The Promise of Roberts' "Measurability Account of Laws"

In the current philosophical discussion of the laws of nature, there is a disturbing disconnect between the philosophers who claim to be theorizing about the laws of nature and the scientists who are presumably engaged in the discovery of these laws. This disconnect is most forcefully demonstrated by the number of propositions put forward by philosophers as laws of *T* that no scientist in the field *T* would accept as a law of *T*. It might be suggested that not much turns on this, but no suggestion could be of a greater disservice to the philosophical endeavor. These laws that aren't are often offered as a premise in an argument that goes something like this:

1. Philosopher *A* has a metaphysical theory of the laws of nature.
2. Proposition *P* is a law of *T*.
3. This law *P* has certain metaphysical implications.
4. These metaphysical implications are contrary to *A*'s metaphysical theory of the laws of nature.

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5. Therefore, *A*'s metaphysical theory of the laws of nature is incorrect.

Now clearly, it is the obligation of the philosopher putting forward any argument to offer, to the best of his/her ability, a sound argument. If a philosopher offers this type of argument and law *P* isn't, then the argument is not sound. But it is equally clear that when presented with an unsound argument, a person who is serious about philosophy has an obligation of generosity to do what s/he can to fix the argument. For example, if *P* is, in fact, *not* a law of *T*, the serious philosopher will cast about for a *P'* that is a law of *T* that has the metaphysical implications necessary for the validity of the argument. But it is not only naive, but indeed dangerous to the philosophical endeavor to always assume on the behalf the philosopher offering the argument, as a matter of course, that there is such a *P'*. In any case, the primary burden for the soundness of an argument rests with the person *offering* the argument, not the person *reading* it.

The philosophical literature about the laws of nature is replete with such laws that aren't. Sometimes these laws of *T* that aren't are so blatantly not that the obligation of generosity simply doesn't obtain - an example of this kind is Nancy Cartwright's law of biology regarding gymnotoids [Cartwright, pg 54-55]. Indeed, any reader with even a passing familiarity with science or what philosophers think must be true of a law, whether it is a law of nature or *merely* a law of a particular science, ought to be given pause by this example. A more difficult example, difficult because it requires a knowledge that goes beyond the philosophical literature of the laws of nature is Bas C van Fraassen's law of radioactive stability for single particles [van Fraassen, pg 109-113], the metaphysical implications of which he uses to attack Armstrong's metaphysical account of natural laws. But van Fraassen is wrong about what the probability of stability of a single radioactive nucleus is, as a reading of the first few sentences of the section covering radioactive decay present in *any* first-year, calculus-based physics text will demonstrate. The correct physics of radioactive particle stability does not have the metaphysical implications van Fraassen needs to make good his objection, and I am aware of no law of physics that does.

So why is the philosophical literature so full of bad science? The answer is sadly obvious: the philosophers writing it are talking to themselves and not to the scientists whose 'laws' they are using as touchstones to motivate their metaphysics. Surely scientists have grappled with the nature of what it is we all take them to be studying and hoping to discover: the laws of nature[1]. And yet a perusal of the bibliography of the literature is remarkably devoid of references to scientists writing about the nature of natural laws. One might respond that this is natural and right as philosophers are interested in the laws of nature, which is a *meta*physical issue, not a merely physical one[2]. But this is absurd.

There appears to be a consensus amongst philosophers that if we know any laws of nature at all, their number is small. Even so, there is also a consensus that physics, of all the sciences, is the most likely to discover the laws of nature, and that what laws of nature we do know, are most likely laws of physics. The ultimate laws of physics may be radically different from those that we now hold to be true, but the philosophically responsible course of action is to work from those things, that to the best of our knowledge at this time, describe and the explain the workings of the universe, and to not worry ourselves overly that this will turn out not to be the case. If it does, then there will be time to revise our metaphysics based upon our new understanding of the universe when we have come to this new understanding of it.

In the meantime, the other sciences do not appear to offer laws of the same sort that physics offers us[3] , so the laws of physics may only be a subset of the laws of nature. But it seems clear that if *nothing* we say for metaphysical reasons must be true of the laws of nature appears not to be true of the laws of physics, then we, as philosophers, are clearly doing something wrong. And if the metaphysical implications of the laws of physics don't dovetail with our metaphysical theories about the laws of nature, it seems it would be somewhat arrogant to tell the physicists that they are wrong about the laws of physics.

