

On Behalf of the Semantic View

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1.

There are a couple of issues raised by Sloep and van der Steen that apply equally to Lloyd, Thompson, and myself. I will get to those issues shortly. In the meantime, I will restrict my comments to those parts of Sloep and van der Steen's article that concern peculiarities (idiosyncracies?) of my own discussions of the nature of evolutionary theory.

Sloep and van der Steen concentrate on one particular criticism that I have raised against the received view of theories. They refer to that criticism as the "generality" problem. And the problem indeed has *something* to do with generality, but to construe it so simply is to misconstrue it. The real problem is lawlikeness. According to the received view of theories, a legitimate scientific theory consists of lawlike generalizations. Generalizations are plentiful in evolutionary biology. Laws are, well, name one.

Of course, these days there is little agreement as to what constitutes a law of nature. There may be no philosophically satisfactory notion of law. Or there may be a philosophically satisfying notion of law, but no laws at all in that sense (in which case it would not be a *terribly* satisfying notion of law; just as a notion of scientific theories, according to which there were none, would leave something to be desired). I have not been concerned to argue against any very specific notion of law, but rather to argue against the applicability of a broad range of notions of law to evolutionary theory.

Whatever else a law is, it is, on many accounts, more than just a contingently true generalization. For example, while it may be a law that $F = ma$, it is not (to use Hempel's famous example) a law of nature that the mass of every body of pure gold is less than 100,000 kg., even if the mass of every body of pure gold is in fact less than that weight.

Again, I do not deny that there are generalizations about evolution and evolutionary outcomes. There are, for instance, many generalizations

about what traits are optimal in what environments. There are also the sorts of generalizations that would presumably play a more central role in an evolutionary theory formulated in accordance with the received view of theories. For instance, we assume that a high proportion of loci of all populations segregate in a Mendelian fashion, from which we infer that a high proportion of loci in all populations behave in a Hardy-Weinberg fashion. But what is it, over and above their (presumed) truth, that renders such generalizations *laws*? I have argued that such generalizations are every bit as contingent as the generalization that “all ravens are black” (Beatty 1980, 1981; I realize that this is a bad example for those nonbiologically-minded philosophers who still invoke this generalization as a law of nature). The fact that so many ravens are black is a matter of historical/evolutionary happenstance, and so is, for instance, the fact that so many loci segregate in a Mendelian fashion. Just how general Mendelian segregation is — whether it holds for many loci or a few — is not the issue here. The issue has to do with whether a description of its degree of generality (whatever its degree of generality) is a law of nature or just a contingently true generalization. According to the received view of theories, an evolutionary theory based on a generalization about Mendelian segregation would be a legitimate scientific theory only if Mendel’s “law” were really lawlike — which seems to me a good reason for questioning the usefulness of the received view for representing evolutionary theory.

On the semantic view, theories are not statements about the world, much less generalizations, much less laws. Rather, theories are often complex definitional descriptions of different kinds of systems. As such, theories are distinguished from their applications, which are often complex statements to the effect that a theory applies to this or that system, or to this or that class of systems. So, for instance, we may distinguish between the definitional description of a “Mendelian locus” (one at which segregation is Mendelian), and applications of that definition to the effect that this or that locus is a Mendelian locus, or to the effect that this or that class of loci are Mendelian loci. Such applications in hand, we can proceed to explain evolutionary changes with respect to the locus or loci in question.

The semantic view itself — pure and simple — is silent on the issue of whether applications of legitimate theoretical system specifications should possess any particular degree of generality and whether legitimate theoretical system specifications should have lawlike applications. Sloep and van der Steen acknowledge this (at least the former point), but argue that this difference disappears in practice (that is, in the practice of presumably good science). For, if a semantic-view theorist discovers that his or her theoretical definition has no generalizable applications, then he or she should seek to amend it so that it does have generalizable applications. Perhaps (I am not sure) Sloep and van der Steen also believe that if a

semantic-view theorist discovers that his or her theoretical definition has no lawlike applications, then he or she should seek to amend it accordingly.

In the meantime — i.e., until the theory in question is reformulated so that it has lawlike or at least generalizable applications — is it not a “scientific” theory? Sloep and van der Steen may feel it is not, but they do not explain why. If they feel that the theory in question may be regarded as a scientific theory in the meantime, then they cannot hold that the issues of generality and lawlikeness arise with equal force in the cases of the received and semantic views. For, on the received view, a theory that is not general and lawlike is simply not a “scientific” theory.

Although Sloep and van der Steen do not mention it, there is an imaginable — indeed, often defended — case to be made for proscribing the formulation of theories that do not have lawlike applications. That is, according to some philosophical accounts of explanation, like the (in-) famous “covering-law” model, one cannot explain a phenomenon without invoking a law of which that phenomenon is an instance. If we want theories that we can use to explain phenomena, and if the covering-law model (or some appropriately restrictive alternative) is correct, then we should indeed be interested only in theories that have lawlike applications. This is a genuine concern of mine. But there is not much agreement these days as to what constitutes an explanation — the covering-law model, for instance, has plenty of problems. And, as I said before, there is also not much agreement as to what constitutes a law. So what are we to make of the proposed methodological stricture?

