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# THE INTERACTION OF THEORIES AND THE SEMANTIC CONCEPTION OF EVOLUTIONARY THEORY

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During the past decade John Beatty, Elizabeth Lloyd and I[1] have characterized and argued for the acceptance of an alternative to the received view — so named by Hilary Putnam[2] — of theory structure in evolutionary biology. The alternative is an application to evolutionary biology of the semantic conception of theories put forward in various ways, and in non-biological contexts, by Joseph Sneed[3], Wolfgang Stegmuller[4], Frederick Suppe[5], Patrick Suppes[6] and Bas van Fraassen[7]. In this paper, I shall contrast this view with the received view of theory structure and argue that the semantic conception provides a richer, more appropriate and more accurate picture of the structure of evolutionary theory.

For the sake of clarity I want to emphasize at the outset that the criticisms of the received view and the arguments for an alternative in biology found in this paper provide no grounds for the view that biology is a different kind of science than physics or for the view that formalization is impossible or inappropriate with regard to evolutionary theory. There may be reasons for holding such views but the contents of this paper cannot be counted among them. Suppe, van Fraassen and others have all argued that the semantic conception is a more useful and accurate picture of theorizing, explaining and experimenting in physics (especially quantum theory). Hence, arguing that it is also more appropriate in biology demonstrates a similarity, not difference, between physics and biology. Also, the semantic conception is an account of the formalization of theories. Indeed, Patrick Suppes considers his 'set-theoretical predicate' conception of theory structure (a view which is a version of the semantic conception of theories) to be an axiomatization — an axiomatization within set theory as opposed to an axiomatization directly in first order logic with identity[8].

In this paper I shall sketch the received view of theory structure — the currently dominant view in biology. I shall then examine two prominent attempts to provide an account of the structure of evolutionary theory on this view and argue that they fail to adequately formalize evolutionary theory. Then I shall provide an account of the semantic conception of theory structure and argue that it provides a richer and more appropriate account of the structure of evolutionary theory.

## I

On the received view of theory structure, a theory is a linguistic axiomatic and deductive structure the language of which is first order predicate logic with identity. Correspondence rules partially define the theoretical vocabulary of the calculus in terms of the observation vocabulary of the theory. Originally correspondence rules were understood to provide explicit definitions of theoretical terms. In response to numerous difficulties with this understanding, they were subsequently understood to provide partial definitions of the theoretical terms. In the later and most sophisticated formulations of Hempel[9] and Carnap[10], partial interpretation is provided by corresondence rules in the form of bilateral reduction sentences. Hence, correspondence rules have the general form :

$$Cx \rightarrow (Qx \longleftrightarrow Ex)$$

where C is a test condition, Q is a theoretical concept, and E is an observable outcome under the test condition[11]. Hence, a theoretical concept like 'fragile' will be partially defined :

$$(x)\,(t)\,(Sxt \rightarrow (Fx \longleftrightarrow Bxt).$$

That is, for any x and for any t if x is struck sharply at t then x is fragile if and only if x breaks at t[12].

Correspondence rules also specify the ways in which the theory is applied to phenomena. This feature of the received view has been criticized on numerous grounds[13]. A criticism important to the thesis of this paper, and back to which I shall be referring later, is due to Kenneth Schaffner[14]. He has convincingly argued that specifying the ways in which a theory relates to phenomena in terms of correspondence rules ignores the ways in which laws from other independent theories are employed in 'causal sequences' which causally relate theories to phenomena. These causal sequences describe the causal mechanisms underlying the measurement procedures which are specified by the correspondence rules of a theory. That is, they explain why the measuring device behaves the way it does and, hence, why it is acceptable to use a particular measuring device to obtain observations relevant to the theory. Since the correspondence rules of the received view account provide no role for laws of other theories in the relating of a particular theory to phenomena, two distinct theories cannot be interactively employed. Even a formalization that unified two particular theories would be of limited value since other theories will be required in order to describe the causal mechanisms underlying the measurement procedures specified by the correspondence rules of the new unified theory.

