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Can Post-Newtonian Psychologists Find Happiness in a Pre-Paradigm Science?

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This paper is a commentary on the essays by Faulconer (1995), Leahey (1995), Rawling (1995), Slife (1995a, 1995b), Vandenberg (1995), and Williams (1995). Whatever the differences among these essays, they nonetheless share a common concern with the image of science which Newton promulgated. What might be termed the Newtonian meta-paradigm is positivistic, in the contemporary sense. This meta-paradigm has survived the demise of the Newtonian paradigm in physics. Each of the authors in this volume, in turn, is concerned with how to expose, and so liberate, psychology from the grip of this meta-paradigm. I comment briefly on their respective strategies and relative success in doing so.

However else the papers I am considering vary with regard to Newton's legacy for psychology, they share a deep and common concern. That concern is with the image of science which Newton promulgated. Indeed, in his Introduction to this special issue, Brent Slife suggests that most psychologists believe that "the very notion of science is somehow equated with the Newtonian perspective" (Slife, 1995a, p. 5). The notion of science identified as Newtonian is, in turn, positivistic in the contemporary sense.[1] From the standpoint of psychology, then, this Newtonian meta-paradigm has proven more enduring than any of the specifics of his physics.

Leahey, Vandenberg, Rawling, Slife, Williams, and Faulconer labor less, in short, with the particulars of Newtonian science and more with its philosophical heritage. Moreover, a thought animating all the papers — with the inter-

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[1]Indeed, although the term "positivism" originates with Comte in the 19th century, Slife anachronistically terms Newton one as well.

esting exception of Rawling's — concerns how to expose the grip of this meta-paradigm on psychology, and to liberate it by doing so. For, the complaint runs, psychology makes a mistake by attempting to shape its practice solely in terms of the conception of science which represents Newton's bequest.

Why do those psychologists seeking to divest themselves of the unwanted Newtonian legacy find it so difficult, apparently, to do so? Why not fashion a psychology without the assumptions Slife identifies as Newtonian? The problem which emerges from these papers, or so I argue, is that there is, as yet, no theoretical shape for psychology to have if this legacy is renounced. In what follows, I direct my comments primarily to what the collected papers imply regarding directions for a post-Newtonian psychology. What this focus on the papers brings to the fore is why it is so difficult to separate psychology from its troubled Newtonian legacy.

Thomas Leahey's (1995) clear and clever paper recalls I.C. Jarvie's quip regarding the awaited arrival of "true" social science; Jarvie dubbed this the "cargo cult view" of social science. In particular, Leahey explicitly appreciates that a key issue is what gets called *science* on the current intellectual scene.[2] As Leahey notes, the "worst effect of waiting for Newton has been the assumption that Newtonian science is the only kind of science" (Leahey, 1995, p. 18). But, again, although Leahey invokes the hoary shade of Wittgenstein, he offers no specific alternatives to the "Newton-cult" mentality which he clearly deplores. Yet, what results can be claimed on behalf of alternative paradigms? As Kuhn (1970) noted in *Structure*, one characteristic of scientific practice is how scientists cling to even unsatisfactory models until some workable alternative is presented. The story Leahey tells is familiar in this regard. It leaves dissident psychologists, and other social scientists, in a "put up or shut-up" position. Despite the wide consensus that the Newtonian/positivist model yields no concrete results for social scientists, there is no clear competitor. So, the old ways persist.

Brian Vandenberg (1995) provides a concise overview of the development of evolutionary thought and its changing relation to theology. In particular, he focuses on how this line of thought evolves given initial linkages to a theology which is ultimately at odds with what an evolutionary approach implies. Early efforts to study God's plan for the universe by empirical methods ultimately result in the conclusion that the existence of God is an hypothesis that evolution can do without. This result, Vandenberg indicates, is not premeditated or intended; early scientists did not set out to dispel the idea that God's plan is immanent in the workings of science.

