issues which I believe my approach can help formulate and clarify. Sloep and van der Steen complain that "it is unclear what her semantic description of population genetics adds to their solution" (p. 11). That paper is clearly marked as an introduction to an entire research program, a taste of a different sort of philosophical approach to evolutionary biology; the issues mentioned as possible research topics there are discussed at length in my forthcoming book (1987).

A Defence of the Semantic Conception of Evolutionary Theory

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Peter Sloep and Wim van der Steen have argued (this issue) that, contrary to the claims of John Beatty (Beatty, 1980, 1981), Elisabeth Lloyd (Lloyd 1984) and me (Thompson 1983, 1985) the semantic conception (SV) of the structure of evolutionary theory falls short of providing the best philosophical analysis of biological theories. As part of their overall criticism, they argue that my arguments for the position that SV is more faithful to foundational work in evolutionary biology (Thompson 1983, 1986) and provides a richer account of the relation of theory to phenomena (Thompson 1985) are fallacious. They also argue for a 'third view' — distinct from SV and the received view of theory structure (RV) — which, they claim, provides a better philosophical analysis of biological theories than either SV or RV.

I shall argue that their cases against my positions rest on a mischaracterization of both RV and SV and, hence, fail. I shall also argue that their 'third view' is indistinguishable from RV correctly characterized. That is, the supposed difference between their third way and RV is a function of their mischaracterization of RV. To this end I shall first comment on their argument against my claim that SV is a more faithful account of foundational work in evolutionary biology. Then I shall comment on their argument against my claim that SV provides a richer understanding of the relationship between theory and phenomena. Next, I shall comment on their 'third position'. Finally, I shall discuss two other concerns that I have with claims made in their paper.

The basic flaw in their argument against my view that SV is more

Biology and Philosophy **2** (1987) 26—32.

faithful to foundational work in biology is that their argument rests on the incorrect view that on SV theories do not have empirical content.[1] The argument seems to be that I am guilty of equivocation concerning the idea that theories need not, in Lewontin's words, "say how the world is". They maintain that my argument about faithfulness succeeds only by interpreting this expression as meaning that theories have no empirical content. Interpreted in this way, they claim, SV does appear to be more in accord with at least those biologists like Lewontin who hold that theories need not "say how the world is". This is because, according to Sloep and van der Steen, theories, on SV, have no empirical content. However, they argue, Lewontin's view should be interpreted to mean that theories are extremely difficult to test. Hence, correctly understood, Lewontin's view of theories does not portray them as having no empirical content and, therefore, they are not better represented by SV.

I agree that the idea that theories need not "say how the world is" is not intended to indicate that theories do not have empirical content. Indeed, were I, as Sloep and van der Steen claim, to have understood the expression to mean that theories do not have empirical content, I would have been faced with the fact that there is a significant difference between SV and the way biologist's like Lewontin understand biological theories, not a similarity. And this is because, contrary to the characterization of Sloep and van der Steen, theories, on SV, do have empirical content.

Contrary to their characterization, theories, on SV, do not lack empirical meaning and, hence, content. What they lack is a specification of their domain of application.[2] On SV, a theory is an extralinguistic mathematical entity which consists in the specification — in mathematical English — of a physical system. Theories understood in this way can be regarded as models in the sense of a model *interpretation* of a formal system. And in the case of *scientific* theories the interpretation will be empirical. Hence, a scientific theory on SV can be regarded as an empirical interpretation of a formal system.[3] Quite clearly, then, scientific theories on SV have empirical meaning (content). That is, they have a semantics (a meaning structure) which is why the view is called the semantic conception.[4]

Indeed, it is because, on SV, theories have empirical meaning that one knows very well what the world would be like if the theory were a correct description of it. What we do not know from the theory is whether it is a correct description of a class of phenomena. Of course, when constructing theories one has in mind potential applications and, hence, one quite naturally asserts these potential domains as the domain of applications to be tested. And, because the theory has empirical content, one knows from the theory what the class of phenomena to which it is supposed to apply will be like if the assertion that that class is part of the theory's domain of application is correct.

Consider, as an illustration, empirical models of Euclidean and non-

Euclidean geometries. *As competing models of the structure of space*, they have empirical content since making the formal systems of Euclidean and non-Euclidean geometries competing models of the structure of space involves providing the formal systems with an empirical interpretation. These models (interpreted formal systems) can now be said to apply to the spatial structure of the world. Testing which one does in fact accurately describe the structure of space is exceedingly complex involving a rich array of methodological considerations and imported theories.

