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Psychology and Newtonian Methodology

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According to Newton, the goals of natural philosophy comprise quantitative generalizations and causal knowledge, the latter being paramount. Quantitative generalizations are sometimes explanatory, in psychology as elsewhere (the role of the Gaussian model in explaining the shape of the ROC curve in signal detection is discussed). However, in psychology, they are not explanatory when the human subject is considered qua bearer of psychological states (beliefs, desires, and their ilk), but only when she is considered qua physical system. In the former case quantitative generalizations are, rather, *to be causally explained.* In this sense, psychology may be closer to the Newtonian methodological mark than contemporary physics.

"Natural philosophy consists in discovering the frame and operations of nature, and reducing them, as far as may be, to general rules or laws — establishing these rules by observations and experiments, and thence deducing the causes and effects of things . . . ."

Sir Isaac Newton
"A Scheme for Establishing the Royal Society"

Various modern authors distinguish between, on the one hand, causal explanations, and, on the other, those that appeal to fundamental quantitative laws. For instance, Nancy Cartwright (1983) claims that ". . . the propositions to which we commit ourselves when we accept a causal explanation are highly detailed causal principles and concrete phenomenological laws, specific to the situation at hand, not the abstract equations of a fundamental theory"[1] (p. 8). Causal explanations come in at least two varieties. The historian might explain a particular historical event by citing its specific causal

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[1]Cartwright adopts what she refers to as a "simulacrum" account of explanation by fundamental laws: whereas the detailed causal principles we invoke in causal explanations are (purported to be) true of the actual phenomena, fundamental laws are true only of objects in a model constructed to fit the phenomena into a theory (see, e.g., Cartwright, 1983, p. 17).

antecedents — a singular causal explanation. Often, however, we are interested in causal regularities, such as the fact that the common cold is caused by a virus, or that high interest rates cause, *ceteris paribus*, reduced numbers of home purchases. Even in the case of explanation by causal regularity, however, we do not have to appeal to fundamental quantitative laws akin to Newton's gravitational law, or the Schrödinger wave equation.

The opening epigram from Newton conveys the sense of an apparent explanatory prejudice on his part. "Deducing the causes and effects of things" is the aim of science presumably because to explain a phenomenon is to cite its causes. From what has been said so far, it might appear, then, that Newton did not share the view that there are two distinct kinds of explanation — perhaps he accepted only causal explanation. However, the following remarks from the General Scholium to Book III of the second edition of the *Principia* suggest that he did regard fundamental quantitative laws as having explanatory value:

> Hitherto we have explained the phenomena of the heavens and of our sea by the power of gravity, but have not yet assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centers of the sun and planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes used to do), but according to the quantity of the solid matter which they contain, and propagates its virtue on all sides to immense distances, decreasing always as the inverse square of the distances . . . . But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses; for whatever is not deduced from the phenomena is to be called a hypothesis, and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from phenomena and afterward rendered general by induction. Thus it was that the impenetrability, the mobility and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered. And to us it is enough that gravity really does exist and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies and of our sea. (cited in Thayer, 1953, p. 45)

Even though Newton viewed himself as having failed to give an adequate *causal* account of gravitation, he regarded his universal law of gravitation as significant because of its (quantitative) descriptive accuracy. He proposed a fundamental quantitative law that, more-or-less, saved the phenomena, and thereby had explanatory value (". . . it is enough that gravity really does exist and act according to the laws . . .").

It is debatable whether Newton should have regarded his inability to "deduce" the cause(s) of gravitational attraction as a failure. Bertrand Russell (1912–13), for example, argues that causes, "though useful to daily life and in the infancy of science, tend to be displaced by quite different laws as soon as science is successful" (he uses the law of gravitation as an example) [p. 13].

Certainly, the notion of cause does not enter directly into the various partial differential equations which most physicists today regard as the fundamental laws of their subject (although advertence to causation is not ruled out when it comes to the explication and application of said laws; and causal intuitions may well often enter into their discovery). Russell's attitude toward causes is by no means universal, however. Cartwright (1983) makes a powerful case for the view that "given the way modern theories of mathematical physics work, it makes sense only to believe their causal claims and not their explanatory laws" (p. 74).

The relationship, in modern physics, between fundamental laws and causal claims is complex. Whatever this relation, however, it is certainly not one of deduction, in the modern logical sense. How are we to interpret Newton's use of the term "deduce" in this context? Perhaps the fairest, and least anachronistic, summary of Newton's view is that causes and effects can be inferred in some way from the phenomena and the quantitative laws under which they fall.[2] The following picture of Newtonian methodology emerges: observe the phenomena, "save" them with quantitative laws, and infer causes and effects— although quantitative laws themselves do have explanatory value.