On the received view, laws describe the behaviour of phenomena and phenomena are explained and predicted by deducing, or at least inducing, the phenomena from a concatenation of laws and phenomena occurring prior to the phenomena being explained or predicted. Hence, the behaviour of phenomena is, in principle, deducible from the laws (statements) of the theory. Laws of the theory are in principle deducible from more general laws and ultimately from the axioms of the theory. In practice numerous subsidiary assumptions need to be added to the laws in order to allow anything approaching an inference of either laws from laws or behaviour of phenomena from laws.

## II

In what follows I shall argue that evolutionary theory does not fit this conception of theory structure easily, despite its wide acceptance as the ideal toward which formalizations of the structure of evolutionary theory should aim. The two most notable attempts to provide a received view account of evolutionary theory are Michael Ruse's sketch of the axiomatic structure of population genetics which he argues is the core of evolutionary theory[15] and Mary Williams' axiomatization of the theory of natural selection[16]. I shall examine both of these views and argue that they are inadequate accounts of evolutionary theory. The ways in which these accounts are inadequate indicates an important structural feature of an adequate account, namely, the interaction of the theories of heredity and natural selection.

Michael Ruse argues that population genetics is the core of evolutionary theory and, hence, axiomatization of population genetics is an axiomatization of the heart of evolutionary theory. Alexander Rosenberg has argued against Ruse's claim that population genetics is the core of evolutionary theory[17] — a controversy to which I shall return later in this section. For now let this assumption stand. What Ruse attempts to provide is a sketch of an axiomatization of population genetics. The sketch consists of a demonstration that the Hardy-Weinberg law can be deduced from Mendel's first law (the law of segregation) which, along with Mendel's second law (the law of independent assortment), is, according to Ruse, an axiom of Mendelian (population) genetics. On the strength of this demonstration, Ruse concludes, "we can now clearly see that at least parts of Mendelian genetics are axiomatized[18]". This conclusion in conjunction with his conclusion that population genetics is the core of evolutionary theory entails that at least parts of evolutionary theory are axiomatized : "Hence, through the incorporation of population genetics into evolutionary theory there has been an extension of the axiomatic nature of evolutionary theory."[19]

This, to say the least, is a very sketchy and modest attempt at axiomatization. To be fair, however, Ruse's use of it is also modest. He uses it to demonstrate that axiomatization is possible and, at least in part, achievable. It is not intended to be an exercise in axiomatization. Unfortunately, however, it fails to achieve even this modest goal. As John Beatty has argued[20], Mendel's laws cannot be axioms of even a part of evolutionary theory because Mendelian inheritance is itself a product of evolution. And, laws that describe processes that are themselves a result of evolution can hardly serve as the axioms of a theory of evolution.

By contrast with Ruse, Mary Williams does undertake to provide a thoroughgoing and rigorous axiomatization. Her axiomatization consist of[21] :

1. two primitive terms ('biological entity' and 'is a parent of');
2. definitions of other terms (e.g., 'ancestor', 'clan', 'subclan', etc.) in terms of these primitives and set-theoretical principles;
3. two axioms stating general propositions that are true of all evolutionary theories (1. No biological entity is a parent of itself and 2. If a is an ancestor of b, then b is not an ancestor of a);
4. an operational definition of fitness; and
5. five axioms of Darwinian evolutionary theory as follows :

1. Every Darwinian subclan is a subclan
2. There is an upper limit to the number of biological entities in any generation of a Darwinian clan
3. For each biological entity, there is a positive and real number that describes its fitness in a particular environment
4. If (a) any Darwinian subclan, D, has a subclan $D_1$, and (b) $D_1$ is superior in fitness to the rest of D for sufficiently many generations, then the proportion of $D_1$ in D will increase
5. In every generation of a Darwinian subclan D (that is not on the verge of extinction), there is a subclan, $D_1$ : and $D_1$ is superior in fitness to the rest of D for long enough to ensure that $D_1$ will increase relative to D; and as long as $D_1$ is not fixed in D, it retains sufficient superiority to ensure further increases relative to D.

