Laws, Idealization, and the Status of Psychology

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Abstract to

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The SPP is, among other things, a place where we discuss nagging and perennial problems on the bordermarches between philosophy and the sciences. Sometimes problems are nagging and perennial because they are deep and difficult. And sometimes they are merely an artifact, a shadow cast by our own way of formulating the problem. I should like to suggest to you that philosophy of mind suffers badly from being the last refuge of the best philosophy of science of the 1950's, and that some of its problems are in fact illusions that could be dispelled by consideration of more recent developments in the philosophy of science. In particular, philosophy of psychology has been plagued by a famous contrast between its "ceteris paribus" laws and the "exceptionless" laws of the physical sciences. This has led to doubts about the scientific status of psychology, the status of psychological kinds as natural kinds, and even their ontological legitimacy. I argue here that this problematic is a consequence of assuming a particular analysis of scientific laws as (exceptionless) universally quantifed claims. This analysis has largely been rejected in contemporary philosophy of science. And more recent analyses that take notice of the role of idealization in scientific modeling both dissolve the nagging problem and shed new light upon differences between the sciences.

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I have chosen to concentrate on one example here, and to treat it in such detail as the limited time will allow. My example is a famous problem about the scientific status of psychology. The problem, noted by Donald Davidson and notably taken up by Jerry Fodor among others, is supposed to go like this: The natural sciences are characterized by having strict and exceptionless laws. Psychology and the other studies of human beings and societies, by contrast, have only *ceteris paribus* laws. From this, different writers have drawn various conclusions.

- Some have taken exceptionless laws to be the hallmark of science as such, and hence concluded that psychology is not (and perhaps cannot become) a science.
- Others have drawn ontological conclusions. If you take laws to be the things that "carve nature at the joints", then you will be likely to think that "natural kinds" are

those kinds to which strict laws apply, and hence that psychological kinds are not natural kinds.

- Moreover, if you are a naturalist, and inclined to believe that the only ontologically legitimate kinds are natural kinds, or at least things that supervene on natural kinds, you will see this all as a threat to the very ontological legitimacy of psychological kinds.

Over the past two decades, much philosophical discussion of psychological laws and natural kinds has labored within these bounds, accepting rather uncritically the assumption that the laws of the natural sciences are best interpreted as something on the order of universally quantified statements ranging over objects or events. Curiously, during this same period, this view of laws has received withering and perhaps decisive criticism within the philosophy of science. And if we appropriate the lessons of these criticisms, much of this familiar philosophical problemmatic simply blows away on the wind.

The problem explicated

The threat psychology could be summed up in the phrase, “psychology doesn’t look enough like physics.” First, psychological “laws” are supposed to be notably different than physical laws. The contrast between “Exceptionless” and “ceteris paribus” laws comes to light most clearly in terms of the prevailing formalization of scientific laws in post-Empiricist philosophy of science—namely, the interpretation of these laws in first-order predicate calculus. Perhaps the most influential post-Logical-Empiricist attempts at explicating the logical structure of scientific laws were those of

Hempel’s “Studies in the Logic of Confirmation” (1945). Hempel interprets laws in terms of first-order logic, where the domain is physical objects and events. For example, he suggests that we interpret the “law” that metal heats when expanded in the following terms:

(x)[(Metal (x) . Heated(x))   Exp(x)] (1965, page 26)

This first-order logical interpretation would now generally be regarded as too weak, in that it aims only at material adequacy and does not cover counterfactuals, and so a more modern version would seek to lend some modal strengthening to such a law by the prefixing of a modal operator for some kind of “natural” or “nomological necessity”:

[](x)[(Metal (x) . Heated(x))   Exp(x)]

The worry about psychology is that psychological generalizations rendered in this form clearly have exceptions. This is patently true of commonsense generalizations like “people will eat the foods they like”. But it is also true of even areas like visual psychophysics, where there are more mature laws like the Weber laws, as even these are variable across populations, are subject to effects like saturation effects, and assume “normal” conditions like trichromatic subjects who do not have conditions like macularization which effect both color detection and subjective experience of color.