To what extent is psychology Newtonian in this sense? To what extent should it be? As a start toward answering these questions, I will analyze, as an illustrative example, a simple part of the classic theory of signal detection (SDT).

I have only the space here to give the briefest of sketches of the theory. My source is Green and Swets (1966), to which I refer the reader for further detail. Perhaps the ultimate goal of a theory of signal detection is a complete explanation and demarcation of our abilities to pick up and process environmental stimuli. Such a complete theory, even if attainable, would, of course, be enormously complex. Classic SDT sets itself a simpler task, by restricting the domain to artificially simple settings.[3]

A typical detection task might require an observer, in an auditory experiment, to attempt to detect a tone burst (the signal, s) in a background of white noise (n). The simplest classic theorizing about such a situation begins as follows:

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[2]Newton's use of the term "hypothesis" also requires care in interpretation. His remarks upon hypotheses are perhaps best taken as decrying speculation either that lacks empirical consequences or is not subjected to empirical test. He has also been interpreted as being opposed to the postulation of unobservable entities. Laudan (1981) is a useful source here (and, indeed, in matters of Newtonian methodology quite generally): see, particularly, pp. 92, 96 and 112.

[3]Such approaches are, of course, vulnerable to attack as ecologically unsound. The laboratory setting, it might be argued, is so artificial as to make extrapolation to perception under normal circumstances at best inaccurate.

> When considering two alternative hypotheses [s and n] . . . we may assume that the various sensory events can be mapped on a single line that we call x. The numerical value of an observed sensory event affects the observer's confidence about which hypothesis is true. Indeed, we assume that he has a criterion k such that he will choose n whenever x<k and . . . s whenever x>k. (Green and Swets, 1966, p. 58)

The assumption is made that the distribution of the random variable x is normal (Gaussian) under each hypothesis. There are then two parameters of interest: the difference between the means of the two distributions, and the ratio of their standard deviations. In the simplest case, the latter is one. The former, when divided by the common standard deviation, is the standard measure, d', of the detectability of the signal. Varying the decision criterion k varies the hit (the observer responds s when s is the case) and false alarm (the observer responds s when n is the case) rates, generating, for each value of d', a unique receiver-operating-characteristic (ROC) curve, when the hit probability is plotted as ordinate, the false alarm probability as abscissa. That is, it is assumed that there are exactly two factors affecting the hit and false alarm rates: the detectability of the signal (a function of the signal and hardwired features of the observer's sensory apparatus, we suppose) and the observer's decision criterion — how sure she must feel that s is present before she responds that it is (a function of the observer's attitude).

So much for the simple theory; now for its empirical confirmation. Suppose we want to generate an empirical ROC curve for our paradigm case of detecting a tone in a background of noise. Throughout the experiment, we need to keep the physical characteristics of the signal and noise constant — so as to keep d' constant. To generate, reliably, a single point on the curve, we require that the subject maintain one decision criterion over a large number of trials. To generate further points, we need her to alter her decision criterion, maintain it for the requisite number of trials, alter it again, and so forth. One common method of inducing changes in decision criteria is to vary the a priori probability of the signal (the rarer the subject knows the signal to be, the more conservative she is, and thus the lower are both her hit and her false alarm rates). Another is to vary the payoffs for hits, false alarms, and so on. Having generated a series of points by inducing variation in the subject's decision criterion, we can then fit a curve, and determine (all, of course, within the bounds of experimental accuracy) whether the Gaussian model with equal standard deviations is appropriate.

Green and Swets (1966, pp. 88–89) display two series[4] of points generated under identical stimulus conditions. In one case the a priori probability of the signal was varied, whereas in the other it was the payoffs that were

[4]The data for the first series are from Tanner, Swets, and Green (1956). Those for the second series were collected by Green and Swets.

altered. ROC curves based upon normal distributions of equal standard deviations fit the data well, and are identical for the two cases. That is, first, the Gaussian model with equal variances appears appropriate. And, second, as the model predicts, provided the physical characteristics of signal and noise remain constant, the best-fit ROC curves are identical, the different methods of inducing change in the observer's decision criterion notwithstanding.

So the observer's ROC curve for the signal and noise presented yields an index of the detectability of that signal against that noise for that observer. Thus we have gleaned information about the observer's sensory apparatus qua "hard-wired" physical system. But, as we have seen, there is another factor, besides the resolution of the observer's sensory system, operative in the production of any particular point on the ROC curve: the observer's decision criterion.

Assuming the observer's utilities for the payoffs involved match the monetary payoffs and penalties, an optimum decision criterion can be calculated for each payoff/probability structure which, if employed by the observer, would maximize her expected utility under that structure. Hence, a further question can be asked of the data: How do the observer's actual criteria for the various structures compare with the optimal criteria?

