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# Newton, Science, and Causation

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Contrary to common belief, acceptance of Newtonian causation does not commit one to a mechanistic, materialistic, or deterministic understanding of the world. I argue that the Newtonian view can be assimilated to contemporary theoretical alternatives in psychology. This means that, given the Newtonian understanding of causation, it is possible for such alternatives to be scientific — to treat of causes — without requiring either mechanism, materialism, or mathematical formalizations. I argue that we best understand Newtonian causation as formal causation. I do this by discussing the history of Newton's theory of causation and comparing his theory to Bacon's. I also compare Newton's theory of causation to Aristotle's, arguing that when we speak of formal causes we speak of our descriptions rather than the nature of things. We may (or may not) accurately impute various elements of our scientific descriptions to the nature of things, but when we speak of formal causes, we are speaking of the patterns we use to describe the changes we observe rather than the nature of things themselves. Since any science must use such patterns, even alternative psychologies use Newtonian causation — if they are genuinely scientific alternatives. However, mathematics is not the only discipline that offers such patterned explanations. Moral explanations offer an alternate model for causal explanation.

Those, like myself, who argue for alternative theoretical stances in the social sciences often speak casually of Newtonian causation in disparaging terms. We look for something broader and more inclusive, something that will allow psychology and the other social sciences more interpretive latitude and, therefore, something that we believe will allow the social sciences finally to mature into genuine sciences. We object to the fact that psychology has often modelled itself on the physical sciences and, particularly, that it has modelled itself on Newtonian physics (cf. Faulconer and Williams, 1985, p. 1180). But in spite of our objections, it remains true that most mention of causation in social science literature relies on a version of Newtonian causation. Whatever the philosophical and theoretical objections to the

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Newtonian understanding of causation, whatever the alternatives presented to Newtonian causation, psychological research and practice continue to find Newtonian causation useful. To the degree that the philosophical and theoretical discussion of causation has yet to inform research and practice, much work in psychology remains marked by a modern (Newtonian) understanding of causation.

As much as I sometimes would like to believe otherwise, I do not think we can ascribe the social sciences' continued reliance on Newtonian causation merely to naivete. Perhaps it is partially a result of the fact that the Newtonian understanding of causation is very much the contemporary, common sense, understanding of causation. But more than that, it appears that social scientists have difficulty conceiving of psychology as a science without something like a Newtonian understanding of causation. Many find proposed theoretical alternatives difficult or impossible to comprehend precisely because those alternatives seem to require them to give up the very understanding of causality that makes science possible. Some alternatives seem to require social scientists to give up any notion of causality at all and, therefore, to make it impossible for there to be a social or psychological science. There seems to be more to psychology's reliance on Newtonian causation than mere historical accident.

In this paper, I will take the Newtonian theory of causation seriously. I will assume that its intuitive appeal is strong evidence for its validity as well as its usefulness. Without delving into the complexities of the philosophical problems of causality, problems discussed in a good deal of philosophical literature (e.g., Kitcher and Salmon, 1989), I will outline Newton's theory of causation, attempting to tease its essentials apart from its inessentials. I will conclude by arguing that the essentials of Newtonian causation are indeed necessary to any science (even a post-Newtonian one). Contrary to common belief, however, acceptance of Newtonian causation does not necessarily commit one to a mechanistic, materialistic, or deterministic understanding of the world. I will argue that the Newtonian view can be assimilated to contemporary theoretical alternatives in psychology. This means that, given the Newtonian understanding of causation, it is possible for such alternatives to be scientific — to treat of causes — without requiring either mechanism, materialism, or mathematical formalizations. Mechanism, materialism, and mathematical formalizations may be important parts of particular sciences, even of particular social sciences or particular social science methods, but they are not necessary to science as such. A non-mechanistic or a non-materialistic science of human behavior is possible, in principle, as is a science of human behavior that does not invoke mathematical formalizations.

