The Semantic Approach to Evolutionary Theory[1]

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ABSTRACT: Paul Thompson, John Beatty, and Elisabeth Lloyd argue that attempts to resolve certain conceptual issues within evolutionary biology have failed because of a general adherence to the received view of scientific theories. They maintain that such issues can be clarified and resolved when one adopts a semantic approach to theories. In this paper, I argue that such conceptual issues are just as problematic on a semantic approach. Such issues arise from the complexity involved in providing formal accounts of theoretical laws and scientific explanations. That complexity is due to empirical and pragmatic considerations, not one's adherence to a particular formal approach to theories. This analysis raises a broader question. How can any formal account properly represent the complex nature of empirical phenomena?

KEY WORDS: Evolution, semantic approach, theory.

1. INTRODUCTION

Suppes (1962, 1967), Suppe (1972, 1977, forthcoming), Van Fraassen (1970, 1980), and Giere (1979, 1988), among others, have written extensively on the virtues of the semantic approach to scientific theories over "the received view." My interest here is the recent application of the semantic approach to evolutionary theory by John Beatty (1980, 1981), Paul Thompson (1983, 1985, 1986, 1989), and Elisabeth Lloyd (1984, 1986, 1987, 1988). I investigate their reasons for believing that the semantic approach provides a better account of evolutionary theory than the received view.

In the introduction to his book, Thompson states that "... important conceptual and theoretical problems of evolutionary biology have been incorrectly understood, and ... proffered solutions have been inadequate, in large part because of a wide-spread adherence to a particular conception of the formal structure of theories" — namely the received view (1989, pp. 1–2). He also claims that "... these issues can be better understood and resolved when a semantic, rather than a [received] conception of theory structure is presupposed" (1989, p. 3). In particular, Thompson, Beatty, and Lloyd argue that four areas of evolutionary

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theorizing are problematic on the received view but not on the semantic approach.

- First, a number of authors (for example, Smart 1963, 1968, and Popper 1979) argue that evolutionary theory is not a scientific theory because it lacks laws. According to Beatty (1980, 1981) and Lloyd (1988), the semantic but not the received view can counter this argument. Thus only the semantic view can retain the scientific status of evolutionary theory.
- Second, as Thompson (1983, 1989) and Lloyd (1988) illustrate, evolutionary biologists often develop theoretical models prior to knowing whether those models have empirical applications. According to Thompson (1983, 1989), the semantic but not the received view can appropriately account for such foundational work in evolutionary biology.
- Third, Thompson (1986, 1989) argues that evolutionary theory is not a single theory, but a theory of interacting disciplines. He claims that the semantic but not the received view can accommodate the interdisciplinary nature of evolutionary theory. Hence only the semantic view can provide a proper formalization of evolutionary theory.
- Fourth, Thompson (1985, 1989) applies the same sort of argument to received accounts of sociobiological explanations: Sociobiological explanations require information from a number of disciplines. Because the semantic but not the received view can accommodate the interdisciplinary nature of such explanations, only the semantic view can serve as a proper basis for sociobiological explanations.

In this paper, I examine whether the semantic view is better suited for resolving the above conceptual problems than the received view.[2] I do not intend to provide a defense of the received view. I agree with a number of authors that the received view has its faults. But I argue that some of the problems facing the received view confront the semantic view with equal force. In particular, three of the above reasons for preferring the semantic view turn on the complexity involved in providing formal accounts of theoretical laws and scientific explanations. As I argue below, such complexity does not depend on one's formal approach to theories, but on the complexity of the empirical phenomena being studied. Consequently, adopting a semantic approach to evolutionary theory does not alleviate the conceptual issues that arise from such complexity. There is a larger question here than whether the semantic view can resolve some of the difficulties facing the received view. How can any formal account do justice to the complex nature of empirical phenomena? I discuss (albeit in a preliminary and sketchy manner) the basis of such complexity and what it holds in store for any formal approach.

2. AN OUTLINE OF THE APPROACHES

Before reviewing the above reasons for adopting a semantic approach to evolutionary theory, I give a quick description of the major tenets of the received and semantic views. On the received view, a theory is a linguistic item consisting of a deductively related set of formal statements in first order predicate logic. Theories consist of two major components: axioms or laws of nature, and correspondence rules that provide semantic interpretations of those axioms. The axiomatic part of a theory consists of a deductively related hierarchy of axioms. The most general axioms are arranged at the top. Less general axioms (or theorems) are deduced from the more general ones, usually with the aid of a number of simplifying and subsidiary assumptions. (See Cartwright (1983) and Giere (1988) for reasons why the relationship between such axioms may not be deductive.) The vocabulary of these axioms consists of two types of terms: logical and theoretical. The logical vocabulary consists of mathematical terms and logical constants, including quantifiers and modal terms (Suppe 1977, p. 51). Theoretical terms range from abbreviations for sets of phenomenal or observational descriptions, to terms which refer to unobservable entities and their behavior. Such theoretical terms must have empirical interpretations. Those interpretations are given by correspondence rules that relate theoretical vocabulary to observational vocabulary.

The role of correspondence rules has evolved from their original introduction in the 1920s to Hempel's formulation in the 1960s. At first, correspondence rules were thought to provide complete definitions of theoretical terms. That is, they were thought to provide necessary and sufficient observational conditions for each theoretical term. But later, correspondence rules were said to provide *partial* interpretations for theoretical vocabulary. They are the sum total of current and past admissible experimental procedures for applying a theoretical term to observable phenomena (Suppe 1977, p. 25). Such definitions are partial because new technologies may expand (or contract) our stock of admissible experimental procedures for a theoretical term.

