metaphysics and methodology – scientific inference is ultimately only the process of developing a reasonable faith in their synthesis. Consequently, we build models and apply them with the understanding that drawing correspondences between theories and phenomena is never a matter of proof. If a scientist argues that observation of an empirical event proves his or her theory, then the scientist is af-

firming the consequent. A mathematician, however, may provide proofs of mathematical assertions, but these proofs are strictly a concern of tautological relations. The simulations and logical rationale that I presented concerning the tautologies of formal cause variables come very close to procedural proof. All that is lacking is an algebraic expression to summarize the tautologies.

Let us now return to William's criticisms of my construction of time and its relationship to manipulation. Williams has failed to distinguish between time as a metaphysical construct and time as a methodological convenience. When I question the primacy of time I do not dismiss the importance of time as a metaphysical or substantive dimension. I just believe there is a formal-cause structure to the events that occur within and across time. Furthermore, as a structuralist, I believe formal causation undergirds efficient causation and that structures, while basic to all temporal transformations, do not themselves require manipulation for their existence. A chair would be a chair even if it had always existed. This, however, is not to deny the existence of efficient causation as a hybrid of formal causation.

My belief that formal cause asymmetries undergird efficient-cause asymmetries runs directly counter to Williams. According to Williams (this issue)

> . . . the relation of asymmetricality is possible only when there is temporal sequencing of behavioral events. (p. 81)

Williams' defends his point by reference to an exchange of emotions between a father and son. The father's anger ($x1$) changes the son's emotion ($y1$) to anger, which in turn changes the father's emotion to another emotion ($y2$). This billiard ball-like time-series divides the interaction into a series of discrete units that somehow push one another along. What is ignored in this time-series, however, are the structural interfaces that occur between and within each unit. The father's anger ($x1$) plus the son's feeling of innocence ($x2$) defines a relationship (y) in the moment. This relationship is based on a compounding of the formal structures of each person's own constructions and that person's constructions of the other person's behavior. These constructions may include memories of previous constructions and behaviors as well as intentions that serve some purpose for the sake of which the person responds (final cause). The dialectic evolves as subsequent structures emerge and are compounded. The formal cause asymmetries of this compounding "time-series" are found in the dependencies of the relationship, which include the constructions and actions of each party construing the corresponding constructions and actions of the other person. However, if each of the constructions of the relationship occurred simultaneously or at a greatly accelerated rate, the formal structure of the dependencies would not necessarily change at all. The compounding of formal causes would be the same, just as $2 = 1 + 1$ regardless of how quickly

the person adds. The upshot is that there is no need to speak of manipulation in this model and time serves only an incidental role as the speed of the compounding of formal causes.

A simulation of a compounding "time-series" will be instructive at this point. In a group of unpublished computer simulations I recently modeled compounding transformations across "time." In the beginning of the sequence there were two independent variables; *x1* and *x2*. These variables were assigned random numbers. The average of these two values defined the value of *y*(*j*). The next step was to treat *y*(*j*) as an independent variable (*x1*) and average it with another random number assigned to variable *x2*. This was done in order to obtain a next dependent value *y*(*k*) which was treated in turn as an *x1* and averaged with another *x2* value to create the third step in the sequence of dependencies. Thus a series of dialectical contrasts and syntheses was modeled, with subsequent syntheses (Y) being dependent on previous constrasts (*x1* and *x2*). Corresponding regressions conducted on the sequence and its lag produced *D* values comparable to those found in the simulations presented in my earlier paper. Subsequent values of *y* appeared to be dependent on "previous" values of *y* and a compounded system of dialectical dependencies was simulated.

In a sense these "time-series" simulations suggest "time" presents a fundamental substantive and metaphysical dimension for formal-cause inferences. Events in certain types of series do seem to incorporate "previous" events in their structure. The previous discussion of the compounding nature of relationships, however, suggests that such compounds do not really rely on time. Compound sequences are not fundamentally temporal, just as 1 and 1 do not make 2 but simply are 2. Even simulated time-series are structurally independent of time. True, I had to manipulate the computer to generate the model. But would the structure of the compounded sequences have changed if I had used a faster computer? Not really, because timing in manipulation is no more essential to the definition of formal cause sequences than is the speed at which a mathematician inspects the values across the array of a variable. What is important are the dependencies that occur between variables by definition – tautologically. These asymmetrical relationships of definition can occur simultaneously or across time without being changed. All that is required is that some variable is a component of another. In other words, it is not an issue of which came first, the chicken or the egg, but whether or not a wing implies a chicken or is implied by a chicken. This "many (parts) in one (whole)" relationship expresses the essence of formal causation and neither manipulation nor time are of the essence.

With a dialectical reversal of procedure we can place even further doubt on the primacy of time as the determining factor in sequences. This procedure was suggested by applying the method of corresponding regressions to music.

A series of notes from Bach's *Preludes* was converted to numbers. The method of corresponding regressions was then conducted on the numbers and their lags. I found that subsequent notes were not dependent on previous notes, but previous notes were dependent on subsequent notes. This is intriguing because the formal cause sequence actually runs counter to the experiential temporal sequence. It is as though the notes flow for the sake of some fundamental premise that has yet to transpire, as though Bach simulated a time-series and dialectically reversed the order to root the present in the future. A listener attending only to the influence of the previous note on the present note, as though these formed a stimulus-response relationship, would miss the aspiration implicit in the structure of the *Preludes* altogether. Perhaps the application of corresponding regressions in psychology could help us avoid making the same mistakes when we study people's constructions.

In making the above arguments I have not proven that people reverse the order of formal sequences and dialectically shape the present for the sake of an axiom or premise in the future. I have provided, however, evidence, both procedural and empirical, to suggest that Bach may have invited us to employ such final cause construction when listening to his *Preludes*. My evidence is based on the nearly "proven" validity of corresponding regressions as a procedural method and on my observations of a small sample of Bach's work. If I must embrace the role of "naive realist" (as Williams' accuses me) to venture such a hypothesis, then I am willing to pay the price. I am not, however, suggesting that all of Bach's music across the time of his life contained final causes. When Williams' labeled me a Humean he implied that I am in search of universal timeless laws. As far as I am concerned, scientific laws do not have be universal – only accurate. Just as Bach composed many types of music, science may address a pluraverse of phenomena, each with its own laws. This plurality may make our heads spin but there are theories, methods and musical preludes to help us concert these complex, compound, and all too often confounded details.

Conclusion

The method of corresponding regressions is a procedural method that can provide a rationale for formal-cause empirical methods. This assertion is supported by the rationale of the method and by the data. The method of corresponding regressions cannot be legitmately dismissed as merely a methodological will-o'-the-wisp. All of us, the substantive, metaphysical and methodological theorists, the "workmen" of procedural and empirical methods, the champions of formal, final, and efficient causes, all of us, are pondering the shadows on Plato's cave. It is true that we can only know with the aid of theory and that theories are sometimes tangled in equivocation, incon-

sistency and ignorence. By conversing in good faith, however, as equals, we may at least keep our various endeavors in balance and avoid the illusions of theoretical and methodological onesidedness.

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