To understand Newton's view, we must move back one historical step, to Francis Bacon, on whose work Newton's thinking about causation is based. According to Bacon, of the four Aristotelian causes (material, efficient, formal, and final), final cause is the "least perfect" (*Novum Organon*, 1620/1960, p. 121). Final causes, purposes, are those about which we can know the least, and an understanding of them results in the least knowledge about the world. Though final cause was perhaps the most important cause for ancient science, in *The Advancement of Learning* (1605/1955, in *Selected Writings*, p. 255), Bacon says that physics deals with material and efficient causation.

By the terms *material cause* and *efficient cause* Bacon appears to mean what he elsewhere calls "variable" causes (*Advancement*, 1605/1955, in *Selected Writings*, p. 255), in other words causative agents that can have more than one effect. Fire, which destroys some things, such as paper, and hardens others, such as clay, is an example of a variable cause. But Bacon's claim that physics deals with material and efficient causation is misleading. Even in *The Advancement of Learning* he suggests that physics must study more than material and efficient causation: when Bacon divides physics into three parts (the configuration of things, the principles of things, and the variety and particularity of things), it is obvious that physics is not limited to the variable causes, for the principles of things cannot be accounted for in terms of variable causes.

Fifteen years after *The Advancement of Learning*, in *Novum Organon* (1620/1960, pp. 121–124), Bacon contradicts his earlier claim, making it clear that formal cause is the most important of the causes for science. For Bacon, in every science, "the investigation, discovery, and development" of the laws of the bodies investigated by that science are the foundation of both the theory and the practice of the science (*Novum*, 1620/1960, p. 122). For reasons that will become more apparent later, I think it not unreasonable to think of these laws as the forms of bodies and their motions. Thus, material and efficient causes convey the formal causes to the objects affected, so a person who understands those forms has full understanding of the change caused in an affected body, an understanding those with knowledge only of the material and efficient causes (and presumably also of final causes) do not have. Genuine theory and free practice result from a knowledge of law, in other words, form (1620/1960, p. 122).

In addition, as we see in the examples of science and scientific questions in Book Two of *Novum Organon*, part of Bacon's philosophy of science — and, therefore, something that impinges on his theory of causation — is the assumption that science deals with nothing but individual bodies and the laws of those bodies: "nothing exists in nature except individual bodies, exhibiting clear individual effects according to particular laws" (1620/1960, pp. 121–122; see also pp. 122–124). Adding this to the scientific necessity of knowing formal causes, Baconian science is the experimental deduction of the formal causes of motions in bodies.

For practical purposes, one can think of Newton as simply adopting the Baconian view. For Newton, too, science is the experimental deduction of the formal causes of motions in bodies. But from a theoretical perspective, Newton does not adopt Bacon's view. There are differences between Bacon's and Newton's discussion of causation that allow us to distinguish what is essential to the understanding of science as the study of causation from what is not essential.

For one thing, Bacon accepts all four Aristotelian causes, but Newton's concept of causation is leaner. Newton accepts only two kinds of causation, mechanical cause and first cause (*Optiks*, 1704/1952, p. 369), neither of which Aristotle mentions, and both of which turn out, on examination, to be species of the same cause, namely formal cause.

What Newton means by *first cause* is relatively unambiguous. It is the Divine. In Aristotelian science, the final cause is the Divine. For Bacon, Newton, and other modern philosophers, the Divine is first rather than final cause. (The shift of the Divine from final to first cause has its roots in medieval theology and marks an important shift in the self-understanding of science, but that shift goes beyond the bounds of this paper.) But whereas Bacon excludes the Divine from his discussion of science, Newton includes it. The first cause is the point at which scientific investigation stops: having deduced, step-wise by experiment, each of the mechanical causes that follow from the first cause, science must stop at the first cause. But, though Newton asserts that there are two causes, for practical purposes there is only one, mechanical causation.

What Newton means by *mechanical cause* is not immediately clear. The word *mechanical* might lead us to believe that mechanical causation is roughly equivalent to Bacon's material and efficient causation. Especially given Bacon's reduction of nature to matter in motion, it is tempting to assume that by mechanical causation Newton means simple "billiard-ball" causation, the collision of objects, one with another. But Newton leaves room for a more sophisticated notion of causation.