This question is indeed addressed by Green and Swets (1966, pp. 90–94). The details need not concern us here. Suffice it to say that, whilst there is a perfect rank-order correlation between the observer's actual criteria and the optimal criteria under the various payoff/probability structures, there are significant differences in the numerical values. The interesting feature, for present purposes, is the fact that such a comparison is made at all: the framework within which it is of interest is that of belief–desire psychology. We assume that the observer wants to maximize her payoff. If she fails to adopt the optimal strategy for doing so, we ask why; and we seek detailed causal explanations.

Green and Swets (1966) are very explicit about distinguishing sensory theory from questions about decision criteria:

> In considering the relation between optimal and obtained decision criteria in a detection task, it is important to place that relation in the perspective of sensory theory. From the standpoint of sensory theory, the decision goals defined within statistical decision theory simply suggest convenient ways of generating an empirical ROC curve. For sensory theory, it is important to obtain various decision criteria . . . which span a range that is sufficient to define clearly an ROC curve; how these decision criteria are obtained is of little or no concern . . . . Thus, the well-known fact that human decision makers commonly introduce a subjective transformation of probabilities and of real decision values is not necessarily salient in the detection setting. Although more sophisticated theories of human decision making, which take into account the subjective transformations, might be fruitfully applied in real detection situations, students of sensory functions working within the framework of decision theory have not viewed them as essential to the idealized detection problems constructed for experimental purposes. (p. 93)

We can distinguish, then, two aspects of our example: quantitative analysis and causal explanation. A quantitative model is proposed, experiments are performed and measurements taken, and the quantitative predictions of the model are compared with the experimental findings. The procedures parallel the "confirmation" of Newton's gravitational law via, for example, the derivations of Kepler's laws and Galileo's law for pendulum motion, these latter being accepted on the basis of observation and measurement. I hesitate to attribute to the Gaussian model from which theoretical ROC curves are derived the status of "fundamental law," in part because it is of limited scope, and in part because the model itself can be "justified" (Green and Swets, 1966, pp. 57–58) by the central limit theorem, if we make various assumptions about sensory events. But the Gaussian model has explanatory power partially analogous to that of Newton's gravitational law. The Newtonian could explain, for example, Galileo's law for pendulum motion by deriving it from the gravitational law;[5] the signal detection theorist can explain the shape of an ROC curve by deriving the curve from the Gaussian model.

However, such laws and models are obviously not themselves causally efficacious (the Gaussian model, for example, is not itself an event with causal consequences). And neither can particulars of causal mechanisms be inferred from them.

So, what of inferring causes and effects? As we have noted, there is discussion about the causal underpinnings of particular points on empirical ROC curves. However, this discussion focuses not upon the Gaussian model (sensory theory), but upon decision criteria — that is, not upon the observer qua physical system, but upon the observer qua decision maker: the bearer of a psychology (propositional attitudes, emotions, and their ilk). This is not to say that there is no interest in the causal properties of the sensory system that give rise to a Gaussian distribution. Indeed, in the justification of the Gaussian model (see above), it is assumed that "sensory events are composed of a multitude of similar, smaller events, which are by and large independent" (Green and Swets, 1966, p. 58). However, the Gaussian model is deemed explanatory quite regardless of detailed causal inferences about the sensory system, just as Newton's gravitational law is deemed explanatory despite Newton's "failure" to infer the cause(s) of gravitational attraction. (Perhaps unification is one of the key considerations in these cases.)

The following claim emerges from these ruminations: within the realm of psychology, to the extent that quantitative general "laws" are considered explanatory, it is only when the focus is the human being qua physical system. When it comes to the human being as an entity with psychological (as

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[5]Cf. the deductive-nomological model of explanation; see, for example, Hempel (1966, pp. 49–54).

opposed to, for example, psychophysical) properties, quantitative generalizations are considered just that: they might have limited predictive value, but they are not themselves explanatory. Indeed, they require explanation in terms of underlying psychological causes — motivations, aversions, beliefs, desires, hopes, and the like. As things turned out in SDT, we had no quantitative generalization about decision criteria that fitted the data — but I trust it is clear that, had, say, the actual criteria matched the optimal criteria, we would have explained this fact in terms of such psychological factors as the observer's desire to maximize her payoff.

To take another example, consider a social psychologist working with longitudinal data on socioeconomic status. My claim, that it is only in the realm of the physical that quantitative generalizations are regarded as genuinely explanatory, is borne out by the fact that she seeks to explain causally any intergenerational correlations she finds. (Indeed, she might well collect and analyze the data with certain causal hypotheses in mind.) The correlations themselves are not considered explanatory. Incidentally, her explanations might well cite structural societal properties (perhaps, though not necessarily, in addition to individual psychological properties): properties do not fall into only two classes, the physical and the mental. Sociological explanation, for instance, when treating the individual as a sociological entity, appeals to sociological properties. Each domain has its explanatory tools, amongst which are the properties to which its practitioners appeal[6] and the appropriate relations between explanans and explanandum.

