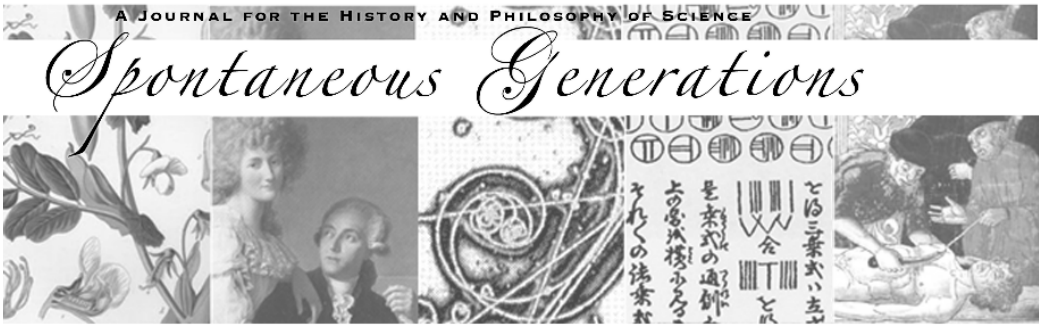

Theoretical Practices That Work: Those That Mimic Nature's Own

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FOCUSED DISCUSSION INVITED PAPER

Theoretical Practices That Work: Those That Mimic Nature's Own[*],[†]

Nancy Cartwright[‡]

I. THE PROBLEM

If the topic is realism about scientific theories, particularly highly abstract, general theories, what might the question be? It is, I shall argue, unfair to suppose that theories of this kind make claims since any way of rendering their equations and principles as propositions either makes them false or undersells their usefulness. So the question should not be: "Are the theory's claims true?" Instead I propose: "What image of the world makes intelligible the successes and failures of our theoretical practices?" The answer I propose: Nature does it just like us. Our successful theoretical practices work well for predicting and manipulating the world because they mimic Nature's own practices.

I have always been a kind of Wittgensteinian Tractarian about the world; an instrumentalist about high-level theories of it;[1] and an empiricist—a prudent empiricist—about warrant. As to theory: it is best seen as a tool we use to produce descriptions of what happens in concrete situations. As to the world: there are just the facts, which involve concrete things, features, relations, processes, powers, and happenings. Possibilities are real but they depend entirely on the facts and on how Nature brings them about, and Nature, I suggest, brings them about in the same way that we predict them. As to warrant: claims about the empirical world are warranted by the facts; and when two sets of hypotheses are supported by the same facts, the less bold hypothesis is the better warranted.

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[†] It will be obvious how much my thinking owes to Bas van Fraassen and Hasok Chang; for concern with details, to Pat Suppes; for finding the possible in the actual, to Ruth Marcus; and for the toolbox of science, to my co-authors Towfic Shomar and Mauricio Suarez (Cartwright, Shomar, and Suarez 1995).

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[1] At least in the disciplines I have studied.

We use our theories to build models of concrete systems for prediction, manipulation, and explanation, all of which, when the model is successful, involve well-warranted propositions.[2] The (usually complicated) propositions that describe the world as the model models it use irreducibly "theoretical" concepts. For example, any detailed description of what happens when a laser emits a beam of coherent light will refer to electrons, photons, quantum energy, and excited states, and at some stage should employ some version of Schrödinger's equation. That secures an image of the world replete with many of the quantities, kinds, qualities, and processes named by our theoretical concepts.

This makes trouble for instrumentalism. It does not seem that theory can be just an instrument, like a barometer or a pocket calculator. These theoretical terms are meaningful, with considerable evidence that they refer, and the equations play a role *as equations* in constructing our best descriptions of the facts. Van Fraassen's constructive empiricism provides a "middle" way to finesse this problem: read the claims of theory literally, but you need not count them true; the theory is acceptable just in case its empirical consequences are correct.

That, I urged long ago with arguments that I still endorse, will not do. First note that what we call the "principles" of a theory are seldom propositions. In the mathematical sciences they are frequently equations; in the non-mathematical sciences, sentences without scope indicators.[3] Despite decades of efforts (see below), I have never found a way to render them as propositions so that the principles of our best theories turn out well-warranted. My current view is the same, after all this effort, as at the start. These theories will not prove acceptable by realist standards nor by the standards of constructive empiricism. That is because theories, if taken literally, do not get their empirical consequences right, for three reasons:

> a. Theoretical equations are usually turned into propositions by adding universal quantifiers.[4] But we have evidence against this universalism—myriads of cases where we cannot get the equations to fit the measured values. Universalists explain by saying there must be factors present that aren't represented; were they represented, the equations would hold. This is a bold conjecture. An alternative explanation is that the equations just don't fit those cases.