If the spacial structure of the world is shown to be isomorphic to an empirical model of non-Euclidean geometry but not isomorphic to an empirical model of Euclidean geometry, it does not follow that Euclidean geometry is false (whatever that might mean). What it does show is that the empirical model(s) of Euclidean geometry does not accurately describe the spacial structure of the world. That is, this world is not part of the domain of its application. The formal system remains, however, an intelligible system of geometry and its empirical model(s) remains an intelligible interpreted formal system on the basis of which we can know what the world would be like if it were Euclidean. What fails to be true is the claim that the world is Euclidean. That is, that space has the same structure as (is isomorphic to) the *empirical* model of Euclidean geometry.

Consequently, I do not, as they claim, equivocate or amalgamate testability and empirical content. I have no interest in interpreting Lewontin's view as claiming that theories have no empirical content — quite the opposite. My argument is that, among other things, SV represents better than RV the complexity and, hence, difficulty, involved in asserting and assessing the fit between theory and phenomena. On SV, theories have empirical meaning but, unlike RV, the specification of the domain of their application is not part of the theory. Consequently, testing theories involves a rich array of methodological considerations and imported theories. This is what makes competing theories of population genetics (and most other theories) difficult to test. Hence, the force of their argument against my view rests on the mistaken characterization that theories, on SV, have no empirical content and on what, based on that mischaracterization, they mistakenly believe to be my interpretation of the claim of Lewontin. (See Note 2 on the meaning of 'empirical content'.)

Similarly, their argument against my position that SV provides a richer account of the relationship of a theory to phenomena rests on a mischaracterization — this time of RV. They concede that my arguments succeed in showing that one feature of the complex and rich relationship between a theory and phenomena on SV is that, "SV will yield a description of sociobiological theory that takes the sting out of the testability charge". They maintain, however, that SV is not necessary in order to achieve this result. Why not? Because, they argue:

> The current situation could as well be described as follows. We have a grand theory

> that seems to account for much animal behavior. We are not so sure about humans. Humans are different although they share much "biology" with fellow creatures. What about the domain of our grand theory? We could decide to restrict it to the province of animal biology. Then we have a theory with a limited scope that has the merit of being well-confirmed. Alternatively, we can make the theory more general by enlarging its intended scope. Then we should not regard it as well confirmed throughout its domain. (p. 9)

This ability to restrict or expand the domain of application is, however, exactly what SV permits and RV does not permit. On RV, the theory includes a specification of its domain of application which cannot, as Sloep and van der Steen imply, be determined independently of the theory. That is, it is not possible to have a grand theory and then specify its domain of application. The grand theory will include a specification of its domain of application. The reality is that, on RV, the formal system of the theory is interpreted (given empirical meaning) by correspondence rules which related the theory to phenomena. A theory, on syntactic views like RV, is a deductive structure within which laws governing the behaviour of phenomena are deduced from axioms and from which, by employment of these laws, phenomena are deduced. Laws and axioms on this view are statements which describe the behaviour of phenomena. Stated simply, theories are deductively related to phenomena with the content of that deductive relationship specified by correspondence rules which are an *intrinsic* part of the theory (see, Suppe, 1977 esp. pp. 102–109).

On the other hand, on SV, which is not a statement view of theories, laws specify the behaviour of a system, not the behaviour of phenomena. The relationship of the system to the world is not one in which phenomena are deduced, by employment of laws of the theory, but one of isomorphism between two systems: one specified by the theory (a physical system), the other the phenomenal system to which the theory is claimed to apply. The domain of application is not specified by the theory and, hence, can be restricted or expanded as seems appropriate. Consequently, SV allows the move that Sloep and van der Steen wish to make while RV and other syntactic accounts do not allow this move.[5] Hence, it seems that SV is necessary for their solution just as much as for mine.

Sloep and van der Steen will no doubt counter this suggestion by pointing out that they reject both RV and SV in favour of a 'third view' and that their 'third view' allows their move thus demonstrating that SV is not necessary to solve the testability problem with sociobiological theory. This solution, however, fails. First, their 'third view', in so far as one can tell from their sketchy account of it, is, like RV, a statement view and, hence, is not immune to the argument I have just given. Second, the illusion that their 'third view' is different from RV rests on the same mischaracterization of RV discussed above. They write:

> The analysis given in the present paper should reveal our own preference for a third

> option, which does not aim at the removal of untidiness. Why not use semantically interpreted empirical *statements* in our theories right from the start? (p. 14, emphasis added).