Elliot Sober and Michael Ruse have both criticized Williams' axiomatization. Sober[22] points out that that the axiomatization : contains no source laws; does not mention mutation, migration, systems of mating and numerous other causes of evolution which evolutionary biology takes into account; excludes genetics; and is Malthusian — i.e., it incorrectly makes selection dependent on reproductive rates. Ruse[23] also criticizes Williams' axiomatization for making no reference to genetics. This is the important criticism relative to the thesis of this paper.

Michael Ruse has argued that Williams' axiomatization is inadequate as an axiomatization of evolutionary theory because it contains no hereditary mechanism. Ruce correctly maintains that any adequate characterization of evolutionary theory must take account of three central features : variation, natural selection (i.e., non-random differential reproduction which is a function on the variation that maps the entire population into the subset of reproductively successful member of the entire population) and heredity (i.e. the transmission to offspring of parental characteristics). Natural selection can occur in the absence of heritability of characteristics, but evolution cannot since, without heritability, natural selection will have no causal effect on subsequent generations.

Hence, the absence of a hereditary mechanism renders Williams' axiomatization inadequate as an axiomatization of evolutionary theory regardless of one's views about its status as an axiomatization of the theory of natural selection. And, Ruse thinks that there is no satisfactory way to incorporate an account of heredity into her axiomatization. Ruse, on the other hand, considers his own axiomatization-sketch, to be conceptually adequate because it incorporates all three of the above features by identifying population genetics as the core of evolutionary theory. In the first place, population genetics quite obviously provides a theory of heritability. And, secondly, it provides an account of variation and natural selection in terms of allelic frequencies. Variation within a population is a result of a high frequency of polymorphic loci (loci with two or more alternate alleles). This high level is maintained by mutation, recombination, immigration, balancing selection, etc. Natural selection is a function which maps one set of allelic frequencies into a temporally later set of allelic frequencies and can be expressed in the theory as coefficients of selection. On this view, evolution is a non-random cumulative change in allelic frequencies.

Recently Alexander Rosenberg has defended Williams' account and criticized Ruse's account[24]. His criticism of Ruse is different from that of Beatty discussed above, and, relative to the purposes of this paper, provides an important perspective on the differences between the accounts of Williams and Ruse.

Rosenberg considers it a strength of Williams' account that it is neutral on the question of hereditary mechanisms and a defect in Ruse's account that it is so wedded to a particular theory of heredity. He argues that evolutionary theory is not dependent on any particular theory of heredity and that far from being the core of evolutionary theory, a theory of heredity is simply an assumption of the theory. The central mechanism of evolution is natural selection. Heredity is assumed as a background condition, albeit an important one. Hence, the theory of evolution is neutral on hereditary mechanisms. Since Ruse's account is not only not neutral with regard to the mechanism of heredity — it is entirely dependent on current population genetical theory — but also characterizes evolutionary theory entirely in terms of heredity, it misrepresents the character of the theory. Williams' account, on the other hand, has precisely the neutrality concerning hereditary mechanisms that is required as well as the correct emphasis on the centrality of natural selection.

Rosenberg also argues that population genetics cannot be the core of evolutionary theory because laws like Mendel's laws and the Hardy-Weinberg law do not describe situations of change but situations of equilibrium. In addition to the laws of Mendelian genetics, evolution requires laws that disrupt the equilibrium described by Mendelian genetics.

Rosenberg's argument against Ruse's claim that Mendelian genetics is the core of evolutionary theory is compelling. No particular theory of heredity is required by evolutionary theory. All that is required is that characteristics of parents be transmittable to offspring. It is also clear that natural selection is a prime mechanism without which population genetics would predict a situation of almost complete stasis — the only changes being due to random drift and recombination. Further it is clear that natural selection usually acts on phenotypes and not directly on genotypes. It is, therefore, not accurately represented by selection coefficients in a genetic calculus.