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[2] *How the Laws of Physics Lie* called these "phenomenological models" (Cartwright 1983).

[3] As for instance in "[S]treaming ... costs very little, improves test scores, and is therefore extremely cost-effective." From the J-PAL website (cited in full in "References").

[4] For example, "$\nabla \times \mathbf{E} = -\delta \mathbf{B}/\delta t$" becomes "For all systems x, $\nabla \times \mathbf{E}(x) = -\delta \mathbf{B}(x)/\delta t$".

> b. In a great many cases where the equations are used to describe what happens, they are amended from their original form by "correction factors." They don't literally describe what happens, they only begin to do so.
>
> c. Much of science works by the analytic method: we pair representations of separate parts with rules of combination[5] to calculate what happens when the parts are arranged together. How are we to turn the representations for the parts into true propositions? The familiar case is forces. The law of gravity becomes "$(x)(y)\ F(y) = Gm(x)m(y)/\mathbf{r}_{xy}{}^2$"; Coulomb's principle, "$(x)(y)\ F(y) = \epsilon_0 q_1(x)q_2(y)/\mathbf{r}_{xy}{}^2$." For some systems y that have both mass and charge, $F(y) = 0$. So these propositions are not true.[6]

These constitute specific reasons that these high-level theoretical principles rendered as claims in the canonical way are not warranted: they don't have the right relationship to more "low-level" claims about concrete situations that are well-warranted for warrant to flow up to them. Yet they do not seem mere instruments to construct descriptions of concrete behaviours in which their form and content are completely invisible, like "$F(y) = 0$ in this setting" and "Q-switching in this laser produces pulse durations on the order of nanoseconds." So here is the task for scientific realism as I reconstruct it: to identify theoretical claims that are well-warranted and that preserve something of the form and content of the theory's principles. The key to accomplish this is to give far more central prominence to theoretical *practices.*

II. Realist proposal

With theoretical practice centre stage, if we want to find some well-warranted claims that resemble the equations and principles we write in our theories, I propose that we take seriously what we say when we use these theoretical principles *in situ.* This will secure an open-ended set of true situation-specific theoretical claims in which our theoretical principles appear much as we represent them. Here's a simple illustration, a situation-specific claim in which the functional form of Coulomb's principle appears explicitly: "If in this setting, charge q_1 is added this way here, charge q_2 located $\mathbf{r}$ away from it will experience an additional force $\epsilon_0 q_1(x)q_2(y)/\mathbf{r}_{xy}{}^2$."

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[5] These are often an open-ended set of "rules of thumb" that get adjusted in different ways in practical application, unlike the fixed and explicit rule of vector addition for forces.

[6] This is in stark contrast with the simultaneous equations models of economics where each and every equation must be true at once.

Though this claim sounds almost identical to Coulomb's principle, it is situation-specific. It would not be true for instance if q_1 were introduced in the middle of a Faraday cage.[7] I'll expand on this in Section 5. Before that I shall explain how my more direct attempts to turn these principles into credibly true propositions have failed. This takes us on a detour through powers, which is one way of arriving at my overall conclusion. But if you don't like powers, you can still adopt the "realist" position I propose.

III. FAILED ALTERNATIVES

Here is one approach I've tried to deal with problem (a). Turn an equation, say "$y = g(z_1,...z_n)$," into a proposition not by adding universal quantifiers across all systems but rather with something like: "For any situation s, so long as all the features that fix the value of y in s are represented by $z_1,...z_n$, then $y = g(z_1,...z_n)$ in s." This allows but does not require that the zs always pick up all the determinants of y. But it undersells the force of the equations. There are many cases where the equations play a significant role in building a model even where the zs do not represent all the determinants of y, or at least we don't recognise how to use them to do so.

This brings us to (b) since many of the corrections may be needed because there are factors at work that we can't represent in the theory. I have never—until now—come up with a reasonable "realist" approach for dealing with (b).

