

Models

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“Few terms are used in popular and scientific discourse more promiscuously than ‘model’”
(Nelson Goodman [1976, 171])

I. Introduction. Philosophical discussions of models and modeling in the biological sciences have exploded in the last few decades. Given that there are three-dimensional models of DNA in molecular genetics, individual-based computer simulations in population ecology, statistical models in paleontology, diffusion models in population genetics, and remnant models in taxonomy, we clearly should have a philosophical account of such models and their relation to the world. In this essay, I provide a critical survey of the accounts of models provided by philosophers of science and philosophers of biology including models as analogies, relational structures, partially independent representations, and material objects. However, there is much, much more work to be done.

To understand the importance of models philosophically, we must begin at the proverbial beginning with the “received” view of theories. This Syntactic View has almost no need for talk of models except in the thinnest sense. However, it is here that philosophers began to see the need for some notion of a “model”. Let us begin there.

II. The Received (Syntactic) View of Theories. Philosophical discussions of models began as a response to the view of theories articulated by the logical empiricists (see Hempel 1965, 182 – 5 for example). On their account, theories are axiomatic systems given in a formal language. The axioms express purported laws of nature that are true of every

object, do not refer to any particular objects, and are necessarily true. From these axioms, in conjunction with particular premises, one can deduce theorems, which may be a description of some particular event or a less general law.

As a formal syntactical system, the theory is just an array of symbols with operations defined on them. If the axiomatic system is to be meaningful as a scientific theory, one must also specify its semantics. The semantics is provided by an interpretation given to the symbols and expressions of the language. Logical empiricists were particularly concerned with how theoretical terms received their meaning. Entities like electrons, beliefs, and genes were supposedly not observable and so sentences about them were suspect. How could they be empirically meaningful if their meaning was not directly tied to observation?

The logical empiricists articulated the notion of a *correspondence rule* that would completely or partially define theoretical predicates in terms of observable entities and these would be the extensions given to those predicates. For example, consider the following Hempelian classic, “For all objects x and times t , if x is struck at t , then x breaks at t if and only if x is fragile” (Hempel 1965, 109); or in first-order logic $\forall x \forall t [Sxt \rightarrow (Bxt \equiv Fx)]$. So, we define the theoretical term ‘fragility’ partially in terms that denote the actual, observable behavior of struck objects. A large literature arose debating the notion of a correspondence rule and whether theories are axiomatic systems (see Suppe 1977).

It is important to note that the syntactic view of theories does not entail that textbooks, monographs, issues of *Nature*, etc. should include axiomatic systems when they contain scientific theories. This view of theories is consistent with the fact that most theories are not pre-packaged as sets of deductively closed axioms. For example, just because theories of population genetics do not come axiomatized does not show that they could not be (for attempts, see Woodger [1929, 1952], Williams [1970, 1973], Ruse [1973]). Nonetheless, if the

theory could be presented axiomatically, then it was a scientific theory and if not, then it was either not scientific or not a theory. Maybe a metaphysical research program if biologists are lucky.

The foremost problem with the Received View is that it is simply too distant from the way that scientists work and affords very few insights into how and why theorizing occurs in science (van Fraassen [1980, 53-6]). The Received View inspired a cottage industry of technical problems which philosophers were eager to solve. However, these problems shed minimal light on the original questions concerning the nature of scientific theories. van Fraassen writes,

Perhaps the worst consequence of the syntactic approach was the way it focused attention on philosophically irrelevant technical questions. It is hard not to conclude that those discussions of axiomatizability in restricted vocabularies, ‘theoretical terms’, Craig’s theorem, ‘reduction sentences’, ‘empirical languages’, Ramsey and Carnap sentences, were one and all off the mark—solutions to purely self-generated problems, and philosophically irrelevant (1980, 56).

One of the first critical responses to the Received View invoking the notion of a model was the work of philosophy Mary Hesse (1966, see Achinstein 1968 as well).

III. Models and Analogies. As we have seen, on the Received View theories are composed of a formal language, a set of axioms T , and a set of correspondence rules C . According to some philosophers like Richard Braithwaite [1962] and Ernest Nagel [1961], partial interpretations *are* models. Here is a classic example of the approach. The following set of uninterpreted formulas might constitute a part of a formal calculus for the kinetic theory of gases (Achinstein [1968, 227-228], Suppes [1957]).