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[2]For an aggressive and stimulating attempt to rethink what science is in light of recent challenges to philosophy of science by sociologists of science, see Fuller (1988).

However, Vandenberg's paper ends just where one hopes it might begin. One wishes, that is, that Vandenberg had said more regarding the emergence of developmental psychology from the evolutionary nebula he describes. Just what aspects of developmental theories have clear roots in theology, and, most importantly, to what effect? Unwittingly, perhaps, Vandenberg's account suggests that the eventual estrangement of theology and science is no bad thing. That is, he does not argue that science is impeded, that there is a conceptual loss, by divorcing developmental theories from their theological roots. Science does not demand atheism from its practitioners; perhaps, however, it is not the source of succor to religious beliefs that investigators in various centuries hoped it might be.

Piers Rawling (1995) offers a very interesting — because so very clear — case of "mixed explanation" and "mixed metaphysics" (although Rawling's analysis concerns only the former). The "explanatory mix" concerns the differing status for purposes of explanation with regard to quantitative laws and the causal mechanisms "behind" the quantitatively described regularity. The "metaphysical mix" involves citing both physical and psychological factors as causal or explanatory.[3]

What Rawling, for his part, finds philosophically curious about the explanatory mix is how this relates, on Newtonian principles, to "complete" explanatory stories. The Newtonian causal story requires an explicitly mechanical account. By this standard, Newton's gravity is *not* a fully explicated causal notion. However, it is metaphysically acceptable because its role can be given a precise mathematical characterization with regard to the explanations into which it enters. Thus, as Rawling reads Newton, Newton allows gravity an explanatory role despite its mysterious ontological status (and so its failure to qualify as "causal" knowledge).

Rawling shows how issues arise in classic experiments in signal detection theory which call for a treatment parallel to Newton's treatment of gravity. In the case of the signal detection experiments, the mix of explanations arises due to the fact that both the physical components on which hearing depends and one's subjective states (one's desires to avoid false alarms, etc.) influence the outcomes. Psychological states, in this respect, are like gravity — they are causally efficacious, but we do not know how to specify their status as parts of the furniture of the universe (Bertrand Russell's phrase). The saving grace — what allows the mix to work for purposes of the experiment — is that both elements permit of mathematical modeling. Ontological mysteries may be tolerated, this suggests, by preserving precision.

[3]With regard to this latter point, his example exhibits nicely the implicit tension which Slife, in his introductory comments, manifests by listing both "empiricism" and "dualism" as basic tenets of Newtonian philosophy.

But Rawling's provocative closing remarks suggest that precision is, on his view of psychology, a means and not an end. The end is causal explanation couched in the categories distinctive of psychology (or other) social sciences. Rawling would say, that is, that in psychology one has causal explanations without laws. *Contra* Russell, who would dispense with mechanisms and keep only the quantitative laws, Rawling extends Nancy Cartwright's view into psychology by maintaining that causal explanations are distinct from quantitative laws. The distinction between quantitative laws and causal knowledge clearly holds important implications for promoting psychology as a science without laws. Whether or not the distinction can be made to bear the weight which Rawling's rests on awaits further analysis.

Newton's metaphysics — including his views on time, space, and the structure of nature — become important because these metaphysical notions comprise the elements of, and set the limits for, explanation in a Newtonian system. Hempel provided the canonical positivist account of scientific explanation.[4] The deductive–nomological (or, covering law) explanation remains (again, somewhat anachronistically) alive in the minds of psychologists. It is this model which demands that symmetry of explanation and prediction (event A explains B just in case the presence of A allows one to predict correctly that B will occur).[5] It is this model which undergirds the mechanistic and reductionist explanations of which Slife (1995b) justifiably complains.[6] Whether or not the metaphysical parameters regarding space and time to which Slife objects are Newtonian is not, to my mind, the primary issue. Rather, the notion of scientific explanation is.

