On the (dis)unity of the sciences | Scientia Salon xscience xphysics xbio xreductionism

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by Massimo Pigliucci

As a practicing scientist I have always assumed that there is one thing, one type of activity, we call science. More importantly, though I am a biologist, I automatically accepted the physicists' idea that — in principle at the least — everything boils down to physics, that it makes perfect sense to go after a "theory of everything."

Then I read John Dupré's The Disorder of Things: Metaphysical Foundations of the Disunity of Science [1], and that got me to pause and think (which, of course, is the hallmark of a good book, regardless if one rejects that book's conclusions).

I found John's book compelling not just because of his refreshing, and admittedly consciously iconoclastic tone, but also because a great deal of it is devoted to subject matters, like population genetics, that I actually know a lot about, and am therefore in a good position to judge whether the philosopher got it right (mostly, he did).

Dupré's strategy in The Disorder of Things is to attack the idea of reductionism by showing how it doesn't work in biology. The author rejects both the notion of a unified scientific method (a position that is nowadays pretty standard among philosophers of science), and goes on to advocate a pluralistic view of the sciences, which he claims reflects both what the sciences themselves are finding about the world (with a multiplication of increasingly disconnected disciplines and the production of new explanatory principles that are stubbornly irreducible to each other), as well as a more sensible metaphysics (there aren't any "joints" at which the sciences "cut nature," so that there are a number of perfectly equivalent ways of thinking about the universe and its furnishings).

But this essay isn't primarily about John's book. Rather, it took form while I re-read Jerry Fodor's classic paper, "Special sciences (or: the disunity of science as a working hypothesis)" [2], together with Nancy Cartwright's influential book, How the Laws of Physics Lie [3] — both of which came out before The Disorder of Things and clearly influenced it. Let me explain, beginning with Fodor, and moving then to Cartwright.

Fodor's target was, essentially, the logical positivist idea (still exceedingly common among scientists, despite the philosophical demise of logical positivism a number of decades ago) that the natural sciences form a hierarchy of fields and theories that are (potentially) reducible to each next level, forming a chain of reduction that ends up with fundamental physics at the bottom. So, for instance, sociology should be reducible to psychology, which in turn collapses into biology, the latter into chemistry, and then we are almost there.

But what does "reducing" mean, anyway? [4] At the least two things (though Fodor makes further technical distinctions, you'll have to check his original article): let's call them ontological and theoretical.

Ontologically speaking, most people would agree that all things in the universe are made of the same substance (the exception, of course, are substance dualists), be it quarks, strings, branes or even mathematical relations [5]; moreover, complex things are made of simpler things. For instance, populations of organisms are nothing but collections of individuals, while atoms are groups of particles, etc. Fodor does not object to this sort of reductionism, and neither do I.

Theoretical reduction, however, is a different beast altogether, because scientific theories are not "out there in the world," so to speak, they are creations of the human mind. This means that theoretical reduction, contra popular assumption, does most definitely not logically follow from ontological reduction. Theoretical reduction was, of course, the holy grail (never achieved) of logical positivism: it is the ability to reduce all scientific laws to lower level ones, eventually reaching a true "theory of everything," formulated in the language of physics. Fodor thinks that this too won't fly, and the more I think about it, the more I'm inclined to agree.

Now, typically when one questions theory reduction in science one is faced with both incredulous stares and a quick counter-example: but look at chemistry! It has successfully been reduced to physics! Indeed, there basically is no distinction between chemistry and physics! Turns out that there are two problems with this move: first, the example itself is questionable; second, even if true, it is arguably more an exception than the rule.

As Michael Weisberg and collaborators write in the Stanford Encyclopedia of Philosophy entry on the Philosophy of Chemistry [6]: "many philosophers assume that

fodor special sci...