In the preface to the first edition of *Mathematical Principles of First Philosophy* ([1687/1960], p. xvii), Newton reminds us that ancient mechanics were divided into rational mechanics and practical mechanics, and speaking of geometry as a species of rational mechanics, he defines rational mechanics as "the science of motions resulting from any forces whatsoever, and of the forces required to produce any motions, accurately proposed and demonstrated" (p. 1). Then, he describes his work as "the mathematical principles of philosophy [in other words, science]" (p. 1), and when he speaks of forces he says "I here design only to give a mathematical notion of those forces" (p. 5).[1]

[1]I ignore the problem Newton creates here whether such things as the first law of thermodynamics is part of mechanics. Newton seems not to be consistent, but the difficulties created by this question are not central to the question of causation.

In scientific practice this means that the Newtonian physicist takes account of causation by describing a force and its resultant motion with a mathematical formula. Like Bacon, Newton specifies that causation is formal causation. But though Newton's physics is a science of matter in motion, he explicitly makes that fact *non*-essential to the essence of science. 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. And Newton's 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.

The essence of the two causes Newton accepts, mechanical and first, is that they are both formal causes: each is the law of a science, a formal description of the origin of motions. The Divine is formal in that it is pure form. 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; the word *mechanical* is a technical term in Newtonian science: to be a mechanical cause is to be a force that is mathematically describable. Newton's definition of mechanics requires no necessary commitment to determinism or materialism.

Both first and mechanical causes are the objects of science, and both are formal causes, though only the latter requires either reference to force or, perhaps, corpuscular metaphysics. Thus, broadly conceived, Newtonian science requires an account of phenomena that invokes formal causes, not reference to motions in bodies, or — in spite of what appears to be Newton's belief to the contrary — reference to forces. Though it is common to think of Newtonian causation as efficient causation and to say that modern science reduced Aristotle's four causes to two, material and efficient, it is more accurate to say that Newton reduces Aristotle's four causes to one, formal cause.

Is Newton's theory of causation, a theory of formal causation, useful to sciences other than the physical sciences? The motions of billiard balls are obviously describable in mathematical terms, as are the motions of larger and smaller particles. As a consequence, even in a post-Newtonian age, such a theory of causation was and continues to be enormously useful to any science that deals with particles. But what about the psychological sciences? Can they use a Newtonian theory of causality? If they use it, does it commit them to mechanism of a narrower sort, namely to that which invokes a corpuscular metaphysics?

Not necessarily. First, notice that though the motions of particles are describable in terms of Newtonian causation, it does not follow that all motions so describable are the simple motions of particles. The inference that Newtonian causation requires materialist determinism, an inference made by some who argue for such determinism, as well as by those who argue against it, is an invalid inference. Newton's formulation of mechanics and mechanical causation does not imply that all mechanical motions are the motions of particles. In principle, non-materialist sciences can use a Newtonian theory of causation because they can, nonetheless, be mechanical sciences — or at least share with mechanical sciences the feature that makes them scientific.

The question remains whether all sciences must be mechanical, whether they must all involve a mathematical description of forces, though I have suggested that they do not. From what we have seen about Newtonian causation, it would appear that any science at all, even a post-Newtonian one, requires a Newtonian theory of causation, broadly conceived: scientific descriptions account for phenomena by reference to formal causes. But, I will argue, a formal theory of causation need not invoke forces, and its formality need not be the formality of mathematics.

Because of its subject matter, Newtonian mechanics always refers to effects in terms of forces. That reference to force makes Newton's science mechanical and distinguishes Newton's science from non-mechanical sciences: mechanical sciences are sciences of forces, non-mechanical ones are not. Given the history of our sciences, it may be tempting to assume that science necessarily involves the assumption of force, that there can be no non-mechanical sciences. But not all effects presuppose force, so *force* and *cause* are not synonyms and giving an account of causes and effects need not involve giving an account of forces. Thus, science is not necessarily mechanical, even in the technical sense of *mechanical*.