Unfortunately, Slife only alludes, in his concluding paragraphs, to possible alternatives — in particular, Heideggerian and hermeneutic ones. Do these suggestions represent a plausible strategy for exorcising psychology of positivism? Recall how the exorcism proceeded in philosophy. It was due, in large part, to a conjurer named Kuhn. Kuhn (1970) moved philosophers' attention away from ahistorical idealizations and back to the details of scientific his-

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[4]Hempel's view is best thought of as one which identifies explanation as a type of deductive argument. The statement to be explained — the *explanandum* — ought to follow, on this account, from a set of statements — the *explanans* — which include a general law as well as a set of initial conditions detailing antecedent facts which, in conjunction with the law, entail the *explanandum*.

[5]For the classic statement of this position, see Hempel and Oppenheim (1965). Hempel (1966) provides a clear and concise formulation of his famous position. Those interested in the fate of this model within philosophy may consult Richard Miller's (1987, chapter one) admirably clear exposition.

[6]What Hempel's model demands, actually, is not so much causal connections as regularities — laws. For a Hempelian, citing the relevant regularity explains. What it is exactly that differentiates a causal connection from a regularity I will not attempt to formulate.

tory and practice. He emphasized, famously, how neither the history nor the practice bore even a passing resemblance to philosophical idealizations. So much the worse, Kuhn concluded, for accounting for the choices of scientists by some philosophical myth of scientific rationality.[7]

This was an observation not just (or even mainly) about the psychology of scientists. The philosophical problem is an inability to *reconstruct* a history of scientific development which shows that the choices made by scientists were the ones which, by philosophical standards, the choices they ought to have made. The paradigm of rational inquiry does not, Kuhn emphasized, proceed according to any known philosophical script. Kuhn told the history of science in an ironic mode — how science proceeds cannot be explained by philosophies of scientific explanation.

There is another important respect in which Kuhn deviated from the previously received view with regard to explanation. The received (i.e., positivist) view held to a unity of method between the natural and social sciences. This is an immediate consequence, of course, of the Newtonian legacy. If all science has a particular form, then, surely social science in general, and psychology in particular, must, *qua* science, follow that form. But here Kuhn demurs. In an infamous passage in *The Structure of Scientific Revolutions*, Kuhn *distinguishes* the natural from the social sciences. Interestingly, this distinction rests not on some point of method; Kuhn does not argue for two conceptions of knowledge, e.g., *Geisteswissenschaften* and *Naturwissenschaften*. What distinguishes one from the other, Kuhn suggests, is the presence of consensus in the case of natural science regarding problems, methods, paradigm solutions, etc. and the absence of consensus on these matters in the social sciences. Kuhn's casual observation inspired a torrent of discussion from social scientists regarding a need to have a paradigm to call their own.[8]

The foregoing suggests that Slife's challenge to the tradition confronts a severe obstacle. To cast aspersions on the Newtonian legacy, at least in its Hempelian form, is common enough. However, rejecting the inherited answers creates only a vacuum. The question remains: What is an explanation? Drop the term "scientific" if you like; invoke the hoary shades of Hegel if you will. The basic issue remains unanswered and unresolved. The great virtue of Hempel et al. is that they provided a marvelously clear, if ultimately unsatisfactory, answer. Discard their answer, and the problem is that *no*

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[7]The contributors to this symposium worry about positivism. An irony here is how this contrasts with current debates within philosophy of science — for a challenge has been posed by sociologists of science who are more impressed by the failures of philosophers to explain scientific practice. For an overview of this debate, see Roth and Barrett (1990).

[8]See, on this point, Gutting (1980). I discuss these matters, with particular emphasis on the "paradigm envy" from which social scientists appear to suffer, in "Who Needs Paradigms" (Roth, 1987, chapter five).

model of explanation is there to replace it. Slife may be right that psychology will ultimately do better by divesting itself of its Newtonian legacy. But without an alternative, how will it explain anything at all?