Despite all of this, however, his detaching of heredity from evolutionary theory is untenable for the same reasons given by Ruse and Sober for rejecting the adequacy of Williams' axiomatization of current evolutionary theory. That population genetics is not by itself sufficient to entail evolutionary change does not entail that it is not a central part of evolutionary theory. Natural selection by itself also does not entail evolutionary change since the effects of selection, without heredity, are limited to each generation and cannot be perpetuated or accumulated. What these arguments for individual insufficiency show is that both are essential. An adequate evolutionary theory, as argued above, is a composite of both heredity and natural selection. And, despite Rosenberg's praise of neutrality on the question of hereditary mechanisms, this composite nature is not undermined by the unlikely possibility that our current account of the specific mechanisms is wrong.

To claim that evolutionary theory presupposes some hereditary mechanism but not any specific mechanism does not entail that heredity is not an essential component in any theory about evolution. Consideration of the similar situation of natural selection makes this clear. That is, it is also true that evolutionary theory presupposes some mechanisms of selection but not any specific mechanisms — i.e., though unlikely, balancing selection, diversifying selection, etc. may be incorrect specific mechanisms of selection even though some mechanisms of selection are among the principal causes of evolution. This is not — as it should not be — taken by Rosenberg, however, to entail that natural selection is not an essential component of the theory. Both natural selection and heredity are essential components of the theory even though no specific account of the mechanism of natural selection or heredity may be entirely correct and, hence, essential to the theory. What seems clear is that some — perhaps yet unformulated — account of both must be possible. Were it is the case that, in principle, no account could be given of one or the other of heredity and selection, current evolutionary theory would, in principle, be incapable of formulation. And, this demonstrates the degree to which both components are essential to the theory.[25]

What emerges from this discussion is that Ruse's account identifies evolutionary theory with a theory of heredity while Williams' account identifies evolutionary theory with a theory of natural selection and that both of these identifications misrepresent evolutionary theory. It is a composite of both. The two accounts also demonstrate the difficulty of providing a single axiomatization of this composite structure. The main difficulty is that evolutionary theory, as Morton Beckner insightfully argued several decades ago[26], appears to be a family of related models rather than a unified structure[27]. Consequently, a received view axiomatization insofar as it is possible and desirable, can only be provided for the component models and not for the theory as a whole. And, this component axiomatization is precisely what Williams and Ruse have provided.

What is required is an approach to theory structure that allows the theory of natural selection and the theory of heredity — at present population genetics — to be separately formalized but capable of interaction and simultaneous employment under those formalizations. This, however, is not possible on a received view account of theory structure. As noted above, Schaffner has convincingly argued that the way, on the received view, that correspondence rules relate the theory to phenomena excludes employment of subsidiary theories in relating the theory to phenomena. In addition, it is not possible for the laws of two independently formalized theories to interact due to the holistic nature of the interpretation of each theory.

Consequently, it is not possible on a received view account of theory structure for two independent theories to interact. Hence, evolutionary theory cannot be adequately formalized within the received view. However, as I shall argue now, it can on the semantic conception.

## III

On the semantic conception of theory structure[28], a theory is an extralinguistic mathematical entity which consists in the specification — in mathematical English — of a physical system[29]. A theory is related to phenomena by asserting that the possible states and behaviours of the phenomenal systems within its intended scope are isomorphic to the behaviour of the physical system specified by the theory. On the state space approach of van Fraassen and Suppe[30] — the approach I shall adopt for the remainder of this paper — a theory specifies a physical system by specifying a state space (an n-dimensional Cartesian space), laws of coexistence which define the set of possible states in the state space, laws of succession which define the physically possible state transitions in the state space, and laws of interaction which define the physically possible interactions of the specified system with other systems. It is the laws of interaction that are important to the subject of this paper.