The normal way to characterize this situation, in which there are generalizations that do not hold of all cases, is to treat them as *ceteris paribus* laws – lawful claims that are hedged by a specification of conditions under which the law does not hold that are explicitly screened out. For a law that holds for color vision, these might include, say, the assumption that the subject is not colorblind, and does not have a macularized cornea. The general form of such laws is taken to be:

$$[\,](x)[\tilde{}(c_1(x)\ \ v\ \ c_2(x)\ \ v \ldots c_n(x)) \qquad L(x)]$$

where L is the law in question, and $c_1 \ldots.\ c_n$ are the conditions in which L(x) does not hold.

Cartwright, Fundamentalism, and Idealization

If the threat to psychology could be summed up in the phrase "psychology doesn't look enough like physics," the response might be summed up, "Well, but really physics doesn't look much like physics either!" Or, more precisely, physical laws do not look much like their philosophical reconstructions in first-order logic.

The most influential criticism of this view of exceptionless laws came in Nancy Cartwright's landmark (1983) essays published under the title *How the Laws of Physics Lie*. Cartwright claims that "there are no exceptionless quantitative laws in physics. Indeed not only are there no exceptionless laws, but in fact our best candidates are known to be false." (46) Or, to put it slightly differently, if you interpret physical laws as universally quantified claims, most or all of them turn out to be false. (Indeed, though she does not put it this way, many of them have no true substitution instances.) For example, if we treat the inverse square law as a statement about how any two bodies will in fact behave, it will prove false in two ways. First, the law idealizes away from the fact that there are multiple gravitational interactions going on at once—a full-scale assessment of gravitational interactions would require a summation of forces that was dynamic over time, which is unfortunately computationally impossible when the number of bodies is greater than two. Second, the model has idealized away from the influence exerted by the other fundamental physical forces: friction, mechanical collisions, strong force, magnetic attraction, etc. And some combination of these is always at work in nature. So whatever laws express, it is not true universal regularities about events. And hence, if you treat them as claiming universal regularities, what they claim is false. (And the case is no better if you move to modal logic instead of first-order predicate calculus, since claims that are not even materially true cannot be necessary, as even nomic necessity is stronger than material adequacy.) This is what Cartwright means when she says, a bit more theatrically, that the laws of physics "lie".

What exactly they <u>do</u> express instead is perhaps more vexed. Cartwright herself seems to offer two different answers in subsequent work published in *The Dappled World*: (1) that laws indeed express regularities, but that these are local rather than global regularities, and are *ceteris paribus* laws rather than universals claims, and (2) that laws indicate capacities or potentialities of things rather than their actual behavior—e.g., that mechanical and gravitational laws in fact express mechanical and gravitational *forces*, respectively.

I prefer, however, to analyze the situation by attending to the role played by <u>abstraction</u> and <u>idealization</u> in the process of arriving at scientific laws. . In examining one kind of property—e.g., gravitation—one commonly abstracts away from other features of the objects of the domain. So whereas Aristotle's physics, for example, stressed the nature of each object (i.e., that it was a man, an acorn or a rock) in its explanation of "natural motion", modern theories of gravitation abstract away from the specific nature of objects and treats them as point-masses. Specific nature is bracketed off, because it is irrelevant to the domain in question.[1]

But science also regularly deals in <u>idealizations</u>. Idealization may be viewed as a special form of abstraction, in which one abstracts away from <u>properties that are relevant to accurate description or explanation *in vivo*,</u> and treats them as though they were not factors. Thus when Galileo rolled balls down an inclined plane to determine the rates at which bodies fall, they did not in fact roll down the ramp with times that conform exactly to his formulas, due to the fact that they were impeded by air resistance, by friction generated by their contact with the ramp and by imperfections in the surfaces. Galileo's description of falling bodies thus abstracts away from factors (air resistance, friction) that really do affect how bodies fall *in vivo*, and gives an exact description, <u>not</u> of how bodies <u>in fact</u> fall, but of how they <u>would</u> fall in the ideal case in which these impediments were removed.