The role of correspondence rules in the received view needs to be emphasized. By giving partial definitions of the theoretical vocabulary in the axioms of a theory, correspondence rules provide a semantic interpretation for that theory. Because those definitions are in terms of an observational vocabulary, the interpretations they provide are manifestations of the empirical world. Indeed, for the received view the only sort of interpretation that distinguishes a merely formal axiomatic system from a scientific theory is an empirical one. Of course, a purely mathematical interpretation can be provided, but the emphasis is on giving an empirical one. In addition, the relationship between phenomena and theory via correspondence rules is a two-way street. Correspondence rules provide meaning for the theoretical terms in the axioms of a theory. But they also specify the admissible experimental procedures for applying a theory to phenomena. For example, a correspondence rule specifies what effect an entity denoted by a theoretical term (e.g., 'chromosome') has upon an entity denoted by an observational term (e.g., 'configuration X seen on a light microscope').

A number of objections are lodged against this view of scientific theories. Putnam (1962) and Achinstein (1968), among others, argue that the notion of partial interpretation cannot be made precise and that the observation-theoretical term distinction is untenable. (See Suppe 1972, 1977, and forthcoming for replies to these criticisms.) Suppes (1962, 1967), Schaffner (1969) and Suppe (1972, 1977, forthcoming) maintain that the traditional notion of correspondence rule distorts the relation between theory and phenomena. (I discuss this observation below.) Finally, Suppes (1967) points out that the first-order logic employed by the received view is impractical for formalizing such theories as quantum mechanics. These are just a few of the criticisms launched against the received view. (For a definitive introduction and review of the received view, see the first six chapters of Suppe (1977).)

On the semantic account, scientific theories are classes of abstract or mathematical models. Such models satisfy the axioms (or laws) of a theory and thus provide semantic interpretations for the axioms of that theory. These models are neither empirical nor linguistic entities; they are abstract entities. Indeed, a scientific theory need not have any empirical interpretation. The laws of a theory range over the abstract objects of those models. Laws apply to empirical systems only indirectly, and only when there is an appropriate isomorphic relation between a model and an empirical system. Such isomorphic relations are not a part of the theory itself, but are described by extra-theoretical hypotheses (called "theoretical hypotheses" by Giere 1979, 1988). Proponents of the semantic view hold varying opinions on the nature of the isomorphism required between a theory's model and an empirical system (see Suppe 1979, p. 320).

The major differences between the semantic and the received views can be summarized as follows:

1. For the received view, the main body of a theory is encompassed by its axiomatic structure, which is a linguistic entity. On the semantic account, a theory is first and foremost a class of mathematical (and/or abstract) models.
2. According to the received view, the semantic interpretation of a theory is the empirical world. For the semantic view, it is a set of mathematical or abstract models; an empirical interpretation may or may not exist.
3. The structure of theories given by the received view relies heavily on

the theoretical/observation term distinction: the axioms of a theory consist of logical and theoretical vocabulary, and the theoretical vocabulary of a theory must have corresponding observational vocabulary. The semantic view places no such emphasis on the theoretical/observation term distinction.

4. The received view's correspondence rules provide partial interpretations for theoretical terms via biconditional reduction formulas (according to later formulations of the received view). Furthermore, such rules are parts of the theories for which they provide semantic interpretations. The semantic view holds that the relation between phenomena and theory is much more complicated than that presented by the received view's original notion of correspondence rules: a theory is applied to an empirical system via a number of hypotheses from an assortment of different theories. Furthermore, such applications of a theory are not part of the theory itself.

This is merely a brief introductory sketch of some of the major tenets of the two views. I give a more thorough presentation of some of these differences and other aspects of the received and semantic views below. I now turn to the specific reasons given for adopting a semantic approach to evolutionary theory.

3. THE LAWS OF EVOLUTIONARY THEORY

The central thesis of Beatty (1980, 1981) is twofold: evolutionary theory lacks scientific laws, and the semantic view can account for this fact better than the received view. According to Beatty (1980, p. 541, 1981, pp. 399—400), a scientific theory on the received view must contain laws (or axioms) and those laws must have three characteristics. First, they must be universal generalizations (e.g., 'All instances of A are instances of B') or statistical generalizations (e.g., 'X% of the instances of A are instances of B'). Second, such generalizations are true not only in that they lack exceptions, but also in that such exceptions are physically impossible. In other words, laws have modal force (see Dretske 1977). For example, the law concerning the boiling point of H_2O does not state merely that all H_2O boils at 100 °C, but that all H_2O *must* boil at 100 °C (provided, of course, that the appropriate background conditions prevail, see below). Third, the truth of such generalizations is not merely dependent on the meanings of their constituent and logical terms; their truth must in part depend on empirical features of the world.

Beatty's argument against the existence of laws in evolutionary theory turns on the first two features of laws. As Beatty (1981, pp. 406—407) points out, all genetically based traits are vulnerable to a number of evolutionary forces: mutation, random drift, and countering selection. Because of these forces, a genetically based trait that appears among all the organisms of a species could disappear among some of its next generation's members. The existence of such forces explains why taxonomists rarely find biological traits that occur in all members of a particular species (Hull 1965; Sober 1980). Furthermore, even if such universal traits could be found, mutation, drift, and selection could easily eliminate their universality in the future. Thus the existence of such forces prevents any generalization of the form 'All the members of species A have trait B' from having modal force. Beatty concludes: "In short, there can be no law of nature to the effect that a genetically based trait is universal within a species or among all species" (1981, p. 407).[3]

Smart (1963, p. 54, 1968, pp. 93ff.) presents a similar argument against the existence of laws in evolutionary theory (I show below that Beatty adds a further dimension to this argument). However, Beatty and Smart draw different conclusions from it. For Smart, the argument shows that evolutionary theory is not a genuine scientific theory, because according to the received view such theories must contain empirical laws. For Beatty, evolutionary theory is clearly a scientific enterprise, so the problem lies with the received view. Beatty suggests that an alternative approach to theories is needed and recommends the semantic view. On the semantic view, theories need not contain universal empirical generalizations. Instead, they contain laws that range over mathematical models (Beatty 1981, p. 410; Thompson 1988, p. 72; Lloyd 1988, p. 17). So on the semantic view, the lack of universal empirical generalizations does not destroy the scientific status of evolutionary theory. As a result, Beatty contends that the semantic view of theories should be adopted.