For (c) we can call on powers, or as I say, "capacities" (Cartwright 1989). Hume rejected powers because he could find no difference between the presence of a feature and its acting, as there should be if the feature were a power.[8] The analytic sciences seem to populate the world with powers in this sense. There's the presence of a charge, the Coulomb force it exerts on another charge when the two are appropriately arranged, and the total force the second charge experiences. At first I called these intermediates, the "influence" or "contribution" the power makes to the actual outcome. For example, "When q_1's Coulomb power is activated, it produces a contribution $F_c = \epsilon_0 q_1(x) q_2(y)/\mathbf{r}_{xy}{}^2$ to the total force a charge q_2 located $\mathbf{r}$ away from it experiences." But this is misleading. It sounds as if the contribution is there in the same way as the outcome, like the stones in a wall. I don't find that plausible even with forces, which do not add but combine vectorially—let alone where more complicated rules of combination apply.[9] John Pemberton

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[7] Faraday cages block electric fields.

[8] Or its occurrence was in some other way associated with the presence of a power.

[9] For instance, resistances (say R_1 and R_2) from different resistors combine differently if the resistors are arranged in series ($R_{total} = R_1 + R_2$) or in parallel ($R_{total} = R_1R_2 / (R_1 + R_2)$), and their effect on the current in a circuit with one battery is given by

and I tried calling these "exercisings" (Cartwright 2015; Cartwright and Pemberton 2013) since it seems that powers are always active in contributing to outcomes. But this seems a cheat. What does it mean to say, "When the Coulomb power acts, it exercises $\epsilon_0 q_1(x)q_2(y)/\mathbf{r}_{xy}{}^2$-ly?" And what can we possibly say about a resistor labelled with "resistance = 5 Ohms"? "It resists 5-Ohmly"?

Both Pemberton and I now think all this is a mistake. When powers exercise they always exercise in some *actual situation*. Yet we think of our representations of them as situation-free, as if they could act "nowhere" or in a world where they occur "all alone." It is easy to get sucked into this by concentrating on forces, which we may suppose we imagine operating in an "idealisation" of a *real* situation: one where the components are present—say both masses—located physically with respect to each other, but no other forces obtain. So we say, as I myself have, "In this situation we see what gravity does *on its own*." Then we suppose that, in other situations, gravity is "trying to do *the same thing*." But what is the cash value of this? It just means that the actual force that occurs when gravity acts is whatever other force is there vector-added to what gravity produces "on its own." Even this much sense cannot be made for other powers. In economics we talk of the powers of the supply mechanism and of the demand mechanism to affect price and we represent these powers in separate equations. But it makes no sense to imagine either operating "on its own." And I don't even know how to start to think about resistances in no circuit whatsoever.

We are also misled into positing contributions by thinking of powers as dispositions that need a conditional analysis: "X is water-soluble iff, if x is placed in water in the right way, x dissolves." Similarly, as above: "Q_1 has the Coulomb power iff, when activated in the right way, it produces a contribution $F_c = \epsilon_0 q_1(x)q_2(y)/\mathbf{r}_{xy}{}^2$ to the total force a charge q_2 located $\mathbf{r}$ away from it experiences." But powers, so I argue, are part of our basic ontology. We don't need contributions because powers don't need conditional analysis. They don't need analysis at all; they just are—lots and lots of them. Looking just to physics, there's gravity, the Coulomb power, friction, the power to resist the flow of electricity, the power to turn on an axis, and so on and so on and so on. There's much to say about powers as a category and there's much to say about specific powers. Naturally we pick out different powers by what they do in various circumstances, or what they look like,

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Ohm's law using R_{total}. But with two batteries and 3 resistors or simple bridge circuits, let alone really complex circuits, we end up using a toolkit of methods to calculate the total effect on current, including various reduction schemes and sets of simultaneous equations. For further examples, including the role of the Coulomb law in determining ground state carbon energy levels, which I discuss in Section 5, see Cartwright (2003, Essay 3).

or how they affect measuring instruments. But that's how we identify which is which. None of that constitutes an analysis, which is fine because things don't need analyses in order to be real.

So I propose to drop talk of "contributions," "exercisings," "intermediates." Of course when a power acts, it does something. But there is not some *one* thing it does. Rather, we have a way of *representing* the power, a representation we know how to use to calculate what happens when that power acts in various arrangements. So I have not solved problem (c) because I have no candidate proposition we might regard as true to associate with the principles for components parts in theories where the analytic method is employed. Still it advances my overall project, but only given a new story about how Nature decides, occasion by occasion, what happens.