This, unlike the abstraction away from the fact that the falling bodies are, say, cabbages or kings, is not innocuous: interpreted as claims about how bodies in fact fall on Earth, Galileo's laws are false, because wind resistance and friction are in fact at work. The moral of this is not, however, that Galileo's laws are incorrect. The moral is that *they are not claims about how bodies in fact fall on Earth*. What are they? I think the answer to this is twofold. They are claims about the *idealized* behavior of falling bodies in the limiting case in which the interference of other forces upon their descent is reduced

[1] Of course, the properties that are significant for classification and explanation in one domain may well be abstracted away from in another, with the effect that, if they describe the same objects, they may cross-classify them or deal with them in terms of unrelated or incommensurable properties. Thus two sciences may deal with the same objects, and yet what appears salient about those objects will be different in the eyes of the two sciences, because they are interrogating the world with a different set of questions and hence have emphasized and abstracted away from different features.

to zero. On the one hand, this means that they are exact claims about the (rare and perhaps nonexistent) behavior of bodies under these limited conditions. This sort of thing could be represented by a *ceterius paribus* law as formulated earlier. But, on the other hand, Galileo was *not* just making a claim about how objects behave in these limited cases, but about the dynamics even of bodies that are also subject to other forces, and this cannot be captured by the *ceteris paribus* formulation.

This is a deep fact about many scientific descriptions and claims, and particularly scientific laws: they are *not* properly interpreted as universally quantified claims about actual objects and events, but as idealizations. Interpreted as universally quantified claims about actual objects and events, most laws are *false*, and indeed *have no true substitution instances*. There may be exceptions, such as the law of intertia, though Cartwright argues in *The Dappled World* that even *F=ma* is applicable only in a very limited range of cases. But, more importantly, the role of idealization ought never to be ignored in the logical analysis of scientific claims, and will prove crucially important when we turn to look at the prospects for laws in psychology and other special sciences.

Are the *Ceteri* ever *Paribus*?

Note that this analysis of the role of idealization in scientific laws has the consequence that the expression "*ceteris paribus* laws" is deeply misleading. For even in some of the core cases of fundamental physical properties, other things are never "equal"! Since gravitation operates over infinite distances, there are no real events that are pure cases of mechanical interaction or electromagnetism. [2]

[2] Oh, often we can get an approximation of real-world behavior that is good enough for our practical purposes while idealizing away from gravity, but the quantification analysis of laws is

More fundamentally, of course, if we were to exclude cases in which gravity is at work from the domain of the laws of contact mechanics in order to preserve their status as exceptionless universal claims, the "laws" that we got as a result would not be the same laws the physicist is interested in. The physicist is interested in the cases in which there are both mechanical and gravitational forces at work. He just deals with the different forces through different sets of laws, which idealize away form different sets of real-world phenomena.

Dissolving the Problem for Psychology

Once one has made the paradigm-shift away from viewing laws as universally-quantified statements over objects and events, much that seemed problematic for psychology just dissolves.

- First, if there is no principled distinction between "strict" laws and "ceteris paribus" laws, then we have lost the very framework in which the supposed problem for the status of psychology was cast. We simply can no longer pose this exact problem, any more than we could pose problems about planetary epicycles after we abandoned Ptolemaic cosmology.

- Second, if even physical laws can fail to yield true and exceptionless generalizations about how events will unfold, it is unclear just what the nature of the difference between physical and psychological laws is supposed to be. Of

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not an attempt to get at the pragmatic success conditions of laws, but their proper logical analysis. And if you interpret laws in this way, not only do things like the laws of contact mechanics turn out to not be strict laws, they are not even *ceteris paribus* laws, since all actual dynamic events have other physical forces at work as well. (92)

course, in many areas of psychology we seem to be without laws altogether. But in areas where we were tempted to see "rough" or "ceteris paribus" laws, in just what ways do they fall short of the standards of physics? There is a real question here; but the answer cannot be as simplistic as that the physical laws yield true predictions more often, since some of them, interpreted as universal claims, never yield true predictions, and hence psychology could not possibly do worse at that particular game!