The first aspect of Beatty's argument I want to consider is his claim that there are no empirical laws in evolutionary theory. Specifically, I want to argue that the lack of generalizations concerning particular species does not imply that evolutionary theory lacks empirical laws. Hull (1976, 1978) takes just this line of defense against Smart's employment of the above argument. Following Ghiselin (1974), Hull maintains that taxa are particulars in evolutionary theory. That is, they are spatiotemporally continuous or historical entities. If one is to search for empirical laws in evolutionary theory, one must look for generalizations at different ontological levels in that theory. Instead of looking for laws that concern all the members of a particular species, one needs to look for laws that range over all species of a certain type. Or, one needs to see whether or not there are any laws that range over all organisms of a certain type, independent of their taxonomic membership. (See Ereshefsky (forthcoming) for a discussion of both types of laws.) In brief, showing that there are no laws ranging over all the members of a particular species does not establish that evolutionary theory lacks empirical laws. One must establish that such laws are missing at other ontological levels as well.

Beatty takes on this challenge. Beatty (1981, pp. 407ff.) argues that the evolutionary nature of biological traits prevents the existence of laws that range over the organisms of a certain type (independent of their taxonomic membership). In particular, Beatty maintains that Mendel's First Law and all genetic laws derived from it are not scientific laws. Consider Mendel's First Law. It describes the statistical outcome of meiosis, a process whereby gametes are formed from gamete-producing cells in the reproductive organs. Suppose a gamete-producing cell consists of just one pair of chromosomes and each chromosome contains a single gene, say B and b. In meiosis, the chromosome pair will first double and then divide twice such that the resultant gametes contain only one gene from the original pair. According to Mendel's First Law, of the four resultant gametes, two will contain the gene b and two will contain gene B. Thus according to Mendel's first law, a hetrozygote Bb organism will produce 50% b gametes and 50% B gametes.

As Beatty points out, the process of meiosis itself relies on a malleable genetic basis. So there may be instances of meiosis which violate Mendel's First Law. Indeed, there are well documented cases of meiosis violating Mendel's law. One is called 'meiotic drive.'[4] A well known case of meiotic drive is the Sd gene found in some Drosophila (Crow 1983, pp. 221–222; Crow 1979). During meiosis (before the homologous chromosomes divide) the Sd gene affects the R gene on the homologous chromosome such that the resultant gamete with the R gene will not mature. Consequently, well over 50% of the gametes produced by hetrozygotes will carry the Sd gene. Mendel's law is clearly violated in such cases, for the law predicts that 50% of the gametes will contain the Sd gene. Concerning the lawlikeness of Mendel's law, Beatty makes two general observations: First, Mendel's law is not really a law because there are cases that violate it. Second, and perhaps more importantly, even though most cases of meiosis do obey Mendel's law, the evolutionary basis of that process allows that some time in the future there may be no instances of meiosis that obey that law. (For example, meiotic drive could become the norm, not the exception.) Beatty concludes that Mendel's law and all laws of genetics derived from it are not genuine laws.

But Beatty overlooks the conditional nature of scientific laws. On the received view, laws describe regularities that obtain in the empirical world. Such regularities do not obtain unless certain background conditions are met. The period of a pendulum is proportional to the square root of its length as long as interfering gravitational forces, air resistance, and surface friction are minimal. A small object dropped near a much larger object (say, a gum wrapper dropped near our planet) will accelerate towards that larger object in accord with Galileo's law of free fall provided there are no large gusts of wind or any other interfering events. Like most (all?) scientific laws, Galileo's law of free fall and his law of the pendulum do not explicitly list such conditions but instead contain implicit *ceteris* *paribus* provisos. The same holds for Mendel's first law. Meiosis results in the 50:50 gamete ratio specified by that law as long as certain conditions hold, one of those being the non-existence of meiotic drive. Beatty has not shown that Mendel's law has exceptions. Instead, he has mentioned phenomena that fall outside the range of that law. (Waters (unpublished) draws the same conclusion.)

Some may object that I have preserved the universality of Mendel's law at the cost of making it unfalsifiable. Beatty (1980, pp. 552—553) gives this reply to the suggestion that the lawlikeness of optimality models can be saved by appending appropriate *ceteris paribus* clauses to them. Beatty's complaint is that the number of conditions that fall under such clauses is infinite, making the models unfalsifiable. But Beatty here places too strong a demand on scientific laws. Most (all?) regularities that we describe by natural laws obtain only when a number of conditions are met. Frequently, that number is so large that it is impractical or impossible to list them all. (See for example Giere's (1988, pp. 76ff.) attempt to list all the conditions needed to make Galileo's law of the pendulum complete; for more examples see Cartwright (1983).) Instead, we test conditional clauses in a piecemeal fashion, testing a particular clause when its invocation to protect a law from falsification is questionable. So in the case of Mendel's first law and the invocation of the meiotic drive clause, we need to find independent empirical evidence for the existence of distorter genes (which there happens to be plenty of — see Crow 1983, p. 221, 1979). In short, the invoking of a *ceteris paribus* clause is not automatically an *ad hoc*, empirically untestable maneuver. But neither can the invocation of such a clause be totally empirically justified. We can examine only a limited number of the particular conditional clauses that fall under a general *ceteris paribus* clause.