Our social psychologist, then, might well employ quantitative generalizations both in conjecturing causal relations and in confirming causal hypotheses, but these generalizations themselves are not explanatory — it is causal explanation that she is after. And this is a Newtonian goal: whilst acknowledging an explanatory role for quantitative generalizations, it seems that Newton viewed causal explanation as paramount. So, when compared with those cases in physics in which a quantitative generalization is itself considered genuinely explanatory, psychologists who employ quantitative methods as means to the end of causal knowledge are, ironically enough, closer to the Newtonian mark.

Should psychology be methodologically Newtonian? Nobody would deny, I think, that psychologists should be in the business of teasing out causal relations (broadly construed, and to the extent that we can make sense of the notion of cause). Recall one conclusion that emerged from the discussion of SDT: we seek detailed causal explanations when considering the observer

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[6]This is not to say that there is never debate as to which domain a particular property belongs. For instance, in the IQ controversy, nativists appeal to genetic causes, nurturists to psychological and sociological causes. Nativists consider IQ a biological property; nurturists deny this.

qua bearer of psychological properties. But what about the use of quantitative generalizations and methods? On occasion, as with the Gaussian model in SDT, quantitative generalizations are themselves explanatory. In the context of discovery, surely a pragmatic attitude is warranted: such methods and generalizations should be employed to the extent that they are useful. It is in the context of justification that controversy arises. Here it seems that if a theory gives rise to testable quantitative predictions, it should be subjected to such testing. But if a theory yields no such testable predictions, should it be discarded as worthless?

I think not. It is often argued, for example, that Freudian theory is not testable in any way (see, for example, Popper,[7] 1965, pp. 34–39). That is, the claim is made that Freudian theory can be invoked to "explain" any relevant findings whatsoever. Yet it is a source of many interesting ideas, and perhaps knowledge (causal and otherwise). And, as Duhem (1906/1982) points out, predictions cannot be derived from a theory in isolation; such derivations require auxiliary assumptions. So, by introducing the appropriate auxiliary assumptions, any theory can "save the phenomena" (see Duhem, 1906/1982, pp. 183–190, for historical examples). Of course, certain auxiliary assumptions are declared *ad hoc*, and rejected on that basis. But philosophers of science have searched in vain for a universal criterion of the *ad hoc*. There are no hard and fast principles here: case by case judgments on the part of scientists are required (although we can isolate factors relevant to these judgments; see, for instance, Hempel, 1966, pp. 28–30, for further discussion).

In the opening citation, Newton speaks only of "general rules or laws"; there is no explicit reference to quantities. In physics, many such "laws" are quantitative; but many are not ("lightning is electrical discharge" might be a candidate). However, in accordance with Newton's criteria, even the non-quantitative candidates for laws in physics claim universality of some variety (this criterion of lawlikeness has been much discussed; see, for example, Hempel, 1966, pp. 54–58 and pp. 66–67). In psychology, however, when considering the human subject qua bearer of a psychology, causal explanations tend to be of the singular variety. Any causal laws which might be claimed are so larded with *ceteris paribus* clauses as not to warrant the epithet. But why suppose laws are an appropriate goal here? Recall the claim of Cartwright cited in the opening paragraph: she claims that even in physics,

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[7]According to Popper (1965), a theory is scientific only if it is testable (see, for example, p. 37). Freudian theory is thus not (yet) science, he claims — but he does not dismiss it as without value:

> This does not mean that Freud [was] not seeing certain things correctly: I personally do not doubt that much of what [he] say[s] is of considerable importance, and may well play its part one day in a psychological science which is testable . . . . [Freudian theory] contain[s] most interesting pyschological suggestions, but not in a testable form. (pp. 37–38)

causal explanations commit us only to "highly detailed causal principles and concrete phenomenological laws, specific to the situation at hand" (1983, p. 8). There is more generality in physics than in psychology, but not so much as some critics of social science might suppose. This generality perhaps gives the physicist more predictive power than the psychologist, but even this is not clear-cut: simple folk-psychology is pretty good predictively. I am very good at predicting my friends' actions on the basis of knowing their intentions, for instance. And when it comes to explanation, there are many different forms; and that form appropriate to physics (which, if Cartwright is correct, is not, perhaps, as general as Newton demands) might not be appropriate everywhere. In our attitude toward psychology, we can share Clark Glymour's (1983) attitude toward social sciences more generally:

> [m]uch of the work in contemporary sociology, educational research, political science, and econometrics . . . produces . . . causal explanations . . . and . . . causal knowledge, without producing general laws, at least not the sort of general laws that [certain] critics of social science demand. (pp. 127–128)

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