A concern with the relation of Newton's conception of science to materialism is also at the heart of the paper by Richard Williams (1995). Williams approaches this topic by examining how the Newtonian paradigm challenged the prevailing metaphysics of science. In particular, Williams reviews Newton's vexed, and ultimately unsatisfactory attempt, to reconcile his account of gravity with the predominant corpuscularism of the time. Newton could not do without the postulation of gravity, but neither could he reduce it to — explain it in terms of — material constructs.

Postulating gravity as an unreduced basic struck Newton's contemporaries as an interjection of mysticism in the heart of mathematical dynamics. Likewise, Williams suggests, postulating human actions as without material cause strikes many in psychology as an introduction of mystery into psychological explanations (Williams, 1995, p. 68). The problem with many extant psychological theories — including humanistic psychology, psychoanalysis, and behaviorism — is that past and present are linked by the psychological equivalent of an aether. In philosophical terms, Williams maintains that any appeal to *dispositions* represents the introduction of an unreduced mystery in the realm of materialist metaphysic (pp. 69–70). It is true, moreover, that a materialist, non-circular explication of the notion of a disposition has proven elusive.[9]

Williams has taken up a problem central to, but one which poses a major difficulty for, any theory of explanation. This is the issue of historical explanation, i.e., a plausibly scientific account of what factors in the past are responsible for matters as we find them. In this context, one may readily grant Williams' contention that the influence of the past on the present — psychological "action at a distance" — lacks a satisfactory materialist explication. What might be put in its place? Here Williams is elusive. He proposes to avoid the "corpuscularization" of events by reifying, it seems, the notion of an event. "If events are fundamental rather than things, then the necessity of an aether is obviated in one sense because events are inherently 'in motion' . . . . Events do not need to be acted upon to be put into action, nor do they require a medium though which to move. They *are* motion" (pp. 71–72).

Williams offers the following example of the sort of event-basic explanation he has in view. Consider someone's accent. Speaking with an accent is, presumably, a kind of event, or at least the kind Williams has in mind. What is the accent, and how is it connected with the past? Its connection, Williams maintains, is through the patterning which speakers' from a partic-

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[9]See Quine (1960), "Natural Kinds," for some philosophical reflections on these matters.

ular region instantiate when they speak. Of course, the pattern is learned. It is realized by the relevant position of tongue, jaw, teeth etc. Apart from occasions of speech, the accent does not "exist." Each person's past is, one might say, embedded in that accent.

What is unclear, however, is what role Williams imagines accent and related notions play in psychological explanation. Regarding "accent," he asserts that it "has no existence in the past and no causal connection running from past to present. As an explanation, 'accent' is an entirely empty construct" (p. 72). There is no "thing" — an accent — which has causal efficacy. An accent is manifested in ongoing behavior; that is all there is.

Likewise, Williams argues, dysfunctional behavior is not causally linked via some sustained connection to the past. Therapeutically, this means that there is no "causal chain" in which a therapist need intervene. The proper therapeutic approach, on Williams' view, is to understand that the "traumatic event has its existence wholly and only in the current constituting of it in the behavior of the client. There is nothing (no traumatic event) 'back there' making connection with the present behavior" (p. 73). In short, "the behavior *is* the pathology" (p. 73).

The first point to note is that Williams presents a false dilemma: either the past trauma causes (in some sense as yet unspecified) present abnormal behavior, or the trauma exists only as it is realized in current behavior. There is no obvious inconsistency in holding both of these propositions. For example, suppose someone who was abused as a child suffers from chronic depression. The abnormal behavior has an etiology — *ex nihil nihil fit*. However, how a person reacts in the here and now is the problem. The core issue, from a therapeutic standpoint, is whether examining one's history, or one's understanding of one's history, has any therapeutic value. Williams suggests not.[10]

Williams describes his approach as hermeneutical. But in his concern to deny a determinate ontological status to the past, Williams obscures the distinction between his approach and, for example, behavioral reconditioning. Traditional hermeneutics, in fact, can be charged with reifying the past in just the ways Williams seeks to avoid. [11]

[10] I ignore here standard philosophical problems regarding how to identify and individuate events. Does the sentence, "He buttered the toast slowly, in the bathroom, at midnight," describe one event or three? (The example is Davidson's). If one person says that Jones raised his arm, and another says that Jones signaled to the auctioneer, are they describing the same event or different ones? I do not share Williams' confidence that the notion of an event is clear, and that it can help clarify the issues in question.