On a semantic conception, two theories can interact on at least two levels. First, there can be inputs to a physical system which result from its interaction with another system such that the state of the system is altered. The laws of interaction will specify, by means of a 'next state function, the possible outcomes which result from these inputs. That is, a 'next state' function maps S X I into S where S is a non-empty set of states and I is a non-empty set of inputs. In this way one system will directly affect the behaviour of another system.

A second level of interaction can, and almost always does, occur when applying a theory to phenomena. Whenever apparatus is use to make scientific observations, as is almost always the case, there will be an interaction between the theory being applied and a theory or theories which describe the behaviour of the apparatus. Suppe[31] considers it the job of a theory of experimental design (a part of the theory of the experiment on his taxonomy) to specify the ways in which these theories interact — or, as he expounds it, the ways in which the phenomenal systems to which the two theories apply interact. The laws of interaction in this case will not be part of the theory being applied but part of the theory of the experiment used to apply the theory to a phenomenal system. Without a theory of the experiment containing laws of interaction, it would not be possible to causally explain why the apparatus (usually a phenomenal system within the intended scope of a theory other than the one being applied) is an observation within the phenomenal system to which the theory is being applied — recall Schaffner's criticism of the received view, outlined above, on exactly this point[32].

The interaction of the theory of natural selection and the theory of heredity, as required by the view that evolutionary theory is best characterized as a family of interacting theories, is of the first level. That is, the theories directly interact with each other. On the one hand, the theory of heredity directly interacts with the theory of natural selection. The laws of interaction in this case will specify, by means of a 'next state' function, the phenotypic next state of the population between selections and across generations. That is, a population will undergo selection in accordance with the laws of succession of the theory of natural selection (S is mapped into S) and then the laws of interaction of the theory will specify the next state of the system (the next generation) by specifying the ways the system behaves under inputs determined by the theory of heredity (S X I is mapped into S).

Conversely, the theory of heredity directly interacts with the theory of natural selection. The laws of interaction in this case will specify, by means of a 'next state' function, the genotypic next state of the population after selection. That is, the genotype of the population will undergo generational transition in accordance with the laws of succession of the theory of heredity and then the laws of interaction of the theory will specify the next state of the system (the state after selection) by specifying the ways the system behaves under inputs which are determined by the theory of natural selection.

On this view, each theory is dependent on the other. Without interaction neither theory is an adequate description of evolutionary change. And, this is precisely the characterization of evolutionary theory that was argued for in section II above — a characterization tht could not be accommodated by the received view account. Consequently, the semantic conception of theories provides a framework within which a formalization of evolutionary theory understood as a family of interacting theories can be given, whereas the received view account does not. The semantic conception is, consequently, a richer account of theory structure and the more appropriate and promising account within which to formalize evolutionary theory.

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## NOTES

[1] See : J. Beatty, "Optimal Design Models and the Strategy of Model Building in Evolutionary Biology", *Philosophy of Science 47*, 1980, 532—561; J. Beatty, "What's Wrong With the Received View of Evolutionary Theory ?" in : P.D. Asquith and R.N. Giere (eds.), *PSA 1980*, volume 2, Philosophy of Science Association, East Lansing, 1981, 397—426; E. Lloyd, "A Semantic Approach to the Structure of Population Genetics", *Philosophy of Science 51*, 1984, 242—264; P. Thompson, "The Structure of Evolutionary Theory: A Semantic Approach", *Studies in History and Philosophy of Science* 14, 1983, 215—229; P. Thompson, "Sociobiological Explanation and the Testability of Sociobiological Theory", in James Fetzer (ed.), *Sociobiology and Epistemology*, Dordrecht, Reidel, 1985, 201—215.