Medicine provides an excellent example of a non-mechanical science, a science interested in the explanation of changes, of phenomena — of effects — not necessarily in the account of forces. The explanation of disease describes a change and need not assume a force. In fact, to assume that the etiology of disease is like the etiology of particle motion would be radically to change our understanding of disease and its causes. (For a readable and more full discussion of this issue, see Harré, 1985, pp. 168–183.)

Similarly, it is possible to give causal explanations of psychological events without assuming that the causal relation is a mechanical one, a relation of force. Many discussions of such phenomena as desire and intent treat them as if they were analogues of Newtonian forces, but there is nothing about either the phenomena themselves or about the nature of science that requires such an analogy. Whether desire and intent are analogous to Newton's mechanical forces is a theoretical question that remains to be decided (mostly because it remains even to be discussed), but they need not be in order for psychology to deal with them scientifically, in other words, in terms of cause and effect.

Just as a broadly Newtonian theory of causation need not assume that all causation is mechanical — even in the technical sense — it also need not assume that all formality is mathematical formality. Here too medical etiology offers a counter-example to the assumption. Explanations of disease may, but need not, invoke mathematical descriptions. Someone might counter that even if medical ascriptions of formal cause do not logically necessarily involve mathematical description, they usually do involve measurement and, for all practical purposes, they must. The implication is that science, therefore, must include measurement.

But it would be incorrect to make that inference. 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. We need not conjoin reference to such a rule with the claim that the moral rule is the efficient cause of the agent's behavior. It may be, but it need not be. (To conjoin them is, I believe, to assume that we must account for behavior in terms of "forces.") For us to do psychology scientifically, it is enough to give a formal cause of behavior. We may, but need not, give an account of the behavior in terms of an efficient cause.

This means that we can conceivably give a scientific account of at least some behaviors without making measurements. Consider moral behavior again. For many if not all moral behaviors there are no degrees of compliance for us to measure. One either tells a lie or one does not; there is no variation or degree of lying to measure. Of course, one can measure such things as the frequency of lying, but that points to the difference between such measurements and the measurement of forces: unlike forces, lies as lies have no magnitude, so when we measure the frequency of behaviors like lying we are not measuring what is analogous to the measurement of force. Physicists do not measure the magnitude of forces by measuring the frequency of their occurrence; the magnitude of a force cannot be operationalized as the frequency of its occurrence. Thus, in the very act of measuring frequency of events rather than their magnitude, psychologists implicitly agree that such things as desire and intent are not analogues of force. A scientific account of lying need not measure the magnitude of the lie (since there is no such thing to be measured); measurement is not a necessary part of a formal account, even though many formal accounts do involve measurement.

Thus far I have given only a negative characterization of formal cause: it is not necessarily an account of forces; it does not necessarily involve a corpuscular metaphysics; it does not necessarily require measurement. What can I state positively about formal causes? First, that mathematics offers us an excellent model of formal explanation and, therefore, that we should not be surprised when it is so often a part of scientific explanation, even in the social sciences. But mathematics is not the only kind of formal explanation possible. Though Newton's causes (first and mechanical, both of which are formal) do not line up exactly with Aristotle's (material, efficient, formal, and final), Aristotle's discussion of formal cause (which he too takes to be of the essence of scientific description) is one of our best guides as to its nature. In the *Metaphysics*, Aristotle identifies form with pattern (991b21 and 1013b27), and his analogy seems to be the shapes and patterns we see in an object, its "outline." For, in *On Generation and Corruption* he describes the form as the "figure" (335a15). Formal cause is, therefore, the shape or pattern of a phenomenon.

Though, for Aristotle, the form we identify is a property of the thing itself, it need not be for us. It is enough to notice that the form inheres in the account, not necessarily in the nature of things. Taking the notion of formal cause together with Newton's insistence that scientific causation need not refer to the motions of bodies (*Mathematical Principles of First Philosophy*, p. 5) suggests that, from a Newtonian view, scientific causation does not inhere in the nature of things. When we speak of formal causes we speak of our descriptions rather than the nature of things. We may (or may not) accurately impute various elements of our scientific descriptions to the nature of things, but when we speak of formal causes, we are speaking of the patterns we use to describe the changes we observe rather than the nature of things themselves.