[11] In conversation, Williams indicated that his use of "hermeneutic" is more Heideggerian than traditional.

Undoing the past, on a hermeneutical approach, would involve a process quite different from mere retraining. Like a shift in scientific theories, such changes might involve conceptual revolutions as well.[12] Psychological action at a distance is, as Williams rightly insists, a central problem. However, what is unclear with respect to the alternative Williams sketches is how he differentiates his strategy for releasing the grip of past from those he finds unsatisfactory.

A sharper appreciation that an escape from Newton's shadow is not necessarily a flight into the light is provided by James Faulconer. Faulconer anticipates the day when psychology, and its kindred social sciences, will "finally mature into genuine sciences" (Faulconer, 1995, p. 77). Whatever this maturational process involves, however, he is confident that it does not include slavish imitation of the natural sciences. Imitation here, as he correctly notes, appears to have stunted conceptual growth.

Newton' s notion of causation goes wrong for psychology, Faulconer believes, but not for the reasons which troubled Slife. It is possible, Faulconer maintains, to adhere to Newtonian causation "without requiring either mechanism, materialism, or mathematical formalizations" (p. 78). Since mathematical formalization is a hallmark of Newtonian science, how does Faulconer sustain this bold claim?

Faulconer separates mechanism from causation by noting that a mathematical formulation of dynamics does not entail a mechanical (physical) relation among the bodies so described.

> Newton's denial of action at a distance and his reliance on a corpuscular metaphysics link formal causation with matter in motion, but they do not link them essentially. [His] insistence that physics does not rely on an account of physical causes, but on a mathematical description of the forces involved shows that formal causation need not refer to the motions of bodies. (p. 81)

However, Faulconer moves too quickly from the claim that Newtonian causation, so understood, need not be mechanistic in a physicalist sense to the claim that Newtonian causation is not wedded to determinism.

The problematic inference to indeterminism occurs immediately after Faulconer argues for the non-mechanistic reading of Newton's view. In this passage, Faulconer equates non-mechanistic with non-deterministic.

> In being mathematical, the mechanical is formal. Even though the word "mechanical" suggests to us strongly that Newton's physics is a matter of materialistic determination, that suggestion is misleading. Newton's physics is mechanical because it deals with force; . . . [T]o be mechanical is to be a force that is mathematically describable.

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[12]I argue elsewhere that psychoanalysis is best understood on the model of a Kuhnian paradigm shift. See my "Interpretation as Explanation," in Bohman, Hiley, and Shusterman (1991).

> Newton's definition of mechanics requires no necessary commitment to determinism or materialism. (p. 81)

The fact that the term *mechanical* entails only mathematical formalization for Newton does not cut against the determinism of a system. Indeed, the essential commitment to mathematization which Faulconer stresses suggests just the opposite conclusion. For given a mathematically precise dynamics, one can let materialism go and yet retain a clear form of determinism, viz., predictability within exact limits. Determinism does not require specifying a material cause. A system is deterministic just in case, from a state description of the system at any time *t*, one may deduce that state of the system at *t+n* (or *t–n*, for that matter). Predictability, not materialism, is the essence of determinism.