SPECIAL SCIENCES (OR: THE DISUNITY OF SCIENCE AS A WORKING HYPOTHESIS)* A typical thesis of positivistic philosophy of science is that all true theories in the special sciences should reduce to physical theories in the long run. This is intended to be an empirical thesis, and part of the evidence which supports it is provided by such scientific successes as the molecular theory of heat and the physical explanation of the chemical bond. But the philosophical popularity of the reductivist program cannot be explained by reference to these achievements alone. The development of science has witnessed the proliferation of specialized disciplines at least as often as it has witnessed their reduction to physics, so the wide spread enthusiasm for reduction can hardly be a mere induction over its past successes. I think that many philosophers who accept reductivism do so primarily because they wish to endorse the generality of physics vis ? vis the special sciences: roughly, the view that all events which fall under the laws of any science are physical events and hence fall under the laws of physics.1 For such philosophers, saying that physics is basic science and saying that theories in the special sciences must reduce to physical theories have seemed to be two ways of saying the same thing, so that the latter doctrine has come to be a standard construal of the former. In what follows, I shall argue that this is a considerable confusion. What has traditionally been called 'the unity of science' is a much stronger, and much less plausible, thesis than the generality of physics. If this is true it is important. Though reductionism is an empirical doctrine, it is intended to play a regulative role in scientific practice. Reducibility to physics is taken to be a constraint upon the acceptability of theories in the special sciences, with the curious consequence that the more the special sciences succeed, the more they ought to disappear. Methodological problems about psychology, in particular, arise in just this way: the assumption that the subject-matter of psychology is part of the subject-matter of physics is taken to imply that psychological theories must reduce to physical theories, and it is this latter principle 98 J. A. FODOR that makes the trouble. I want to avoid the trouble by challenging the inference chemistry has already been reduced to physics. In the past, this assumption was so pervasive that it was common to read about "physico/chemical" laws and explanations, as if the reduction of chemistry to physics was complete. Although most philosophers of chemistry would accept that there is no conflict between the sciences of chemistry and physics, most philosophers of chemistry think that a stronger conception of unity is mistaken. Most believe that chemistry has not been reduced to physics nor is it likely to be." You will need to check the literature cited by Weisberg and colleagues if you are curious about the specifics, but for my purposes here it suffices to note that the alleged reduction has been questioned by "most" philosophers of chemistry, which ought to cast at least some doubt on even this oft-trumpeted example of theoretical reduction. (Oh, and closer to my academic home field, Mendelian genetics has not been reduced to molecular genetics, in case you were wondering [7].)

The second problem, however, is even worse. Here is how Fodor puts it, right at the beginning of his '74 paper:

"A typical thesis of positivistic philosophy of science is that all true theories in the special sciences [i.e., everything but fundamental physics, including non-fundamental physics] should reduce to physical theories in the long run. This is intended to be an empirical thesis, and part of the evidence which supports it is provided by such scientific successes as the molecular theory of heat and the physical explanation of the chemical bond. But the philosophical popularity of the reductivist program cannot be explained by reference to these achievements alone. The development of science has witnessed the proliferation of specialized disciplines at least as often as it has witnessed their reduction to physics, so the wide spread enthusiasm for reduction can hardly be a mere induction over its past successes."

I would go further than Fodor here, echoing Dupré above: the history of science has produced many more *divergences* at the theoretical level — via the proliferation of new theories within individual "special" sciences — than it has produced successful cases of reduction. If anything, the induction goes the other way around!

Indeed, even some scientists seems inclined toward at least some bit of skepticism concerning the notion that "fundamental" physics is so, well, fundamental. (It is, of course, in the trivial ontological sense discussed above: everything is made of quarks, or strings, or branes, or whatever.) Remember the famous debate about the construction of the Superconducting Super Collider, back in the '90s? [8] This was the proposed antecedent of the Large Hadron Collider that recently led to the discovery of the Higgs boson, and the project was eventually nixed by the US Congress because it was too expensive. Nobel physicist Steven Weinberg testified in front of Congress on behalf of the project, but what is less known is that some physicists testified *against* the SSC, and that their argument was based on the increasing *irrelevance* of fundamental physics to the rest of physics — let alone to biology or the social sciences.

Hard to believe? Here is how solid state physicist Philip W. Anderson put it already in 1972 [9], foreshadowing the arguments he later used against Weinberg at the time of the SSC hearings: "the more the elementary particle physicists tell us about the nature of the fundamental laws, the less relevance they seem to have to the very real problems of the rest of science." So much for a fundamental theory of *everything*.