Still, Beatty can (and does) point out that the problem is not just one of the existence of a few exceptions to Mendel's law that can be handled by a series of (implicit or explicit) conditional clauses. The problem is much worse. Given the evolutionary basis of meiosis, it is possible that in the future *no* instances of meiosis will conform to Mendel's law. Thus Mendel's first law does not represent "any physically *necessary* regularity" (Beatty 1981, pp. 409—410; italics added). Beatty, however, confuses the existence of a physically necessary regularity with the existence of objects that fall under that regularity. Or to put it differently, Beatty incorrectly denies the truth of a conditional because of the possibility that there are no instances of its antecedent. (Sober (1989) makes a similar point.) As I argue above, laws are not statements to the effect that certain phenomena always occur. They are statements to the effect that certain phenomena will occur *if* certain conditions obtain. Pendulums swing according to Galileo's law of the pendulum *if* certain conditions prevail, one of those being the non-existence of strong gusts of wind. Our world may turn into a very windy one, or it could have been such a windy place that no pendulums ever conformed to Galileo's law. Still, the truth of that law does not rest on the existence of a relatively non-windy world, or even the existence of pendulums. The same reasoning applies to Mendel's First law. Our world may change such that all chromosomes carry distorter genes. But that does not falsify the fact that if gamete-producing cells did not have such genes (and there are no other interfering processes), then meiosis would accord with Mendel's law.[5]

In sum, Beatty's arguments for non-existence of empirical laws in evolutionary biology are off the mark (see Suppe (forthcoming) for a semantic theorist who agrees with this conclusion). But suppose Beatty was correct that evolutionary theory does lack universal empirical generalizations. As mentioned above, that would show that evolutionary theory is not a scientific theory on the received view (Beatty 1980, p. 554; Lloyd 1988, pp. 4—5, 18; Thompson 1983, pp. 215ff.). According to Beatty (1980, p. 554, 1981, p. 410) and Lloyd (1988, pp. 17—18), the semantic view should then be adopted because it would preserve the scientific status of evolutionary theory. In what follows I review the premise underlying the second half of this argument, namely that theories on the semantic view need not have empirical laws.

On the semantic view, a theory is primarily a set (or "family") of related models (Beatty 1981, p. 420; Lloyd 1988, p. 35). The laws of a theory determine the possible states of those models; that is, they describe the behavior of the entities within those models (Thompson 1988, p. 72; Lloyd 1988, pp. 19—20; Suppe 1977, p. 226). Such laws specify a set of models that correspond to causally possible empirical systems (Suppe 1972, p. 14; Wessels 1976, p. 230). For example, Galileo's law of the pendulum specifies a set of models that correspond to a set of causally possible systems. A model in that set corresponds to (or is isomorphic with) an actual empirical system when the values of the parameters and variables of that model match the appropriate data points obtained from observing/measuring the empirical system. The relation between laws, models, and actual empirical systems occurs at two levels: first, laws determine the behavior of the models within a theory; second, those models correspond to actual or possible empirical systems. According to the semantic view, laws never apply directly to empirical phenomena, but apply to only the abstract entities in the models of that theory. In this sense, advocates of the semantic view deny that theories consist of empirical laws.

The second tier of this relation comes into play when a model is used to explain the occurrence of an empirical system. Consider the following explication.

On the semantic view, evolutionary theory may be construed as a non-empirical

> specification of a natural-selection system and/or a Mendelian breeding system. Empirical instantiations of the specifications provide the explanatory link between the theories and the behavior of empirical systems. For having identified an empirical system as an instance of a natural-selection and/or a Mendelian breeding system, an investigator can then account for the evolutionary behavior of the empirical system in terms of the consequences of its being an instance of a specified kind (Beatty 1980, p. 555, n. 14; also see Beatty 1981, p. 411; and Thompson 1988, p. 82).

A model serves as a sort of blue-print for a kind of empirical system. Once an empirical system is identified as an instance of that kind (i.e., an instance of that model), reference to the model can then be used to explicate the causal structure of the empirical system and hence to explain the state of that system.

Given this account of explanation, it is hard to maintain that theories on the semantic view lack universal empirical generalizations. An explanation of an empirical system can be given only when that system corresponds to a theory's model. This correspondence requires that the behavior of an empirical system and a model be isomorphic. Advocates of the semantic view say that a model's behavior is described by laws. If the behaviors of an empirical system and a model are isomorphic, then the laws describing the model's behavior describe the empirical system's behavior as well. Indeed, if the same laws describe models and empirical systems, then those laws have empirical content. Thus explanations on the semantic view seem to require the existence of empirical laws. Or to put the point differently, semantic models can be used to explain empirical phenomena only when those models are governed by laws which have empirical applicability.

I have examined the relation between laws, models, and empirical phenomena on the semantic approach because Beatty argues, and Thompson and Lloyd concur, that evolutionary theory lacks universal empirical generalizations. As a result, they claim that evolutionary theory is not a *bona fide* scientific theory on the received view. Theories on the semantic approach need not contain universal empirical generalizations. Thus Beatty, Lloyd, and Thompson argue that the semantic approach should be adopted because it preserves the scientific status of evolutionary theory. However, I have suggested two problems in this general argument. First, Beatty has not shown that evolutionary theory lacks universal empirical generalizations. Second, even if Beatty had shown it, the semantic approach would not save evolutionary theory because explanations on the semantic view also require empirical laws.