My own intuition in this regard is that if one wants to understand, for example, an evolutionary change at a particular locus, then what one wants to know is whether or not *that particular* locus segregates in a Mendelian fashion, not whether 90% or even all loci so segregate. The causal processes that locus undergoes — the causal processes that are supposedly the subject of a causal explanation of evolution at that locus — do not involve *all* other loci, or even 90% of all other loci. So why should a generalization about all, or 90% of all, loci be necessary for the explanation of an evolutionary change at one locus? What is the causal relevance to the story of all those other loci?

To put the point more abstractly, I do not see why causal explanations cannot proceed entirely on the basis of restricted claims about the particular systems whose behaviors are at issue. So while I can see the virtues of generally applicable theories, I do not see why theories *need* be generally applicable — much less why they should have lawlike applications — to be of use in understanding phenomena. Of course, it is a long way from these overly autobiographical remarks about my intuitions to a full-fledged theory of explanation on the basis of restricted claims. It may turn out that the restricted claims that play central roles in scientific

explanations have some additional feature (e.g., some special modal force) that makes them suitable for the job (I believe that Harmon Holcomb is working on this problem). I cannot answer that question yet. In the meantime, are there any other reasons (i.e., besides those concerning explanation) for denying the status of scientific theory to system specifications that lack generalizable or lawlike applications?

2.

The semantic view of theories may seem an extremely big step to take just in order to avoid the problem of the lawlessness of evolutionary theory. Why not simply construe theories as semantically interpreted generalizations, of varying degrees of generality, which may or may not be laws of nature? This seems to be the position of Sloep and van der Steen (although I cannot tell for sure whether they are really as ambivalent toward the importance of laws as their silence on the subject suggests). After all, they reason, evolutionary biology is full of more or less restricted generalizations — on that we can all agree. Yes, but.

It is important to note first that Sloep and van der Steen's proposal fares no better with regard to issues about scientific explanation than does my own. They too will have to show how explanations can proceed without laws of nature.

Second, and more importantly, why should Sloep and van der Steen acknowledge only that more or less restricted generalizations abound in biology, and yet not acknowledge that complex definitional descriptions — in state-space, other graphical, and natural-language modes of expression — *also* abound in evolutionary biology, and that the more or less restricted generalizations are often related to the definitional descriptions as more or less restricted application/instantiations? Of course, Sloep and van der Steen do acknowledge these additional aspects of evolutionary theorizing. But they feel free to disregard them in philosophizing about the nature of evolutionary theory. This gets us to what they call the problem of "faithfulness."

Sloep and van der Steen quote me as saying, "Given that optimality models cannot be construed as general, empirical laws, there is . . . reason to characterize them in terms of the semantic view." I have since extended the argument in question beyond optimality models, but that is not my point here. In the quotation, Sloep and van der Steen substituted an ellipsis for the word "further." The other main reason that I cited (and have continued to cite) for characterizing evolutionary theories in terms of the semantic view is the *faithfulness* of the latter to the former. Lloyd and Thompson have also argued this. Lloyd, Thompson, and I believe that the usefulness of the semantic view of theories is in large part a matter of its ability to faithfully represent evolutionary theorizing.

Sloep and van der Steen have a different view of the matter. They believe that such faithfulness detracts from the philosophical usefulness of the semantic view. (They do not challenge the faithfulness of the semantic view. They could — the semantic view is not 100% faithful, but it does characterize a lot of evolutionary theorizing. The issue here though is whether, *to the extent* that the semantic view is faithful to evolutionary theorizing, it is also useless in philosophical discussions of evolutionary biology.) They reason as follows:

As soon as the philosopher's and the scientist's formulation of some theory are virtually identical, we apparently do not need any separate philosophy. That is, no room will be left for the philosopher's clarifying role, including that of making (normative) suggestions about how scientific language and methodology might be improved.

If Sloep and van der Steen had said, "As soon as the philosopher has nothing more to say about theories than the scientist, we apparently do not need any separate philosophy," then I would have been more inclined to agree. What Lloyd, Thompson, and I have been concerned to do, however, is not just to quote evolutionary biologists, but to articulate what goes unarticulated in much evolutionary theorizing. We have tried to do that in terms of the semantic view of theories. If evolutionary biologists themselves regularly and clearly articulated the semantic view prior to presenting their own theories, then we would have found something else better to do with our time (perhaps we should anyway, but not for the reason that Sloep and van der Steen have in mind here).