Back to Fodor and why he is skeptical of theory reduction, again from his '74 paper:

"If it turns out that the functional decomposition of the nervous system corresponds to its neurological (anatomical, biochemical, physical) decomposition, then there are only epistemological reasons for studying the former instead of the latter [meaning that psychology couldn't be done by way of physics only for practical reasons, it would be too unwieldy]. But suppose there is no such correspondence? Suppose the functional organization of the nervous system cross cuts its neurological organization (so that quite different neurological structures can subserve identical psychological functions across times or across organisms). Then the existence of psychology depends not on the fact that neurons are so sadly small, but rather on the fact that neurology does not posit the natural kinds that psychology requires." [10]

Just before this passage in the same paper, Fodor argues a related, even more interesting point:

"If only physical particles weren't so small (if only brains were on the outside, where one can get a look at them), then we would do physics instead of paleontology (neurology instead of psychology; psychology instead of economics; and so on down). [But] even if brains were out where they can be looked at, as things now stand, we wouldn't know what to look for: we lack the appropriate theoretical apparatus for the psychological taxonomy of neurological events."

The idea, I take it, is that when physicists like Weinberg (for instance) tell me (as he actually did, during Sean Carroll's naturalism workshop [11]) that "in principle" all knowledge of the world is reducible to physics, one is perfectly within one's rights to ask (as I did of Weinberg) what principle, exactly, is he referring to. Fodor contends that if one were to call up the epistemic bluff the physicists would have no idea of where to even begin to provide a reduction of sociology, economics, psychology, biology, etc. to fundamental physics. There is, it seems, no known "principle" that would guide anyone in pursuing such a quest — a far more fundamental issue from the one imposed by merely practical limits of time and calculation. To provide an analogy, if I told you that I *could*, given the proper amount of time and energy, list all the digits of the largest known prime number, but then decline to actually do so because, you know, the darn thing's got 12,978,189 digits, you couldn't have any principled objection to my statement. But if instead I told you that I can prove to you that there is an infinity of prime numbers, you would be perfectly within your rights to ask me at the least the outline of such proof (which exists, by the way), and you should certainly not be content with any vague gesturing on my part to the effect that I don't see any reason "in principle" why there should be a limit to the set of prime numbers.

Fine, but does anyone have any positive reasons to take seriously the notion of the impossibility of ultimate theory reduction, and therefore of the fundamental disunity of science (in theoretical, not ontological, terms)? Nancy Cartwright does (and so does Ian Hacking, as exemplified in his Representing and Intervening [12]). Cartwright has put forth a view that in philosophy of science is known as theory anti-realism [13], which implies a denial of the standard idea — almost universal among scientists, and somewhat popular among philosophers — that laws of nature are (approximately) true generalized descriptions of the behavior of things, especially particles (or fields, doesn't matter). Rather, Cartwright suggests that theories are statements about how things (or particles, or fields) *would* behave according to idealized models of reality.

What's the big deal? That our idealized models of reality are not true, and therefore that — strictly speaking — laws of nature are false. Of course the whole idea of laws of nature (especially with their initially literal implication of the existence of a law giver) has been controversial since it was championed by Descartes and opposed by Hobbes and Galileo [14], but Cartwright's rather radical suggestion deserves a bit of a hearing, even though one may eventually decide against it (I admit to being a sympathetic agnostic in this regard).

Cartwright distinguishes between two ways of thinking about laws: "fundamental" laws are those postulated by the realists, and they are meant to describe the true, deep structure of the universe. "Phenomenological" laws, by contrast, are useful for making empirical predictions, and they work well enough for that purpose, but strictly speaking they are false.

Now, there are a number of instances in which even physicists would agree with Cartwright. Take the laws of Newtonian mechanics: they do work well enough for empirical predictions (within a certain domain of application), but we know that they are false if they are understood as being truly universal (precisely because they have a limited domain of application). According to Cartwright, *all* laws and scientific generalizations, in physics as well as in the "special" sciences are just like that, phenomenological.

Funny thing is that some physicists — for example Lee Smolin [15] — seem to provide support for Cartwright's contention, to a point. In his delightful The Trouble with Physics Smolin speculates (yes, it's pretty much a speculation, at the moment) that there are empirically intriguing reasons to suspect that Special Relativity "breaks down" at very high energies [16], which means that it wouldn't be a law of nature in the "fundamental" sense, only in the "phenomenological" one. (Smolin also suggests that General Relativity may break down at very large cosmological scales [16].)

But of course there are easier examples: as I mentioned above, nobody has any clue about how to even begin to reduce the theory of natural selection, or economic theories, for instance, to anything below the levels of biology and economics respectively, let alone fundamental physics.

If Cartwright is correct, then, science is fundamentally disunified, and its very goal should shift from seeking a theory of everything to putting together the best patchwork of local, phenomenological theories and laws, each one of which, of course, would be characterized by its proper domain of application.