- Third, if you abandon the idea that laws are universally quantified claims over objects and events, you also have to give up on that too-easy road to a naturalistic ontology that uses the truth of these universally quantified claims to yield a privileged partitioning of the quantum soup into natural kinds. It is not clear how those drawn to this ontology might try to salvage it; but at least for the moment, it would seem that there is no threat to the ontological legitimacy of psychological kinds from this quarter.

In short, an entire philosophical problematic that psychologists may have spent undue time trying to ward off would seem to be an artifact of a particular—and erroneous—philosophical analysis of scientific laws.

Problems Recast

This does *not* mean, of course, that there are not real and perhaps even principled differences between psychology—or specific parts of psychology—and sciences like physics (or better, localized parts of physics like thermodynamics or quantum mechanics). At a purely formal level, physics enjoys a level of mathematical modeling that is considerably more elegant, fundamental and mature than that found in psychology. And at an empirical level, some of the differences that were pointed to by the inapt language of "strict" and "*ceteris paribus*" laws are enduring differences. Much of psychology, after all, does not have structural or dynamic laws at all, but only statistical generalizations over populations. And not only belief/desire generalizations, but even more mathematically-precise areas of psychology (like psychophysics and the modeling of the neurological processes underlying perception), seem incomplete and subject to an open-ended list of extrinsic factors in a way that at least seems very different from the mutual independence of physical forces like gravity and electromagnetism.

My intent here is not to minimize these problems in interpreting the nature of psychology, but to avoid drawing the wrong philosophical conclusions. The sciences of vision and hearing are messier than the sciences of light and sound, as evidenced by the fact that great mathematical physicists like Mach and Helmholtz found problems they could not solve when they turned their attention to perception. And viewing laws in terms of idealization instead of true categorical claims does not make the messiness go away.

It may, however, give us resources to understand it better.

Idealization in Systems with Feedback

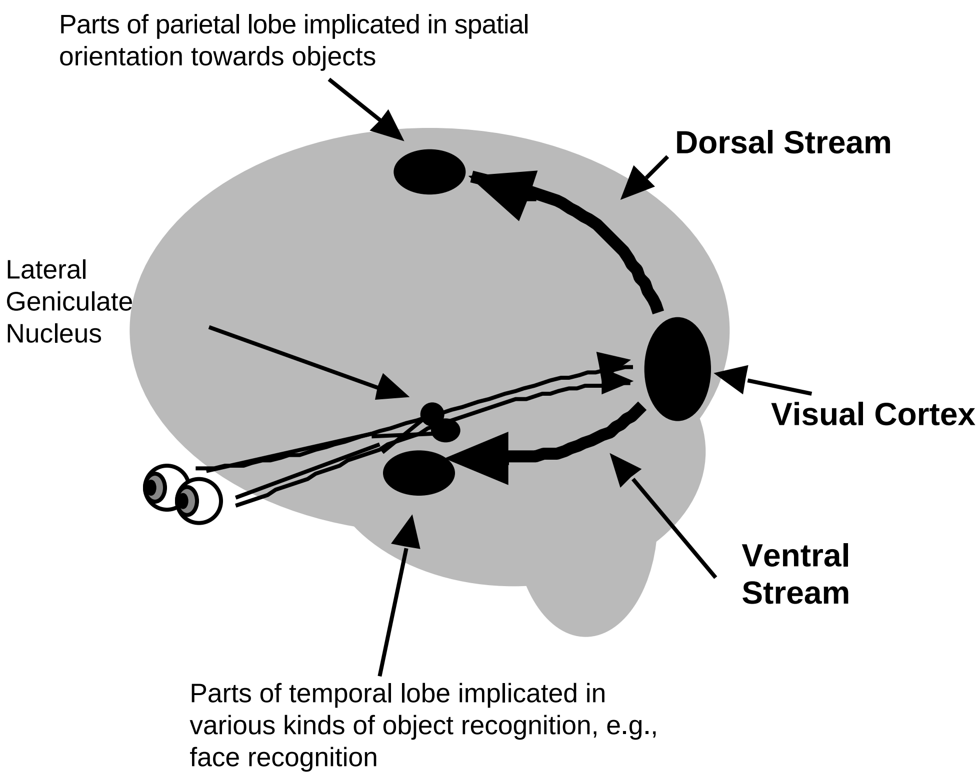
Consider: in the case of physics, we are blessed with a relatively small number of fundamental forces, and they are believed to be independent of one another. In principle, this means that you can look at a dynamical system by performing a number of separate idealizations that allow you to look separately at the influences of mechanical force, gravitation, electromagnetism, strong and weak force, and then treat the overall causal situation at a given time as a summation of these forces. There are well-known principled problems in turning this summation of forces into a prediction of how the dynamics of the system will unfold—indeed, even if one restricts oneself to gravitational force and classical mechanics, the problem of predicting the unfolding dynamics of a three-body system is computationally impossible. Cartwright gives an additional reason for pessimism about composition:

> When different kinds of causes compose, we want to explain what happens in the intersection of different domains. But the laws we use are designed only to tell truly what happens in each domain separately. (12)

But even if the actual dynamics of systems is very complex and defies computational composition, at least the independence of the fundamental variables gives us reason to believe <u>that each of our laws deals with a real, distinct, and fundamental simplicity.</u> Moreover, we can <u>intervene</u> in nature, creating experimental situations which asymptotically approach conditions in which only one force is in fact at work. And in these situations, the fundamental force that is left behaves "normally", and indeed, approximates its "ideal" behavior.

We too easily tend to think of this kind of situation—in which there are a small number of mutually independent variables which are, in principle, factorable—as the "normal" or even the universal case. But there are clear and familiar cases all around us of systems that do not meet these conditions. Basically, any system in which there are feedback mechanisms that modulate the behavior of the component systems will differ in fundamental ways from what we find in doing basic physics. Examples are found not only in psychology and neuroscience, but in physiology, biology, cybernetics, computer science and some parts of physics and chemistry as well.

The explanatory method of black box functionalism is common in many areas, including computer science and cognitive neuroscience. To understand a system, we break it down into its component parts, and when we have understood them, we use them to understand the behavior of the entire system. For example, in understanding vision, we may break up the visual cascade into stages: [Figure 1]



Often there is a strong convergence of different kinds of evidence that lead to the conclusion that we are in fact dealing with separate "modules" involved in cognition—evidence from selective insult, MRI and CT scans, anatomical and physiological differences, etc.—and hence one is drawn to a reasonably robust realism about these modules.