4. FOUNDATIONAL RESEARCH

This discussion of laws brings to the fore another alleged advantage of the semantic view, namely that it provides a more faithful representation of how evolutionary biologists perform foundational work. According to Thompson (1983, pp. 227ff., 1989, p. 94), biologists frequently develop evolutionary models without knowing whether those models correspond to empirical systems. In fact, many biological models are developed and *then* biologists debate over whether those models have empirical application. For example, the classical and balance theories of genetics contain competing models for population structures (Thompson 1983). A debate among geneticists concerns which of these established models correspond to actual empirical systems. Similarly, Lloyd (1988) contends that part of the units of selection controversy centers on whether established selection models correspond to empirical phenomena. According to Thompson, the semantic view is well suited for this aspect of scientific practice. Theories are seen as idealized models whose development are distinct from the question of whether they have corresponding empirical instances. Consequently, the semantic view allows the development of models prior to the examination of whether they have empirical applications.

Lloyd and Thompson contend that this is an advantage of the semantic approach over the received account of theories. They are right only if the received view does not allow the development of laws prior to knowledge of whether those laws have empirical applications. But is the received view so restricted? As Thompson (1989, pp. 34—35, 44) points out, theories on the received view are not merely deductive systems of axioms or laws; theories also must contain correspondence rules that provide those laws with empirical meaning. More specifically, on the received view, the theoretical vocabulary in the laws of theory must be partially defined in terms of an observational vocabulary (this is the job of correspondence rules). Of course, the semantic view does not require theories to have such correspondence rules. So at first glance it seems that theories on the received view require empirical grounding, while theories on the semantic view do not.[6]

This distinction, however, is a bit more muddled than that. Consider the general form of the correspondence rules given in the later and more sophisticated formulations of the received view (Thompson 1989, p. 35; see Suppe 1977, pp. 17ff. for a more general discussion). Correspondence rules have the form

$$Cx \rightarrow (Qx \leftrightarrow Ex),$$

where C is an observation term describing a test condition, Q is a theoretical term, and E is an observation term describing a result under that test condition. For example, the theoretical term 'fragile' can be defined by

$$(x)(t)(Sxt \rightarrow (Fx \leftrightarrow Bxt)),$$

where 'Sxt' is 'x is struck sharply at t,' 'Fx' is 'x is fragile,' and 'Bxt' is 'x breaks at t.' Notice that such correspondence rules provide *dispositional* partial definitions of theoretical terms. They specify what sort of observations are associated with a theoretical term *if* certain conditions are satisfied. My point is that correspondence rules provide the laws of a theory with empirical meaning without requiring that those laws have actual empirical applications. The theoretical term 'fragile' is given empirical meaning by the above correspondence rule even if there are no fragile items in the physical world. The rule merely specifies what would happen if such items existed in the world and if those items were subjected to certain conditions.

This notion of empirical meaning is not very far removed from the way Thompson believes theories on the semantic view have empirical meaning.

> The relationship of a model to phenomena is one of isomorphism, and the establishment of the isomorphism is a complex task not specified by the theory. If the asserted isomorphism is not established, it may be that the theory has no empirical *application*. The theory will nonetheless be empirically *meaningful* ... in that one knows from the theory what the structure and the behavior of the phenomena would be if the phenomena were isomorphic to the theory (1989, p. 72).

Of course the manner in which one applies a model or a law to phenomena on the semantic and received views differs. On the received view that job is done by correspondence rules which are part of the theory; on the semantic view it is performed by extra-theoretical hypotheses. Furthermore, the semantic view puts no weight on the observation-theoretical term distinction (but neither does Hempel in some of his later writings, see Suppe 1977, p. 79, n. 171). Nevertheless, the notions of empirical meaning and application are quite similar on both views. Laws on the received view must be empirically meaningful, but they need not have empirical applications. Theories on the semantic view are empirically meaningful — their models must correspond to "causally possible physical systems" (Suppe 1972, p. 14; Wessels 1976, p. 230) — but those models need not have empirical applications. So while it is true that the semantic view permits the positing of models that may or may not have empirical applications, the received view is not excluded from a similar practice concerning laws.

5. THE INTERACTION OF THEORIES

Thompson's (1985, 1986, 1989) primary reason for preferring a semantic approach to evolutionary theory is the following:

> [E]volutionary theory is not a unified theory but a family of theories which interactively constitute a framework for explaining, investigating, and integrating evolutionary phenomena. This view of evolutionary theory cannot be accommodated in a [received] conception but can in a semantic conception (Thompson 1989, p. 129).

Specifically, Thompson believes that there are two areas of evolutionary theory where the semantic but not the received approach can accommodate that theory's interactive nature. The first is in providing a proper formalization of evolutionary theory (Thompson 1986, 1989). The second is the ability to provide an adequate construction of human sociobiology and evolutionary epistemology (Thompson 1985, 1989).

Concerning the first area, Thompson (1986, pp. 75ff., 1989, pp. 51ff.) focuses on Williams' (1970) and Ruse's (1973) sketches of how to formalize portions of evolutionary theory. Although Thompson finds those sketches "notable attempts," he believes that they contain numerous problems. One stands out in particular. Evolutionary explanations typically employ information from a number of disciplines (genetics, selection theory, biogeography, paleontology, developmental biology, etc.). A formal account of such explanations must encompass information from many of those disciplines. Yet according to Thompson, Ruse's and Williams' formalizations of evolutionary theory are too limited to provide an account of such explanations. Ruse provides only an axiomatic sketch of population genetics; Williams provides only an axiomatization of selection theory. Thompson attributes the limited nature of Williams' and Ruse's formalizations to their adherence to the received view of theories.[7] For example, Thompson writes,

> [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 provided (Thompson 1986, p. 80, also see p. 82, and 1989, pp. 67, 99).