John Roberts suggests that our philosophical intuition about the nature of laws is summarized in what he calls the Common Philosophical Characterization (CPC):

> A law of nature is a true, (logically and mathematically) contingent generalization that supports counterfactuals, plays an important role in scientific explanation, and can be inductively confirmed by its instances. [Roberts, pg 2]

And though he does not make this claim explicitly, it seems clear that this intuition is deeply informed by the way physicists, and to an extent the researchers in other sciences, have chosen to express the regularities that they have observed in nature and the way in which they have chosen to express the laws that seem to explain these regularities. But as Roberts does write,

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1 Cartwright does reference [Cartwright, pg 56-62] just such a work, namely Richard Feynman's *The Character of Physical Law* [Feynman]. Unfortunately, Cartwright seems to thoroughly misconstrue Feynman's comments about the Law of Universal Gravitation and Coulomb's Law [Feynman, Chapter 1] in ways that cannot be considered here.

2 Which is not to imply in any way that only physics has anything to contribute to the discussion! I mean to take absolutely no stand whatsoever on the question of whether or not other sciences, *e.g.* chemistry, biology, psychology or economics, have the laws of nature as the proper subject of their inquiries and study. But I am a physicist by training, not a chemist or biologist, and certainly not a psychologist or economist, and I am extremely reluctant to speak on behalf of sciences with which I am not terribly familiar.

3 I mean here to allude to the issue of *ceteris paribus* laws, which I will not address further.

> But we should note that if we use the CPC to fix our usage of the term "law", then we have no *a priori* guarantee that there is anything interesting to say about all laws that goes beyond the CPC itself. [Roberts, pg 3]

If we, as philosophers fail to take this admonishment to heart, then we run the risk of having a consistent and coherent theory of the laws of nature whose connection to the actual laws of nature amounts to nothing more than the phrase "laws of nature" which appears in the moniker by which we refer to the theory.

Guided by the intuition of his CPC, Roberts offers a new and novel account of the laws of nature, a meta-theoretical account which he calls the "Measurability Account of Laws". Under this account, (1) a law *P* is a *law of nature* iff it is a law relative to some true theory and (2) it is a *law relative to theory T* iff *P* is implied by *T* and plays role *R* within *T*. Now, I do not mean to evaluate this meta-theoretic account of the laws of nature here for a number of reasons. First, the account is, by Roberts' own admission, incomplete for he has not yet an account of the role *R P* must play in *T* for *P* to be considered a law relative to *T* - this is not a fault of the account; it is merely in its developmental stages. Roberts does offer, however, a 'toy' physical theory with which he sketches what he thinks is a promising description of the *kind* of role *P* must play to be a law relative to any theory [Roberts, pg 13-18]. Second, it seems to me that his definition (2) of laws of nature is too broad, *i.e.* it's not clear to me that "1+1=2" won't turn out to be a law of nature under this account, but I think some sort of unobjectionable restriction on what *kinds* of theories or the *subject* matter of the theory considered under the category of the "true theories" of (2) can address this concern. Third, what really interests me in Roberts' account is that he points to the role *R*, even if he doesn't offer a complete account of this role, as the key to identifying the laws of a theory.

Roberts recognizes a obvious objection to his nascent meta-theoretical account that all it will be able to tell us is that "*P* is a law just in case some true theory says that *P* is a law" [Roberts, pg 7-8]. But in response he asks us to consider Molly [Roberts, pg 8-9], a competent undergraduate student who has mastered classical mechanics, and who is now taking classical electrodynamics. Roberts rightly claims that while many propositions and/or equations are identified as laws in the general classical mechanics text, this is not the case in many classical electrodynamics texts. And yet, as he also rightly claims, it is generally the case that a competent student like Molly, who has mastered classical mechanics, has no problem identifying those propositions and/or equations in her classical electrodynamics texts that are the laws of electrodynamics (*e.g.* Maxwell's equations, *etc*). Roberts' claim is that in addition to learning the laws of classical mechanics (*e.g.* Newton's laws of motion, *etc*) while she studied the subject, she learned something else, something quite important, namely, to identify the propositions and/or equations of other theories that are laws relative to those theories!