[2] H. Putnam, "What Theories Are Not." in: E. Nagel, P. Suppes and A. Tarski (eds.), *Logic, Methodology and Philosophy of Science*, Stanford, Stanford University Press, 1962, 240—251.

[3] J. Sneed, *The Logical Structure of Mathematical Physics*, D. Reidel, Dordrecht, 1971.

[4] W. Stegmüller, *The Structure and Dynamics of Theories*, Springer, New York, 1976; and W. Stegmüller, *The Structuralist View of Theories*, Springer, Berlin, 1976.

[5] F. Suppe, *The Meaning and Use of Models in Mathematics and the Exact Sciences : A Study in the Structure of Exact Scientific Theories*, University Microfilms International, Ann Arbor, 1967 (Ph.D. Dissertation); F. Suppe, 'What's Wrong with the Received View on the Structure of Scientific Theories ?" *Philosophy of Science 39*, 1982, 1—19; F. Suppe, *The Structure of Scientific Theories*, 2nd ed., University of Illinois Press, Chicago, 1977; F. Suppe, 'Theoretical Laws', in M. Przelecki, K. Szaniawski, and R. Wojcicki (eds.), *Formal Methods in the Methodology of Empirical Science*, Ossolineum, Wroclaw, 1976, 247—267.

[6] P. Suppes, *Introduction to Logic*, Van Nostrand, Princeton, 1957; and P. Suppes, "What is a Scientific Theory ?" in S. Morgenbesser (ed.), *Philosophy of Science Today*, Basic Books, New York, 1967, 55—67.

[7] B. C. van Fraassen, "On the Extension of Beth's Semantics of Physical Theories", *Philosophy of Science, 37*, 1970, 325—339; B.C. van Fraassen, "A Formal Approach to the Philosophy of

Science", in R.E. Colodny (ed.), *Paradigms and Paradoxes*, University of Pittsburgh Press, Pittsburg, 1974, 303—366; and B.C. van Fraassen, *The Scientific Image*, Oxford University Press, New York, 1980.

[8] See, *An Introduction to Logic*, 246—249.

[9] See, Hempel, C., "Empiricist Criteria of Cognitive Significance: Problems and Changes," and "The Theoretician's Dilemma: A Study in the Logic of Theory Construction," both in: Hempel, C.G. *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*, New York, The Free Press, 1965, 101—122 and 173—226.

[10] See, Carnap, R., "Testability and Meaning," *Philosophy of Science 3*, 1936, 420—468; 4, 1937, 1—40.

[11] See "The Theoretician Dilemma", 188—189.

[12] See "Empiricist Criteria of Cognitive Significance", 109—110.

[13] See, Suppe, F., *The Structure of Scientific Theories*, 2nd edition Urbana, University of Illinois Press, 1977, for an exposition of these criticisms.

[14] Schaffner, K.F., "Correspondence Rules", *Philosophy of Science 36*, 1969, 280—290.

[15] see Ruse, M., *The Philosophy of Biology*, London, Hutchinson, 1973, 32—68; and Ruse, M., "Is Biology Different From Physics ?" in R. Colodny (ed.), *Logic, Laws, and Life*, Pittsburgh University of Pittsburgh Press, 1977.

[16] Williams, M., "Deducing the Consequences of Evolution", *Journal of Theoretical Biology*, 29, 1970, 343—385.

[17] Rosenberg, A., "The Interaction of Evolutionary and Genetic Theory," in L.W. Sumner, J.G. Slater and F.F. Wilson (eds.), *Pragmatism and Purpose : Essays Presented to Thomas Goudge*, Toronto, University of Toronto Press, *1981*, and Rosenberg, A., *The Structure of Biological Science*, New York, Cambridge University Press, 1985, 136—152. See also his comments in Rosenberg, A., "Ruse's Treatment of the Evidence for Evolution : A reconsideration", in: P.D. Asquith and R.N. Giere (eds.) *PSA 1980*, volume 1, East Lansing, The Philosophy of Science Association, 1980.