Thompson sees the same sort of problem in certain accounts of human sociobiology. As Thompson (1985, 1989) illustrates, an adequate evolutionary account of human behavior and knowledge requires information from a number of disciplines. Evolutionary explanations typically cite such factors as gene frequencies and selection pressures. But instances of cognitive behavior can be related to such evolutionary factors only via information from such disciplines as physiology, cognitive psychology, and neurobiology (Thompson 1989, pp. 103ff.). Despite this need, Thompson (1989, pp. 110–114, 118ff.) faults Ruse's (1986), Dawkins' (1982), and Lumsden and Wilson's (1981) accounts of human behavior for lacking such interdisciplinary information. Thompson's diagnosis of why those accounts lack such information is familiar. Concerning Ruse's work, Thompson writes,

> The problem with Ruse's evolutionary epistemology is that it is formulated within a [received] conception of theory structure. The central problem with adherence to this conception is that it renders impossible the integration of multiple numbers of theories within a causal explanatory framework (Thompson 1989, p. 122, also see, pp. 110—111, 123—124).

What is the source of this pervasive problem? Why can't the received view provide a proper account of evolutionary explanations as explanations that incorporate information from a number of different disciplines? Here is Thompson's (1989, pp. 42—43) rationale. Suppose we want to give a formal account of why there are so many species on the Galapagos Archipelago. Such an account — perhaps modeled on a typical Mayrian (1970) explanation of speciation — requires information from genetics, selection theory, and biogeography. Moreover, such an account must allow that information to be "interactively employed." But on the received view, the theoretical terms of a theory are provided meaning by that theory's unique set of correspondence rules. Consequently, each theory has its own "global meaning structure." Thompson observes that "since different theories have different global meaning structures, they cannot interact" (1989, p. 43). Hence, "theories which are to be interactively employed must be simultaneously axiomatized in a single theory" (1989, p. 43). The resulting problem for the received view is that "[t]his is seldom possible or desirable because such simultaneous axiomatization will be complex and unmanageable" (1989, p. 43). In brief, received accounts of interdisciplinary explanations are just too complex.

Such complexity can arise on two levels of theoretical interaction (Thompson 1989, pp. 95ff.). The first is the theoretical level where the laws of a particular discipline need to be written to allow the input of information (e.g., the value of a variable) from laws in other disciplines. The second level arises when those laws (or their corresponding models) are actually applied to some empirical phenomena. Thompson claims that received accounts of evolutionary and sociobiological explanations are prohibitively complex. But are semantic accounts of such explanations any less complex?

Consider the first level of interaction, the theoretical level. Suppose we want to give a standard adaptational explanation for why a trait has become predominant in a population. Such an explanation would cite, among other things, what effect selection pressures have on that trait plus information concerning how that trait has been genetically transmitted through the different generations of the population. In citing this information, such an explanation relies on a version of the Hardy-Weinberg equation that is supplemented with a selection coefficient. In other words, it relies on a law of genetics incorporating a variable that allows for the input of information from selection theory. Take, for example, the simplest version of the Hardy-Weinberg equation,

$$p^2 + 2pq + q^2 = 1,$$

where p and q represent the frequencies of allele A and allele a in a population of randomly mating, diploid, bisexual organisms and where there is no mutation, migration, or selection. In the case of simple dominance, a selection coefficient can be added by assuming that the fitness of genotypes AA and Aa equals 1, and the fitness of genotype aa equals $(1 - s)$. So an expanded version of the Hardy-Weinberg equation which allows input from selection theory is the following,

$$p^2 + 2pq + (1 - s)q^2 = 1 - sq^2.$$

(For a derivation and discussion of this version of the Hardy-Weinberg equation, see Dobzhansky (1970), pp. 101ff.)

As Thompson (1989, pp. 95ff.) points out, the semantic view is well equipped to handle laws that allow inputs from different disciplines. Such laws are called "laws of interaction" on the semantic view (see van Fraassen 1970, pp. 330ff.). But is the received view any worse off with respect to laws of interaction that connect theoretical terms from various disciplines? In other words, are such laws any more complex and less manageable on the received view?

Consider the just mentioned example of incorporating a selection variable into the Hardy-Weinberg equation. The complexity of such a law turns on the number of variables and parameters built into that law. The choice of which variables and parameters to incorporate is affected by a number of considerations. Versions of the law may be developed with the hope that eventually it will be used to explain certain types of empirical phenomena. So the choice of which variables and parameters to incorporate may be performed with an eye towards some actual or potential phenomena. Pragmatic factors may affect that choice as well. Some variables may be dropped or converted into constants to facilitate the use of the law. Or perhaps the variables in such a law are kept to a minimum so that it may serve as a more abstract, higher level law from which other laws can be derived. In all of these cases, the empirical nature of real and potential phenomena and/or pragmatic considerations determine one's choice of variables and parameters. Those considerations, not one's adopting the received or the semantic approach to theories, determine the complexity of such a law. Indeed, both Thompson (1989, pp. 90ff.) and Ruse (1973, pp. 44ff.) provide similar examples of how to incorporate selection variables into the Hardy-Weinberg equation. The slight variations one finds in their examples do not depend on their different philosophical approaches to evolutionary theory.