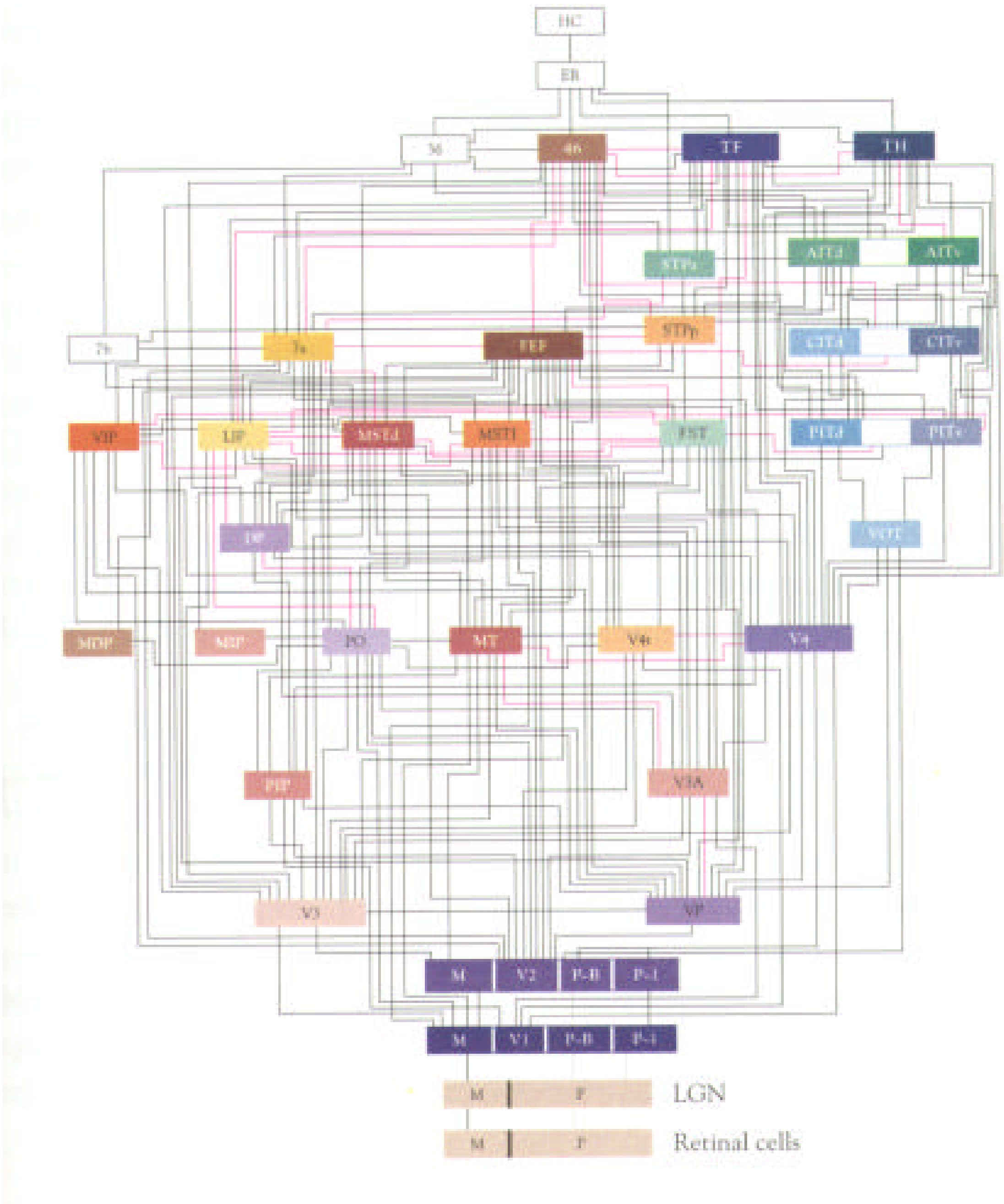
However, while the cognitive modules may be real, they are not independent in the ways that the fundamental physical forces are independent of one another. First, and most obviously, we are dealing here with a causal chain of events in different parts of the brain, and not with the factoring of independent forces. But it gets more complicated than this. Generally we are not dealing with a simple causal chain in the brain, but a very complicated and iterated feedback system. In most cases in which there are feed-forward projections from an area A to an area B in the brain, there are also feed-back projections from B to A as well. And the dynamics of feedback systems naturally leads one to expect that some of the important processes will really take place through the unfolding over time of a kind of resonance relationship, or stable and self-supporting pattern of interaction between modules. Feedback systems allow an organism, for example, to compensate for changes in illumination and see a constant color on an object as the sun goes behind a cloud, or to compensate for overshooting a target in perception or locomotion.