Here is how Cartwright herself puts it, concerning physics in particular: "Neither quantum nor classical theories are sufficient on their own for providing accurate descriptions of the phenomena in their domain. Some situations require quantum descriptions, some classical and some a mix of both." And the same goes, a fortiori, for the full ensemble of scientific theories, including all those coming out of the special sciences.

So, are Dupré, Fodor, Hacking and Cartwright, among others, right? I don't know, but it behooves anyone who is seriously interested in the nature of science to take their ideas seriously, without dismissing them out of hand. We have already agreed that it is impossible to achieve reduction from a pragmatic epistemic perspective, and we have seen that there are good reasons to at the least entertain the idea that disunity is fundamental, not just epistemic. True, we have also agreed to the notion of ontological reduction, but I have argued above that there is no logically necessary connection between ontological and theoretical reduction, and it is therefore a highly questionable leap of (epistemic) faith to simply assume that because the world is made of one type of stuff therefore there must be one fundamentally irreducible way of describing and understanding it. Indeed, ironically it is the anti-realists who claim the mantle of empiricism to buttress their arguments: the available evidence goes against the idea of ultimate theory reduction (it can't be done in most cases, and the number of theories to reduce is increasing faster than the number of successful reductions achieved so far), so it is a metaphysically inflationary (i.e., unnecessary and undesirable) move to assume that somehow such evidence is deeply misleading. And most physicists wouldn't be caught dead admitting that they are engaging in metaphysics…

Massimo Pigliucci is a biologist and philosopher at the City University of New York. His main interests are in the philosophy of science and pseudoscience. He is the editor-in-chief of Scientia Salon, and his latest book (co-edited with Maarten Boudry) is Philosophy of Pseudoscience: Reconsidering the Demarcation Problem (Chicago Press).

[1] The Disorder of Things: Metaphysical Foundations of the Disunity of Science, by J. Dupré, 1993.

[2] Special sciences (or: the disunity of science as a working hypothesis), by J. Fodor, Synthese, 1974.

[3] How the Laws of Physics Lie, by N. Cartwright, 1983.

[4] Scientific Reduction, by R. van Riel, Stanford Encyclopedia of Philosophy, 2014.

[5] Rationally Speaking podcast #69: James Layman on metaphysics; Rationally Speaking podcast #101: Max Tegmark on the mathematical universe hypothesis.

[6] Philosophy of Chemistry, by M. Weisberg et al., Stanford Encyclopedia of Philosophy, 2011.

[7] On the debate about the reduction of Mendelian to molecular genetics, see: Molecular Genetics, by K. Waters, Stanford Encyclopedia of Philosophy, 2007.

[8] Superconducting Super Collider, Wiki entry.

[9] More Is Different, by P. W. Anderson, Science, 177:393-396, 1972.

[10] A "natural kind" in philosophy is a grouping of things that is not artificial, that cuts nature at its joints, as it were. A typical example is a chemical element, like gold. See: Natural Kinds, by A. Bird, 2008, Stanford Encyclopedia of Philosophy. Notice that Fodor here is in tension with Dupré, since the latter denies the existence of natural kinds altogether.

[11] Moving Naturalism Forward, an interdisciplinary workshop, 25-29 October 2012.

[12] Representing and Intervening: Introductory Topics in the Philosophy of Natural Science, by I. Hacking, 1983.

[13] Which she couples with "entity" realism, the idea that unobservable entities like genes and electrons are (likely) real. This position is therefore distinct, and in between, the classical opposites of scientific realism (about both theories and entities) and scientific anti-realism (about both theories and entities). See: Scientific Realism, by A. Chakravartty, Stanford Encyclopedia of Philosophy, 2011, and Constructive Empiricism, by B. Monton and C. Mohler, Stanford Encyclopedia of Philosophy, 2012.

[14] Are there natural laws?, by M. Pigliucci, Rationally Speaking, 3 October 2013.

[15] The Trouble with Physics: The Rise of String Theory, the Fall of a Science, and What Comes Next, by L. Smolin, 2006.

[16] For Special Relativity, see chapter 13 of Smolin's book. This has to do with the so-called GZK prediction, which represents a test of the theory at a point approaching Planck scale, where quantum mechanical effects begin to be felt. Regarding General Relativity, the comment is found in chapter 1.

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