On the second level of theoretical interaction, complexity can arise when one actually applies a theoretical law (or its corresponding model) to explain some empirical phenomena. On the received view, such applications depend on the theoretical terms in a law being connected via correspondence rules to observation terms. Thompson finds such correspondence rules unwieldly. Following Schaffner (1969), Thompson (1986, pp. 74–75) argues that the received view's account of correspondence rules "... ignores the ways in which laws from independent theories are employed in 'causal sequences' which causally relate theories to phenomena." Consider the correspondence rule needed for relating the theoretical term 'chromosome' to its appropriate observational terms, specifically relating 'chromosome' to some term reporting the observation of a chromosome under a light microscope. Such a rule would trace the causal path from an observer's sighting to the actual chromosome. In doing so, that rule would depend on laws from a number of different disciplines. For example, the particular chromosome would be stained in order to be observable under the microscope; an account of why staining highlights certain properties of a chromosome requires laws from biochemistry and cellular biology. The properties of the stained chromosome would then cause light to impinge on an observer's retina in a specific way; an account of this phenomenon would depend on laws from physics and human physiology.

Clearly, a full explication of all the links required by such a correspondence rule is quite complex, perhaps prohibitively so. An important insight of a number of proponents of the semantic view (for example, Suppes (1962, 1967), Schaffner (1969) and Suppe (1972, 1977, forthcoming)) is that the complexity of correspondence rules was underestimated by original proponents of the received view (see Suppe 1977, pp. 102ff. for a summary discussion). But does the semantic view escape the complexity associated with the received view's correspondence rules? On the received view, theoretical laws are applied to empirical phenomena via correspondence rules; on the semantic view, theoretical models are applied to phenomena via theoretical hypotheses. I contend that all the complexity found in correspondence rules appears with equal force in the semantic view's theoretical hypotheses.

Consider, for example, Suppes (1962) explication of how a theoretical model is applied to empirical phenomena (an explication strongly endorsed by Thompson (1989, pp. 41–42), Lloyd (1988, p. 146) and Suppe (1977, pp. 106ff.)). Three levels or "theories" mediate such an application. The theory of the experiment describes all possible data sets obtainable from a scientific theory via a certain experimental procedure (Suppe 1977, p. 107). The theory of experimental design is a general statement of the design principles for actually setting up that experiment; in other words, it specifies how to relate a theoretical model of chromosomes, for example, to sightings of chromosomes in a microscope. Finally, the theory of the data describes how the raw experimental data is to be converted into the canonical form given by the theory under investigation ('goodness of fit' questions arise at this level) (Suppe 1977, p. 108).

There are two points I want to make here. First, as Thompson (1989, pp. 41–42) himself notes, all the inter-theoretical complexity built into correspondence rules appears in the second level of Suppes' semantic account of how a model is applied to phenomena. Second, and more importantly, it is not hard to see why the procedure for applying a law or model to empirical phenomena is equally complex on the semantic and the received views. Consider an explanation concerning the behavior of a chromosome. On the received view, the term 'chromosome' must be connected to some appropriate observation via a correspondence rule. That connection is simply the experimental procedures that connect the term 'chromosome' to observations of chromosomes. On the semantic view, the behavior of a chromosome is explained when a model concerning chromosomes is isomorphic to that chromosome. That isomorphism is established by the experimental procedures which connect that model to observations of chromosomes. Clearly such experimental procedures do not vary given one's philosophical approach to theories. So such experimental procedures are equally complex on either view. Recall that Thompson claims that a received view application of a theory to phenomena is prohibitively complex. The above analysis shows that adopting the semantic view in no way alleviates that complexity.

Some readers may argue that I have passed over how the semantic view does in fact alleviate such complexity. Theories on the received view consist of laws, perhaps models, plus the correspondence rules that connect those laws to empirical phenomena. On the semantic account, theories are just families of interrelated models described by theoretical laws; a theory does not contain application procedures for applying its models to empirical phenomena. Viewed in this light, theories on the semantic view lack all of the complexity involved in applying theoretical models and laws to phenomena, whereas theories on the received view contain all that complexity in the form of correspondence rules. So obviously theories on the received view are going to be much more complex. But this argument is too quick. By definition theories on the semantic view avoid the complexity associated with correspondence rules because by definition theories on the semantic view are devoid of application procedures. Yet the complexity associated with giving evolutionary accounts of phenomena still remains; it now just comes under a different heading. Proponents of the semantic view are to be praised for recognizing the complexity involved in applying a theoretical law or model to empirical phenomena. But again, the semantic view in no way alleviates that complexity.

In sum, Thompson (1985, 1986, 1989) maintains that received accounts of interdisciplinary explanations are prohibitively complex. Subsequently, he argues that the received approach cannot provide an adequate formalization of evolutionary theory or sociobiological explanations. Thompson suggests that the semantic approach can better accommodate the interdisciplinary nature of theories and explanations, and thus we should adopt a semantic approach to evolutionary theory. Although I concur with Thompson concerning the complex nature of received accounts, I do not think that such complexity indicates that we should adopt a semantic approach. For as I have argued in this section, semantic accounts of interdisciplinary explanations are equally complex.

6. SUMMARY

Thompson (1983, 1985, 1986, 1989), Beatty (1980, 1981), and others advocate a new approach to evolutionary theory, namely the semantic approach. I agree with them that there are problems and inadequacies in the received view (see Suppe 1977, Chapter 4 for a definitive summary). Thus I am all for explorations of new philosophical approaches to theories, whether they be alternatives to the received view, such as the semantic approach, or revised versions of the received view. But Thompson and Beatty advocate a more specific reason for adopting a semantic approach to evolutionary theory. They maintain that "... important conceptual and theoretical problems of evolutionary biology have been incorrectly understood ..." as the result of adherence to the received view (Thompson 1989, pp. 1–2). And they claim that "... these issues can be better understood and resolved when a semantic, rather than a [received], conception of theory structure is presupposed" (Thompson 1989, p. 3). Thompson and Beatty certainly bring to the fore some of the conceptual problems facing evolutionary theory, but I argue that those problems plague both the semantic approach and the received view.