Leaving aside what 'interpreted empirical statements' might mean (I would have thought that if the statements were empirical they were already interpreted), I have difficulty distinguishing this view from RV. On RV, theories are interpreted formal systems. Hence, on RV, theories are already what Sloep and van der Steen suggest they should be: they are deductively organized *statements* which are *interpreted* by correspondence rules. They are, in short, deductively organized empirical statements, the empirical interpretation of which (i.e., the semantics of which) is given by correspondence rules *which are part of the theory.* Theories, on RV, are already a composite of syntax (a formal system) and semantics (interpretation by correspondence rules) (see, Suppe, 1977). And, on RV scientific theories must include correspondence rules since without them all that remains is a formal system without meaning — empirical or otherwise. Hence, on syntactical statement views of theory structure like RV, theories consist of precisely what Sloep and van der Steen suggest they should (i.e., semantically interpreted 'empirical' statements). It is this feature of RV that Sloep and van der Steen seem to not to have grasped and that makes their 'third view' indistinguishable from RV. Both, it seems, are statement views which take theories to be a composite of syntax and semantics. Both, it seems, involve semantically interpreted 'empirical' statements. Hence, without further explication of the nature of the semantics of their 'third view' or the relation of a theory to phenomena it is difficult to see how their view differs from RV. And, hence, how it offers an option to RV that makes it unnecessary to adopt SV as way of taking "the string out of the testability charge".

My general conclusion regarding the arguments of Sloep and van der Steen against my arguments in support of SV is that they rest on fundamental mischaracterizations of both RV and SV. I also suggest that their 'third view', in its exceedingly sketchy present form, is indistinguishable from RV.

Having now responded to the arguments they offer against my claims about SV, I shall conclude by briefly discussing two of a number of problems that I have with other claims made in their paper.

Sloep and van der Steen claim (p. 15)

> 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.

This they see as a problem generated by Elizabeth Lloyd's view (Lloyd

1984) "that SV is able to provide a "natural reconstruction" of population genetics". However, SV is a view about the *logical* structure of scientific theories which leaves open most questions concerning methodology and clarity. Indeed, it leaves open a host of methodological questions about the relationship between theories and phenomena as well as numerous questions about the role of other imported theories in applying theory to phenomena (see Suppes, 1962; Thompson 1985). There will still be lots of work here for philosophers and biologists even if SV correctly describes the logic of scientific theories.

On p. 28, they claim:

> Statements to the effect that some empirical system [more accurately and less ambiguously 'phenomenal system'] exemplifies an ideal system are *at best* a good starting point for inquiries about processes in the real world. We should also want to know *why* the mappings we establish do obtain. Unfortunately, however, attempts to answer this question will reintroduce all the messy items that were initially suppressed in our endeavours.

Two questions arise in connection with this claim. First, why do Sloep and van der Steen believe that supporters of SV are looking for tidiness? Second, how much do they think a view about the logical structure of theory is supposed to cover? Of course a view about the structure of theories is not going to answer every (or even very many) conceptual and methodological questions. One virtue of SV is that it makes clear that all the questions about experimental design, goodness of fit, analysis and standardization of data, etc. are not part of a theory and are open issues requiring experimental skill, insight, other theories, etc. SV is not an attempt to promote tidiness or to answer all methodological and conceptual concerns. It is a view about the correct way to formalize a scientific theory.

NOTES

[1] The clearest statement of this mischaracterization is found on p. 4 where they claim, "A theory thus characterized semantically has no empirical content because it only defines a set of ideal systems (models). Empirical import is provided by *theoretical hypotheses* . . .". This is false. What is the case is that theories do not specify any *applications* to phenomena. Hence, theoretical hypothesis are needed to make assertion about such applications that can be tested by exploring whether there is an isomorphism between the theory and phenomena. Theories, on SV, do have empirical content (meaning).

[2] Perhaps, although it seems unlikely, their claim that theories, on SV, have no empirical content is not intended to mean that they have no empirical meaning. Perhaps they mean by the expression 'no empirical content' that it does not contain a specification of its domain of application to phenomena. This latter view is correct but if this is what they mean by 'no empirical content', then the problem with SV, as they see it, becomes the difficulty of specifying and, hence, testing the relationship between theory and the phe-

nomena. This, however, is precisely the problem that they claim Lewontin, correctly understood, is asserting about biological theories. Hence, far from equivocating, and amalgamating testability and empirical content, I have, on their own terms, correctly interpreted Lewontin and have shown that SV has precisely those features of biological theorizing that Lewontin outlines.

[3] For a more detailed account of the semantic view see Thompson 1983 and the references therein to expositions of the semantic view by Bas van Fraassen, Frederick Suppe and Patrick Suppes.

[4] They also seem to have misunderstood the sense in which theories on SV are 'ideal systems'. Theories, on SV, are ideal systems and abstractions only in the sense that they abstract from the complexity of phenomena a system that is simpler and will describe phenomena under ideal conditions — controlling for these conditions in actual experiments is part of the task involved in designing an experiment. This does not entail that they have no empirical content.

[5] One major difference between SV and RV is that while for both the interpretation is part of the theory, the interpretation on RV, unlike SV, makes the phenomenal world, or part of it, the model of the formal system. Hence, the domain of application and the way the theory relates to the world is part of the theory itself. Correspondence rules interpret the formal system of the theory in a way that results in an identity between the world and the model interpretation of the formal system.