And it is this aspect of Roberts' account I wish to focus on, because I think it promises some relief from the ubiquity of bad science and laws that aren't which currently infect the philosophical literature regarding laws of nature. Philosophers are not scientists, and generally have no training as scientists, so perhaps it is not surprising that they apparently have such trouble separating the chafe from the wheat, and often accept propositions of a scientific field/theory *T* as laws that no scientist trained in *T* would. But if Roberts' meta-theoretical account gains purchase in the literature, perhaps it will serve to connect philosophers who claim to be theorizing about the laws of nature and the scientists who are presumably engaged in the discovery of these laws.

After all, what role *P* plays for scientists trained in *T* can be learned by learning *T* the way the scientists who learn *T* learn *T*, but as we've already observed, we are generally philosophers not scientists, and don't have time to *learn T*. But, of course, *R* can, presumably, be discovered empirically, that is by *talking* with those trained in *T*!

Now if Roberts is correct that one can learn to identify the *P′* that serve the law role *R′* in a theory *T′* by learning the *P* that serve the law role *R* in theory *T*, perhaps it is understandable that I, a person trained in physics but *not* biology, was given pause when I read Cartwright's claim that the following was a law of biology [Cartwright, pg 54-55 - all "[]" in this quote from the "Stanford text" are Cartwrights']:

> Biological laws provide good examples [of laws of nature, which describe facts about reality]. For instance, here is a generalization[4] taken from a Stanford text on chordates:
>
>> The gymnotoids [American knife fish] are slender fish with enormously long anal fins, which suggest the blade of a knife of which the head is a handle. They often swim slowly with the body straight by undulating this fin. They [presumably 'always' or 'for the most part'] are found in Central and Sourth America ... Unlike the characins they ['usually'?] hide by day under river banks or among roots, or bury themselves in sand, emerging only at night.[5]

This didn't strike me as a law of biology, much less something that would qualify under *any* philosopher's notion of a law of nature, not even the most naive regularity theorist. But as I've said, I don't have any training as a biologist, so my first impulse was to ask some biologists if they would consider this to be a law of biology, and if not, why not. I sent email to the 90 or so active faculty in the Division of Biological Sciences here at the University of Kansas asking if this was the case, and received 8 responses[6]. If I wanted to employ irony, I would say: *sadly*, 7 of those who responded said that this was most certainly *not* a law of biology. The eighth, Rudolf Jander wrote:

> If you define "Any regularity in nature that can be used to make predictions constitutes a natural law," then the answer is yes. However, the choice of the example below is ill advised, because very few people know anything about knife fish. A good example: If you find an animal species with feathers, then you can safely predict that its females lay eggs. In biology we have millions of such "laws."
>
> Note, however, your question is thoroughly ambiguous, because you are not telling how you would like to define the concept "natural law." The laws of logic tell us: It is impossible to draw crips [*sic*] conclusions from vague premises.

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[4] This, in fact, makes Cartwright sound like an *unbelievably* naive regularity theorist as she appears to count this as a biological law simply because it is a generalization!

[5] This is footnoted: R. McNeill Alexander, _The Chordates_ (Cambridge: Cambridge University Press, 1975), pg 179.

[6] The respondents were Professors David Alexander, Bruce Cutler, Larry Dean, Walter Dimmick, Rudolf Jander, Charles D Michener, Ray Pierotti and Richard O Prum.

Evidently, the biologists have thought some about what makes a proposition a law of biology, and in addition to being able to tell us what does and does actually not count as a law of biology, they might even have something to contribute to the metaphysical discussion of the laws of nature.

Does Cartwright offer this biological law that isn't as a premise in an argument of the form that I offered above? Yes, and the philosopher *A* is Hilary Putnam, to whom Cartwright attributes (though she implies that Putnam has never explicitly staked out) the metaphysical position that the equations of modern physics best, in comparison to the other sciences, represent facts about reality [Carftwright, pg 56]. Her argument is actually more convoluted than the model I offer above, but can be roughly shoehorned into the model:

1. Putnam has a metaphysical theory of the laws of nature which entails that no law of nature represents facts about reality.
2. "The gymnotoids..." is a law of biology.
3. "The gymnotoids..." represent facts about reality.
4. That "The gymnotoids..." represent facts about reality is contrary to Putnam's metaphysical theory of the laws of nature.
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5. Therefore, Putnam's metaphysical theory of the laws of nature is incorrect.