Consider the two main areas of evolutionary theory that are of concern to Beatty and Thompson: the non-existence of empirical laws in that theory and the interaction of disciplines required for adequate evolutionary explanations. Evolutionary laws have been hard to find and explicitly state for (at least) two reasons. First, the evolutionary nature of taxa prevents the existence of laws that attribute a biological property to all the members of a particular taxon. Thus biologists and philosophers have been hindered in their search for biological laws by their belief that genealogical groups are amenable to law-like generalization. The resolution of this problem has been the result of realizing that genealogical entities are evolving entities, not the result of adjusting one's overall approach to theories. Second, even when a search for laws is shifted to the proper ontological level, the problem remains of not being able to give fully explicit accounts of those laws. It would be simple if all the regularities described by laws consisted merely of a causal factor and its effect. Unfortunately, a causal factor usually gives rise to (or is a positive causal factor for) an effect only when a number of conditions are met. The number of conditions and the complexity of their interactions can be enormous. That number and complexity often hinders our ability to give fully explicit accounts of laws. Adopting the semantic view of theories will not make our attempts to fully represent such laws any easier. Furthermore the semantic view cannot escape this problem by denying that there are such laws, because, as I argue in section three, explanations on the semantic view require the existence of empirical laws.

Such complexity also infects the interdisciplinary nature of evolutionary explanations. Evolutionary explanations require information from a number of interacting disciplines. The interacting nature of evolutionary explanations renders formal accounts of such explanations prohibitively complex. As I show in section four, the complexity of such explanations turns on empirical and pragmatic considerations, not on one's philosophical approach to theories. Thus a semantic approach does not alleviate the problems involved in providing formal accounts of evolutionary theory or sociobiological explanations.

Beatty and Thompson suggest that the semantic view can avoid formalization problems because on the semantic view theories neither contain empirical laws (Beatty) nor the procedures for applying theoretical models to empirical phenomena (Thompson). But this only hides the complexity. A better approach is to recognize that because of the complex nature of empirical phenomena, we can give only incomplete formalizations of theoretical laws and explanations.[8]

NOTES

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[2] For an earlier review of the semantic approach to evolutionary theory (prior to the publication of Thompson 1989 and Lloyd 1988), see Sloep and Van der Steen (1987). My analysis is quite different from theirs.

[3] Beatty (1980, pp. 548ff.) draws a similar but more specific conclusion concerning the design constraints placed on optimality models. According to Beatty, no design constraint necessarily applies to all the members of single population.

[4] Another phenomenon that can cause meiosis to violate Mendel's first law is called 'nondisjunction.' It occurs when homologous chromosomes fail to divide properly at anaphase. One resultant gamete will have an extra chromosome, the other a corresponding deficiency (Crow 1983, p. 19; Beatty 1981, pp. 407—409). Down's syndrome is a result of this phenomenon.

[5] Beatty (pers. comm.) still objects that there is no way to write a non-analytic version of Mendel's First law that takes into account the existence of distorter genes. What happens during meiotic drive shows otherwise. In the case cited above, cells containing R genes do not mature into healthy gametes because they have a chemical abnormality. That abnormality is the result of such cells interacting with cells containing Sd genes (the exact nature of that interaction and the resultant chemical abnormality is not yet known) (Crow 1979,

p. 138). Mendel's law is obeyed in the sense that the production of cells containing R and Sd genes conforms to the ratio specified by the law. The problem is that many of the cells containing R genes do not mature because of the chemical abnormality. So a refined, yet still incomplete, version of Mendel's law can be stated as follows: If an organism is hetrozygote for two particular traits and if the chromosomes produced during meiotic division mature properly, then 50% of the gametes will have one trait and 50% will have the other trait. When the exact nature of the chemical abnormality is known the above law can refer its absence rather than use the blanket 'mature properly' clause. The above rendition of Mendel's first law is not analytic.

[6] Of course, some proponents of the received view allow that during the developmental stage of theoretical laws, the theoretical terms in such laws may lack correspondence rules. See, for example, Carnap (1966, pp. 240—241) and Nagel (1961, pp. 101ff.). Nevertheless, I argue a stronger point below.

[7] Ruse and Williams do not (at least intentionally) limit their formalizations of evolutionary theory as a result of their adherence to the received view. They have other motivations that transcend adherence to any particular account of theories. Williams (1985, p. 580) points out that she does not provide a complete axiomatization of evolutionary theory because "the complete theory is not yet sufficiently well understood to be axiomatized." Ruse (1973, p. 68) merely wants to demonstrate the *possibility* of an axiomatization of evolutionary theory. He observes that the diverse nature of the theory prohibits a full formalization. Thus he tries for second best and formalizes just that part of evolutionary theory that he believes is presupposed by all other parts of the theory — population genetics (1983, p. 48).

[8] The notion of a law or an explanation being incomplete is a common one. Laws that contain *ceteris paribus* clauses are incomplete — they do not list all the conditions that must obtain for the regularities they represent to occur. Hempelian (1965) explanation sketches are examples of incomplete explanations. For a more detailed discussion of why most scientific laws and explanations are incomplete, and why that should not alarm us, see Ereshefsky (forthcoming).

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