IV. How Nature builds what happens

Here's an old story. There are true universal laws from which what happens in every situation can be deduced. Since the laws are true, what they say happens is what does happen. So Nature looks at the laws and produces what they say. I can't use this story. I have no laws, just a large battery of different powers that we represent in our theories and for which we have theoretical practices that allow us to calculate what happens when these powers act in various arrangements. What image of Nature makes sense of the successes and failures of these practices?

I propose that Nature does it just the way we do it, piecemeal and with no firmer rules than we employ. Why suppose that Nature employs a tight system rather than a loose one like ours? This is by far the most straightforward account of how our practices succeed: they replicate Nature's own practices.[10]

V. Back to realism

Here's how I see theory working when it works well: we develop sets of theoretical practices and representations that together allow us (using our theoretical representations as representations, not as claims) to generate situation-specific truths. These truths are true because Nature generates the situation-specific facts in the same way that we do. Or rather, this works for us because we have cottoned on to how Nature does it. As I noted in Section 1, generally our theoretic representations—the theory's principles

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[10] This leaves space for genuine indeterminacy: what's open in Nature is much like what is open so far as we can calculate with a really good theory. I welcome this since it is how I experience the world. But it is not a necessary consequence of the view. For further discussion, see Cartwright and Merlussi (forthcoming).

and equations—are invisible in these situation-specific truths, though many of our theoretic concepts may appear in them. Sometimes, though, even the representation itself, much as it appears in our theories, appears in these claims.

For the pleasure of coming full circle and also to find a simple illustration, I'll consider the case of the five energy levels of the ground state of a carbon atom that I discussed in *How the Laws of Physics Lie* (Cartwright 2003, Essay 3).[11] Textbook treatments first derive three levels using only the Coulomb potential, then divide the ^{3}P state into three by adding terms to correct for spin effects. In *How the Laws of Physics Lie* I made a few attempts at formulating some true factual claims with the quantum Coulomb potential for the two 2p carbon electrons as cause and the three levels as effect but with no success. I finally proposed a counterfactual: "[T]he Coulomb potential, if it *were* the only potential at work, would produce the three levels" (Cartwright 2003, 69), which, I suppose, if it is to be true at all must be said of some particular carbon atom, say Carby. But what makes this counterfactual true? The answer is surely Schrödinger's equation and the quantum version of Coulomb's principle. But how do they do this, given that these cannot be rendered as true propositions from which the counterfactual can be derived?

The counterfactual is true and it depends on Schrödinger's equation and the quantum version of Coulomb's principle. But that's not because these are true laws and Nature makes happen what true laws say. It is rather because this is the way Nature operates. She uses both Coulomb's principle and Schrödinger's equation, but as representations, just as we do. She uses them to determine what energy levels Carby will display. We have ample evidence that this *is* how she operates. So we also have good warrant for the claim that if Carby's spin effects were missing, she would see to it that Carby displayed the three levels in question.

There are also a great many true factual claims. Supposing Carby has been prepared for a finely-tuned experiment to measure energy levels (as in n. 11 above), Schrödinger's equation in almost its pure form (i.e., with few "correction factors"), with both the Coulomb term and a spin-orbit term in it, will be true of Carby. This too is true for Carby: the Coulomb potential is literally an additive part of the potential that determines Carby's ground state energy levels. Of course these two claims would be available given my solution to problem (a). But we should want far more than (a) provides for both the Schrödinger equation and Coulomb's principle since we use them

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[11] Here I depart from my own standards in aid of providing a simple illustration. This is not any real carbon atom but itself a tool we use to construct models of real carbon atoms. Nevertheless, let's pretend that it is some real carbon atom, perhaps one whose fine-structure intervals are being measured by laser magnetic resonance, as for instance in the experiment by Saykally and Evenson (1980).

for a vast number of cases where correction factors must be included.

VI. Conclusion

I suggest that Nature does not derive what is to happen from some set of true claims. She does it just as we do, from representations and practices like ours. This surely should be counted as a kind of realism. Certainly it renders the principles of a successful theory true in the sense of pragmatic truth that Hasok Chang defends in this volume. But I think this image of Nature permits a more robust realism about our theoretical representations. First, many of their terms refer. Second, Nature uses them for fixing what happens in the very form in which they appear in our theories, just as we use them in the process of predicting those happenings. Third, they are associated with a great many situation-specific truths in which their form appears almost unchanged. Finally, returning to my prudent empiricism, this is an image of Nature that the successes of our theories and their practices supports, unlike one in which Nature employs claims we can't formulate and methods for which we have no use.

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