Cartwright seems to subscribe to a premise that is suppressed in my model argument, namely "A law of biology is a law of nature.", and with this premise, the argument seems to be valid. But the biologists here at the University of Kansas tell me that "The gymnotoids..." is not a law of biology. Thus Cartwright's argument is not sound, and she is not entitled to her rejection of Putnam's position.

On the other hand, I, as a physicist, had no trouble identifying van Fraassen's law of radioactive decay [van Fraassen, pg 109-113], which he uses to critique Armstrong's theory of probabilistic laws [Armstrong, pg 128-136], as a law that isn't. van Fraassen suggests that it would be useful to have a law that one might accept as such as a touchstone with to evaluate the acceptability of Armstrong's metaphysics. Unfortunately, van Fraassen's law of single atom stability isn't - no physicist who would offer van Fraassen's e^{-At} as the probability that a single atom would remain stable over a period of time *t*. van Fraassen writes,

> The new[[7]], probabilistic law of radioactive decay is that each single atom has a probability (depending on a decay constant *A*), namely e^{-At}. (pg 110)

van Fraassen's law of single atom stability looks like the exponential decay law: $N(t)=N_0e^{-At}$, where *A* is customarily referred to as the decay constant and the Greek letter lambda, λ, is

[7] van Fraassen claims that the exponential decay law was originally understood to be a deterministic law, but is no longer. If he meant 'as it applies to the decay of radionuclides', then perhaps he is, in some way, correct, but this law describes the decrease in any population when the decrease is proportional to the population, and there are many situations in which the mechanism of the population decrease is *not* indeteterministic, *e.g.* a bank account in which there are monthly fees that are a percentage of the current balance of the account. If he really means to limit his comment to the law as it applies to radioactive decay, I seriously doubt that he has his history correct - Madam Curie was only beginning to investigate radioactive decay in the last decade of the 19th century, and within in three decades, quantum mechanics, which offers a fairly good phenomenological account of radioactive decay, was fully developed. In any case, the exponential decay law was known to apply to populations whose decrease was deterministic long before it was applied to radioactive decay, just as it is to this day.

customarily used instead. The decay constant is related to the half-life of the decay, $t_{1/2}$, by the relation $A=ln(2)/t_{1/2}$. (*ln* is the natural log function, and is such that it is equal to 0 for x=1 and 1 for *x=e*, it approaches infinity as x grows without bound, it approaches negative infinity as x approaches 0, and is undefined for x less than 0.) If one substitutes this *A* into the decay law and evaluates the function at $t=t_{1/2}$, one gets:

$$N = N_0 e^{-(\ln(2)/t_{1/2})/t_{1/2}} = N_0 e^{-\frac{\ln(2)}{t_{1/2}} t_{1/2}} = N_0 e^{-\ln(2)} = \frac{N_0}{e^{\ln(2)}} = \frac{N_0}{2}$$

This gives the definition of the half-life, $t_{1/2}$; the half-life is the amount of time which must pass in order for half of the radioactive sample to decay. This law applies to macroscopic samples of radioactive nuclei, and no physicist would claim that it applies to individual or even relatively small numbers of radioactive nuclei. As a result, any conclusions van Fraassen draws from comparing Armstrong's metaphysics are, at best, highly suspect.

Before examining the ways in which this invalidates van Fraassen's critique of Armstrong, let's derive the exponential decay law[8] so that we can see why a physicist would reject van Fraassen's law of single particle stability. The decay of radioactive nuclei is what is known as a Poisson process, which is to say it is time-independent, independent of the age of any particular nucleus, and the probability that any given nucleus in a given state will decay is the same for all nuclei in that state. Contrary to van Fraassen's claim, predictions about the life-times of particular nuclei are impossible - it makes sense only to make predictions about relatively large numbers of nuclei. Again, contrary to van Fraassen, the probability that any given nuclei decays in a time interval *dt* is defined to be λdt, where λ (van Fraassen's *A*) is called the decay constant, and *dt* is an infinitesmal period of time. λdt is, correctly speaking, independent of time. Now, if one has *N(t)* nuclei at a given time *t*, then the number that will decay in a infinitesmal time *dt* is $-N(t)\lambda dt$, *i.e.*:

(1) $$dN(t) = -N(t)\lambda dt\,.$$

After dividing both sides by *N(t)*, one integrates both sides of this equation from *t=0,* when there are N_0 atoms, to *t,* when there are *N(t)* atoms:

$$\int_{N(t=0)=N_0}^{N(t)} \frac{dN(t)}{N(t)} = \int_{t=0}^{t} -\lambda dt$$

As, λ is assumed to be a constant, the integrals result in:

$$\ln(N(t)) - \ln(N_0) = -\lambda t\,,$$

and then one does some algebra:

$$\ln(N(t)) = \ln(N_0) - \lambda t$$

exponentiates both sides:

[8] A not too terribly technical discussion of the radioactive decay law can be found at http://www.phy.uct.ac.za/courses/phy300w/np/ch1/node30.html - I reproduce the relevant parts of it here.

$$e^{\ln(N(t))} = e^{\ln(N_0) - \lambda t} = e^{\ln(N_0)} e^{-\lambda t}$$

and finally one arrives at the exponential decay law:

$$N(t) = N_0 e^{-\lambda t},$$

where, as we said above,

(2) $$\lambda = \frac{\ln(2)}{t_{1/2}}$$

and $t_{1/2}$ is the half-life of the radionuclide.

Now, for the kind of long-lived radionuclide radium, which has a half-life of ~1600yrs (van Fraassen's example), direct measurement of the half-life would, quite obviously, be problematic. As a result, we arrive at the half-life by a means suggested by the derivation of the exponential decay law itself. If one divides both sides of (1) by *dt*, one gets:

$$\frac{dN(t)}{dt} = -\lambda N(t)$$

The left-hand side of this equation can be interpreted as the rate of change of the number of nuclei, and as the right-hand side is negative, this rate is negative, *i.e.* the number of radioactive nuclei is growing *smaller* with time. So assuming we can measure both the number of radionuclei at a given moment and then measure the rate of subsequent decay, we can calculate the decay constant:

$$\lambda = -\frac{\left(\frac{dN(t)}{dt}\right)}{N(t)}$$

The decay constant can then be used to calculate the half-life by inverting equation (2):

$$t_{1/2} = \frac{\ln(2)}{\lambda}$$

There are two key points to note here:

1. It is not, in general, the half-life, $t_{1/2}$, to which we have direct access, but instead the decay constant λ, and this is because many radionuclei have extremely long half-lives, some longer than the estimated lifetime of the universe!
2. It would be, in principle, impossible to determine the half-life of a radionuclide if we had only one specimen, but, contrary to van Fraassen, the half-life of an odd number of radioactive nuclei is perfectly well defined (see van Fraassen, last paragraph of pg 109) if the half-life is defined in terms of the decay constant, the measurement of which is utterly independent of whether or not there are an odd or even number of radionuclei present at any given time.

So if vanFrassen were right about the probability of single atom decay, the probability that a single radioactive atom decayed over a time interval *t* would be $1\text{-}e^{-At}$ - a radioactive atom can only remain stable or decay, so the probability that it decays plus the probability that does not must equal 1. But this makes the probability of decay a function of time, which experimental observation has demonstrated not to be the case. In particular, if the probability of decay were a function of time, it would make the half-life of a sample of radioactive particles dependent on the initial number of particles of the sample. It would also validate van Fraassen's claim that the half-life of a sample could only be well defined for samples having an even number of particles initially.

Because Armstrong does not allow for uninstantiated universals, van Fraassen considers the implications of the positive universals, (i) *remaining stable for interval t* and (ii) *decaying into radon within interval t*. Given his incorrect probability of stability of a single particle, e^{-At}, and Armstrong's theory of functional laws, which van Fraassen fails to mention in his critique[9], he concludes that Armstrong is committed to either (a) the existence of infinitely many radium atoms, one of which remains stable for a time *t* for each possible value of *t* in his bogus probability 'law', or (b) one radium atom that remains stable indefinitely, *i.e.* one which never decays. Likewise, if (ii), then van Fraassen claims Armstrong is committed to (c) the existence of infinitely many radium atoms, one which decays after time *t* for each possible value of *t*, (d) or there is a one that decays 'now', *i.e.* which remains stable not at all. Of course regardless of which of the two possible positive universal one chooses, there are infinitely many 'nows', so this requires infinitely many radium atoms. On Armstrong's behalf, van Fraassen appeals to apparently safe claim that there are finitely many radium atoms in the universe, and he allows that Armstrong can, therefore, eliminate the untenable options (a)-(c). But van Fraassen claims that Armstrong cannot avoid the commitment to the claim that there exists a radium nucleus which will never decay, and though this is obviously not a verifiable prediction, it is, nonetheless, a substantive prediction. Of which van Fraassen says [van Fraassen, page 113, last complete paragraph],

> This is a striking empirical deduction. It shows that Armstrong's reconstruction of probablilistic laws is not mere word-play, but has empirical consequences, which were not present in the law as heretofore understood. I do not say verifiable - it is no use to apply for a grant, to find the sempiternal atom - but concrete and strikingly general. For the argument would apply to any law which delineates objective probabilities as a positive function of time.