It is only upon articulating a strategy of theorizing used by evolutionary biologists that we can proceed to discuss that strategy from a conceptual and methodological point of view. We think that the semantic view of theories is a better, more faithful way of representing what evolutionary biologists are doing in many cases than is the received view of theories (and better than Sloep and van der Steen's third alternative). An unfaithful representation of some branch of evolutionary theorizing is not going to be a very good starting point for the conceptual and methodological discussions of current evolutionary biology that Sloep and van der Steen — and Lloyd, Thompson, and myself as well — want to pursue.

It may turn out, upon closer scrutiny, that the semantic view of theories only confuses discussions of conceptual issues in evolutionary biology, or is at odds with some generally accepted aims of science. With respect to methodology, it is certainly not the case that the semantic view of theories, all by itself, will help scientists to achieve their most general aims. The semantic view of theories certainly needs to be implemented with some methodological rules. But it is wrongheaded to suggest that the faithfulness of the semantic view to evolutionary theorizing necessarily renders it useless in discussions of conceptual and methodological problems in current evolutionary biology.

3.

There is another issue raised by Sloop and van der Steen that applies equally to Lloyd, Thompson, and myself. It is motivated in Sloop and van der Steen's article by the following "caricature" (their term) of the enterprise of evolutionary biology according to the semantic view. An evolutionary biologist finds that a real population is correctly described by a specification of the notion of Hardy-Weinberg equilibrium and exclaims, "my empirical system is isomorphic to the ideal system, how wonderful, period," i.e., as if that was all there was to evolutionary biology. Sloop and van der Steen claim (as if anything needed to be said about such a silly caricature) that a scientist would only be happy with such an identification if he or she could also understand "why" the population was at Hardy-Weinberg equilibrium. The way I construe "why" here, that would indeed be one reason for being interested in the identification in question (see further). What Sloop and van der Steen mean by "why" here is, as they say, knowing the "conditions that the relevant population satisfies." I presume that by knowing the "conditions that the relevant population satisfies" they mean knowing the values of the coefficients associated with mutation, selection, migration, population size, and degree of random mating — i.e., all the conditions that, according to the notion of Hardy-Weinberg equilibrium, contribute to keeping the gene frequencies of a population stable. While a "scientist's" interest in the identification in question would be contingent upon knowing something about these conditions, a "philosopher," Sloop and van der Steen say, "might rest satisfied with the identification."

I find this remark (especially the reference to philosophers) rather cryptic. I do not see how one — whether one was a scientist or a philosopher — would be able to say that a population was in Hardy-Weinberg equilibrium without knowing the values of the coefficients in question, unless one means by "Hardy-Weinberg equilibrium" just "equilibrium." To me, "Hardy-Weinberg equilibrium" is a theoretically much richer notion than just "absence of gene-frequency change." Moreover, empirical instantiations of the notion of Hardy-Weinberg equilibrium are correspondingly more complicated, and say more about the population under investigation, than empirical instantiations of the notion of "no gene-frequency change."

Of course, the further question of why a population in H-W equilibrium is characterized by particular values of the coefficients of selection, mutation, etc., rather than other values of those coefficients, is not answered by simply instantiating the definitional description of "H-W equilibrium." But there is nothing in the semantic view of theories that suggests that theory instantiations do not raise further questions, or that theory instantiations do not have further uses (e.g., explanatory uses).

On the basis of their failure to differentiate between what is involved in the instantiation/application of more elaborate and less elaborate system

specifications, Sloep and van der Steen proceed to criticize the semantic view of theories for making overly “tidy” (their term) and “weak” (they use “uninformative” as a synonym) the connections between theories and phenomena. But the semantic view of theories itself says nothing about how complicated or informative theory applications are *in general*. Whether a theory application is complicated or uncomplicated, and informative or uninformative, depends on the nature of the theory being instantiated.

To take an example, instantiations of selection theories (like frequency-dependent theories, kin-selection theories, etc.) are more or less complicated and informative depending on how one defines “fitness” and “natural selection.” On the propensity interpretations of “fitness” and “natural selection” (Brandon 1978, Mills and Beatty 1979), instantiations of selection theories are more complicated and informative than on interpretations of “fitness” and “natural selection” based exclusively on actual descendant contribution.

To claim, as Sloep and van der Steen do, that empirical applications of semantically construed theories are “obviously simple and straightforward” is plain wrong.

Sloep and van der Steen clearly do not treat their “caricature” of evolutionary biology according to the semantic view entirely as a caricature. I find that especially unfortunate because the semantic view is not a view of science as a whole, but a view of theories. No one concerned with pursuing the semantic view presumes that the whole point of science is *just* to instantiate system specifications. One also wants to *explain* things. Having recognized an instantiation of a system specification, one may wonder why that particular system instantiates that specification (which may involve pursuing still other system specifications), or one may want to use the instantiation in question to explain something else. It is easy to criticize the semantic view of theories for being an incomplete view of science; but it is also beside the point to do so.

Response to Sloep and Van der Steen

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Sloep and Van der Steen raise the important issue of whether the state-space version of the semantic view can be useful in sorting out the various