[18] *The Philosophy of Biology*, 35.

[19] *Ibid*, 65.

[20] "What's Wrong with the Received View of Evolutionary Theory ?"

[21] Rosenberg provides a clear, non-technical presentation and discussion of Williams' axiomatization in *The Structure of Biological Science*, 136—152.

[22] Sober, E., "Fact, Fiction and Fitness : A Reply to Rosenberg", *Journal of Philosophy 81*, 1984, 372—383; and Sober, E., *The Nature of Selection : Evolutionary Theory in Philosophical Focus*, Cambridge, The MIT Press, 1984, 188–196.

[23] 'Is Biology Different From Physics ?", 111–113. He also makes the claim, without argument, in *The Philosophy of Biology*, 50.

[24] *The Structure of Biological Science*, 130—148.

[25] Rosenberg has advanced another argument which, though undermining the claim that population genetics is the core of evolutionary theory, is entirely uncompelling as an argument against considering heredity as an essential component in evolutionary theory. He has argued that heredity is a presupposition of any evolutionary theory and not just of Darwinian evolutionary theory. Without debating the truth of this claim, I fail to see how, even if true, it entails that it is not an essential part of current Darwinian evolutionary theory. It is like claiming that since oxygen is an ingredient in all cases of combustion and not just those involving burning candles it is not an essential part of the burning of a candle. I see no contradiction in claiming that something is presupposed by all theories of a given type and is an essential ingredient in any specific theory of that type.

[26] Beckner, M., *The Biological Way of Thought*, Los Angeles : University of California Press, 1968, 160 (originally published, New York, Columbia University Press, 1959).

[27] Richard Lewontin *(The Genetic Basis of Evolutionary Change*, New York,Columbia University Press, 1974, 14.) has provided a schematic representation of the relationship between heredity and selection within evolutionary theory which makes clear the different kinds of transformation rules involved and the distinct though related spheres of selection and heredity. On Lewontin's view, Ruse's account is inadequate because is conflates the variety of laws of transformation to laws of genetics alone. While Williams' account is inadequate because it includes only phenotype transformations.

[28] What follows is a brief summary account of the semantic concep-

tion of theories on a state space approach. For a more detailed discussion see my "The Structure of Evolutionary Theory : A Semantic Approach" or Suppe's "What Wrong with the Received View of Scientific Theories ?" or van Fraassen's "On the Extension of Beth's Semantics of Physical Theories".

[29] I am grateful to Howard Sobel for suggesting to me the importance of emphasising that, on a semantic conception, a theory is a mathematical entity. He correctly observed that many advocates of the semantic conception, myself included, have allowed the impression to linger that a theory is after all a set of sentences — mainly laws of coexistence and succession.

[30] While van Fraassen and Suppe both advocate a state space approach, there are several differences in their account. For example, Suppe takes a realist position on scientific theories while van Fraassen takes a position he calls constructive empiricism. This difference affects their views on the representational relationship between theory structures and reality. Also, for Suppe there is a distinction between a theory and its phase space models, whereas for van Fraassen there is no such distinction. For him, a theory and one of its phase space models,as distinguished by Suppe, are identical. Suppe contends that for quantitative theories — van Fraassen's exclusive concern — this identification is legitimate but if the semantic conception is to be applied to qualitative theories as well, then the distinction between a theory and its phase space models must be made (see, Suppe, F., "Theories and Phenomena", in W. Leinfellner and E. Kohler (eds.), *Developments in the Methodology of Social Science*, Dordrecht: Reidel, 1974, 87.). Neither of these differences effect the thesis of this paper and I shall, for ease of exposition, follow van Fraassen's identification of a theory with one of its phase space models, although I think Suppe is correct and that his account is more comprehensive.

[31] "Theories and Phenomena", 74—79.

[32] For a detailed account of this level of interaction on a semantic view see, Suppe, "Theories and Phenomena", 74—79.