So van Fraassen's argument, *ala* my model is:

1. Armstrong has a metaphysical theory of the laws of nature which does not allow for uninstantiated laws.
2. "The probability that a single radioactive nuclei remains stable over an interval of time, *t*, is e^{-At}." is a law of physics.
3. This law of radioactive stability has the metaphysical implication that there exists that radioactive nuclei that will never decay.
4. It is absurd to believe that there is a nucleus (of each species) of radioactive decay that will never decay.

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5. Therefore, Armstrong's metaphysical theory of the laws of nature is incorrect.

[9] It would seem that Armstrong could disarm van Fraassen's critique quite handily by pointing out that there need not be instances of every instance of a functional law, *i.e.* not every value of variable or parameter of a functional law need be represented for the functional law itself to be instantiated.

Now if van Fraassen were right about the decay law of radionuclides, there might be reason to believe that Armstrong is engaged in more than "mere word-play", but unfortunately, he's not[10]. Given the correct probability of decay for any particular radionuclide (not just radium nuclei, of course), *i.e.* λdt, this prediction evaporates completely, because this differential is *not* a function of time, and thus there is no 'now', and therefore no infinitude of 'nows' or infinitude of times *t* for which there must a radium nucleus to decay. There is likewise no ground upon which to conclude that there is a nucleus that will never decay, and Armstrong's metaphysics of probabilistic laws makes no physical prediction. van Fraassen claims that this prediction is forced upon Armstrong because of Armstrong's denial of negative universals, but the problem arises not from Armstrong's denial of negative universals, but instead from van Fraassen's lack of understanding of the physics of radioactive decay, a misunderstanding that a first-year physics student could correct.

In conclusion, the current philosophical literature regarding the laws of nature is littered with laws of *T* that are, in fact, not laws of *T*, and arguments regarding the metaphysics of laws of various *T* that are not sound. Perhaps this helps explain a quote often attributed[11] to Richard Feynman[12]:

> The philosophy of science is about as useful to scientists as ornithology is to birds.

Presumably we do not do philosophy of science strictly for our own sakes as philosophers. Presumably we intend to offer something useful to science. But the scientist who is confronted with laws that aren't will most likely conclude that any claim that follows from such laws that aren't will be of little value to him/her. And rightly so. Getting the science right is not all that difficult, if we, as philosophers, are willing to include scientists in our discussions about *their* endeavor. If Roberts is right and what is distinctive about a law of *T* is the role it plays in *T*, then including scientists in our discussion about the laws of nature may accomplish a number of things: it can only enrich the discussion, it will help remove some of the dross currently cluttering the discussion, and it holds out the promise that our metaphysical theory of the laws of nature will have more a connection to the laws of nature than the mere inclusion of the phrase "laws of nature" in its moniker.

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[10] This is a delightfully ambiguous way to put this. Of course, I mean the "he" to refer to van Fraassen, but since I'm not convinced, for reasons independent of van Fraassen's faulty critique, that Armstrong *is* engaged in nothing more than "mere word-play", putting it this way allows me to cover two bases at once!

[11] Noretta Koertge (editor), "A Plea for Science Studies," in *A House Built on Sand: Exposing Postmodernist Myths About Science*, Oxford Univ Press, 1998, pg 32 - the attribution is by Philip Kitcher; and Steven Weinberg, *Dreams of a Final Theory*, Pantheon, 1992, (in Chapter 7 - "Against Philosophy" in which Weinberg discusses "the unreasonable ineffectiveness of philosophy"). Neither author can document that Feynman actually said this, though it certainly sounds like the sort of thing he might say.

[12] Feynman, along with Sin-Itiro Tomonaga and Julian Schwinger won the 1965 Nobel Prize for physics for their theoretical work with elementary particles.

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