The interdependence of modules that are involved in this kind of feedback circuit is not simply a matter of one of them being downstream of another in a causal chain. Rather, they are dependent for their normal functioning upon the feedback they get from one another. And this presents both formal and philosophical problems. The root issue is that, even once you map out a model of the input-output conditions of a component of a system—its "circuit diagram" if you will—you haven't got a handle on the dynamics of the system, because these depend upon the unfolding of feedback loops over time. What happens at any one juncture at the visual cortex is going to be a function, not only of the physiology of that little bit of cortex, or even of that combined with some canonically described "input", but also by various kinds of modulating feedback from other parts of the cortex—for example, expectations based on memory or priming, nonspecific arousal, and emotion. The formal problem is that you need a very different kind of mathematical model for this kind of system than you do for at least classical physics, though the mathematical machinery needed is shared in common with some other parts of physics, and is currently collectively designated as "nonlinear dynamics."

The philosophical/empirical problem is this: In physics, if you succeed in creating a laboratory situation in which you have eliminated the influences of electromagnetism, mechanical, strong and weak forces, you are left with gravitational interaction in its pure and native glory. In an idealized situation, gravity in fact behaves ideally. But if you isolate an element of a feedback system like the brain from the rest of the system, <u>you do not get its ideal behavior, but radically abnormal behavior or no behavior at all</u>. If you cut out a monkey's lateral geniculate body, you don't get the behavior of the LGN in its pure and native glory, but merely a morsel of monkey brain sushi.

In this respect, there is a radical difference between cognitive neuroscience and fundamental physics. The fundamental forces of physics are radically independent. The modules of the brain are just as radically <u>inter</u>dependent. The <u>normal</u> behavior of a module is <u>not</u> captured by something like an internal circuit diagram, but a complex model of how it behaves as modulated by various other systems within a living and biologically normal organism.

This all has an important effect upon the relationship between our models and prediction. In cases like physics, in which you have a small number of independent variables, you can often use just one law, or a rough summation of several, and come up with a prediction that is roughly accurate. The mathematics of chaos tells us that there are classes of systems in which this will not work at all; but it works well enough for, say, launching a cannonball into a pirate ship. Calculate ballistics and wind, and these will drown out other factors if you are content to hit anywhere on the side of the galleon. Launch a mortar into a tornado that has just ripped through a magnet factory, however, and all bets are off.



The brain is more like the tornado case. Take a look at any recent schematization of the modules of the brain and the relationships between them. Any one module might be modulated by dozens or even hundreds of others, many of which are involved in its normal performance. When we idealize away from most of these to isolate some crucial function we are interested in—say, that this bit of brain performs noise reductions in perceptual information coming from that bit of brain—we are indeed getting at something real. In this our model is like physical law. But we are not getting at something that is independent of the rest of the brain. And in this respect, our case is very different from fundamental physics. The enterprise of modeling requires us to idealize away from things that matter in vivo, not just as additional things going on in the world (like electromagnetism), but as things that matter to the normal performance of the system we are studying. And as a result, models that get at true generalizations that are useful in the enterprise of understanding will produce results that are much farther removed from the separate ideal of yielding true predictions.

The moral of the story is that psychology is, in a principled way, harder than physics. The reasons for this are both systemic and formal. The systemic reason is that in psychology we are dealing with very complex, interdependent, feedback systems. To say anything rigorous about them, we need to idealize away, not only from things that matter in vivo, but things that may be relevant to the normal behavior of the very faculty we are discussing. The formal reason is that the mathematics needed to talk about the dynamics of complex systems is only recently developed, and is different from the mathematics of classical physics.

Conclusion

I very much support the detailed philosophical study of differences between different sciences. My point in this paper is that merely that this study ought to be guided by the details of the sciences studied, and not by a preconceived philosophical idea of what science should look like. When we do the latter, both philosophy and science labor under an unnecessary yoke.

Works Cited

Cartwright, Nancy. 1983. *How the Laws of Physics Lie*. Oxford University Press.

Hempel, Carl G. 1945. "Studies in the Logic of Confirmation." *Mind*. Reprinted in Hempel, 1965.

Hempel, Carl G. 1965. *Aspects of Scientific Explanation*. Free Press.