Aristotle gives two more clues that help us understand further what he means by *form*: in the *Physics* (194b26) he says that form is the archetype or the statement of the essence and its genera, and in the *Metaphysics* (1022ba5) he tells us that one of the meanings of *limit* is *form* — the form is that beyond which something cannot be further divided without referring to some new thing. These two clues mean essentially the same thing. From them it follows that, for contemporary scientific purposes, the form is the abstraction that identifies the thing in question such that any division of that identification would identify something else. A statement of formal cause, therefore, would be an abstract description of a phenomenon (for example, a behavior) given in the simplest terms possible.

Science does seem to require formal causal descriptions; it appears to be correct that no alternative account of psychology or any other science can succeed without giving such descriptions. Such a demand is no mean demand. As Immanuel Kant showed, formal descriptions are universal — in contemporary terminology, replicable (though it remains to ask what *replicable* means either theoretically or practically). Much that has been demanded by traditional social scientists in the name of objectivity must remain if there is to be a social *science*. But formal causal accounts are not limited to any narrow understanding of science. Only the confusion of physical causation (material and efficient causation taken together) with formal causation allows us to think that Newtonian causation is efficient and material causation. Only the confusion of mechanical causation with formal causation allows us to think that Newton's theory of causation is necessarily mechanical, even in the technical sense of that term. Only the confusion of experimental science with all science allows us to think that science, giving a causal account of changes, is necessarily experimental. Only the confusion of formality with mathematics allows us to believe that all scientific accounts involve measurement.

These confusions are ones that Newton himself sometimes falls prey to — and there is something "natural" about them — but they are nonetheless confusions. After all, though Newton deals only with the motions of bodies in his physics, his theory of causation, unlike Bacon's, does not require that science investigate only bodies in motion. Neither does it require that science be only experimental or that it be mechanical or that it be deterministic or mathematical. Pared down, what Newton requires is that science invoke formal causes to give an account of a phenomenon. Such a description of science and causation leaves open a wide array of possibilities for theories of psychology and social science, including, but not limited to, mechanistic empirical theories.

Many who call themselves empirical psychologists and social scientists speak as if the only empiricism is mechanistic empiricism, as if alternatives to traditional empirical studies are, somehow, cut off from the facts of the world, from bodies and their interactions, from sense data, and so on. Consequently, psychologists often think of alternatives to mechanistic empirical social science as necessarily "fuzzy" or "ethereal," but certainly not grounded in the concrete facts and details of the world. Those who advocate alternatives have often accepted the traditional empiricists' claim and have branded themselves non-empirical and, therefore, "fuzzy," non-rigorous. But there is no reason to let the devil have all the good words. If science need not be mechanistic to be scientific, then empiricism need not be mechanistic to be empirical. Just as those proposing alternative psychologies must recognize the need for causal accounts in their psychologies, they must also insist that their alternatives are empirical. We should not allow the word *empirical* to be used only to describe the narrow empiricism of the American School. That is not to say that the practitioners of the American School are not empirical scientists; it is to say they are not the only empirical scientists.

The same remarks apply to the idea of rigor. Those offering alternatives to traditional psychology must not abandon the necessity of rigor and discipline in their reaction against the rigor and discipline of mechanistic empiricism. They must not give in to the easy idea that a lack of rigor and discipline is acceptable in social science because they do not accept the understanding of rigor one finds in the dominant practice of social science.

Whether or not any particular proposed alternative to traditional psychology is actually scientific often remains to be seen. Whether it is practically useful also remains to be seen. But we cannot reject an alternative out of hand as unscientific (or impractical, for that matter) because it is not deterministic, or because it does not ground itself in the material as the material is conceived in the American School of psychology (in other words in terms of a corpuscular metaphysics), or because it does not invoke mathematical models of explanation. Alternatives to traditional social science and psychology can be scientific, and they must, but even within those confines, they can be genuine alternatives.

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