Faulconer attempts to remedy this gap in his argument by distinguishing between "force" and "cause." He imagines that when forces go, determinism goes.[13] In particular, he maintains that "*force* and *cause* are not synonyms and giving an account of causes and effects need not involve giving an account of forces" and "a formal theory of causation need not invoke forces, and its formality need not be the formality of mathematics" (p. 82). Science attends to formal causes, even if the causes are not the forces which dynamics describes. Faulconer's example of an event with a formal cause but which is not mathematically describable is that of a disease.

Faulconer appears to maintain that any behavior that is rule-governed is describable as conforming to a formal cause in his sense. But surely not any rule will do. The natural history of a disease could, for the sake of argument, be said to have a formal cause in this sense. There is a pattern which the disease entity exhibits. However, it seems incorrect to suggest that the formal cause of my driving 55 mph on the highway is the legal rule requiring that I do so. It is incorrect not just because some other factor might well be the cause — my radar detector alerted me to the presence of a speed trap, my car just won't go any faster, etc. Rather, the problem is that as a general explanation, it is false. When the *explanandum* is some observed behavior, it may or may not be relevant to cite, in the *explanans*, some social convention or

[13]I confess that I find Faulconer unclear regarding just what he takes to be central to Newton's conception of cause. On p. 81, he writes that the "essence of the two causes Newton accepts, mechanical and first, is that they are both formal causes; each is a law of science, a formal description of the origins of motion." Faulconer later divorces the key notion of a formal cause from what is only mathematically formalizable. What is central, however, is patterning (p. 84). But then, Faulconer insists, "only the confusion of formality with mathematics allows us to believe that all scientific accounts involve measurement" (p. 85). But how is patterning discerned without measurement? What counts, for Faulconer, as discerning a pattern, if not counting, if not regularity?

other. Whether it is or not, however, how that convention functions is not analogous to how citing a natural law functions in a scientific explanation.[14]

Faulconer also seems to conflate the natural and social senses of law when illustrating the explanatory powers of formal causes. "For example, moral behavior can properly be explained as caused by a moral rule, a formal cause. It is possible to give a scientific account of moral behavior by giving a formal account of that behavior, by referring to a moral rule — a formal law — that accounts for the behavior" (p. 83). But the term *formal* in this quote does not bear the proper relation to the use of that term in the context of the notion of a formal cause. One has a formal cause just in case one has a descriptive regularity of a certain kind. Faulconer conflates descriptive and prescriptive laws, and the result is a confusion regarding what is explanatory.

For Faulconer, a "statement of formal cause . . . would be an abstract description of a phenomenon (for example, a behavior) given in the simplest possible terms" or, again, "when we speak of formal causes, we are speaking of the patterns we use to describes the changes we observe rather than the nature of things themselves" (p. 84). But this refers, in the very lineage that Faulconer traces from Bacon through Newton, to a patterning *qua* natural regularity. Whatever a scientific law is, it is not a prescriptive social convention. A moral law is not a law of science for just this reason; its lawfulness is purely *prescriptive*. Formal causes are descriptive.

Indeed, Faulconer is caught in a dilemma. If moral and social laws are descriptive in the way laws of nature are, then, against his intention, he offers just a deterministic system in another form. But if they are not descriptive in the formal sense ascribed to Newton, they are not scientific laws at all.

What does this imply for psychology? All the papers considered above suggest that psychology has been locked in the thrall of a particular paradigm. The paradigm is *not* that of a specific psychological theory. It is a meta-paradigm regarding what it is to be a science, and what is required to provide a scientific explanation. Vandenberg hints, although he nowhere makes the point explicit, that the essentials of a particular paradigm — for example, the tenet that God exists — change and evolve. For Newton and many later thinkers, God had a necessary place; for Laplace and many others, God is an hypothesis that physical theory can do without. Psychology is not waiting for Newton. For if these authors are right, Newton is still too much with them. But still they wait for the yet to arrive science, and perhaps that is their problem.

[14]I am reminded here of a t-shirt I once saw which pictured a motorcycle policeman with Einstein's head. The t-shirt read: "186,000 miles per second. That's the law."

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