- (1) The set P is finite and non-empty,
- (2) The set T is an interval of real numbers,
- (3) For p in P , s_p is twice differentiable on T ,
- (4) For p in P , $m(p)$ is a positive real number,
- (5) For p and q in P and t in T , $f(p, q, t) = -f(q, p, t)$

$$\begin{aligned}
(6) \text{ For } p \text{ and } q \text{ in } P \text{ and } t \text{ in } T, f(p, q, t) &= -s(p, t) \times f(p, q, t) \\
&= -s(q, t) \times f(q, p, t) \\
(7) \ m(p) D^2 s_p(t) &= \sum_{p \in P} f(p, q, t) + g(p, t)
\end{aligned}$$

We can informally interpret the formalism as follows. P designates a class of molecules in a gas, T is a set of elapsed times, s_p is the position of molecule p , $m(p)$ is the mass of p , $f(p, q, t)$ is the force that p exerts on q at time t , and $g(p, t)$ is the resulting external force acting on p at t .

However, Mary Hesse [1966] argued following N. R. Campbell [1920] that such interpretations would be importantly incomplete since we have ignored much needed *analogies*. For example, there exists an extremely fruitful analogy between particles in a gas and a set of billiard balls. If we let P designate a set of perfectly elastic billiard balls in a box and we do not change the rest of the interpretation, then one can reinterpret the axioms in more familiar terms. Thus, (5) under the two interpretations are as follows:

- (5') The force exerted by a molecule p on molecule q at time t is equal in magnitude and opposite in direction to that exerted by q on p at time t .
- (5'') The force exerted by a billiard ball p on a billiard ball q at time t is equal in magnitude and opposite in direction to that exerted by q on p at time t .

We can use an analogy between the unfamiliar particles and familiar billiard balls for understanding our kinetic theory of gases.

Now it is obvious that particle in a gas and billiard balls in a box are not identical. There are properties they are known to share and ones that they are not; moreover, there are properties that they *may* share *unbeknownst* to us as well. Hesse names these properties *positive analogies*, *negative analogies*, and *neutral analogies* respectively. As a positive analogy, both particles and billiard balls have mass and velocity and obey a principle of conservation of momentum. Of course, particles in a gas do not have numbers written them though billiard balls do; thus

this is a negative analogy. The neutral properties would be those that we do not know whether they are shared or not.

Hesse distinguishes between two senses of the term ‘model’. What she calls ‘model₁’ is “the imperfect copy (the billiard balls) *minus the known negative analogy*,...” [1966, 9]. She then writes, “Since I shall also want to talk about the second object or copy that includes the negative analogy, let us agree as a shorthand expression to call this ‘model₂’” (1966, 10). Thus, there are two types of models, model₁ and model₂. The former is the analogue including the positive and neutral analogies and the latter is the analogue with the positive, negative, and neutral analogies.

Many philosophers of science at the time recognized that scientists reason with models in Hesse’s senses. However, they argued following Pierre Duhem [1954] that they were dispensable. In fact, some philosophers like Rudolf Carnap would argue that models *should* be dispensed with.

It is important to realize that the discovery of a model has no more than an aesthetic or didactic or at best heuristic value, but it is not at all essential for a successful application of a physical theory (1939, 68).

On her view (and Campbell’s), it was essential to interpret a theory’s axioms in terms that were familiar via an analogy. There are essentially three reasons though it will be the last reason that plays the largest part of her rationale. Models are necessary for explanation, the meaning of theoretical terms, and for novel prediction.

According to Campbell, if a theory is to explain some phenomena, then it must produce understanding in the scientist. The only way to produce such understanding is to provide a model – that which is familiar. Hence, if a theory is to explain some phenomena, thus we must provide models for our theories. However, this argument seems problematic for several reasons. First, it is not clear what this notion of “understanding” is nor is there

good reason to believe that it is necessary for explanation (Salmon 1984). The case that traditionally causes the most grief for this view is that of mid-1920s quantum mechanics. Given that the theory could not be supplied with a hidden-variable interpretation or classical models, there was nothing “familiar” in which to interpret it. However, it was a powerful theory that seems to explain a large number of phenomena.

Second, traditional Received View proponents simply accepted either that there are certain theoretical terms are incapable of being partially interpreted or could only be interpreted implicitly. For the latter, their position or functional role in the theory defines them. According to Hesse, both of these options are deeply problematic. A model supplies an interpretation of the theoretical terms and thus explains how they have the meanings that they do. Given that these issues take us deep into the philosophy of language, I shall leave them here.

Hesse places most of her emphasis on the claim concerning novel prediction. She characterizes theories as either *strongly* or *weakly falsifiable*. A theory is strongly falsifiable if that theory make *novel* predictions. A theory is weakly falsifiable if it only *accommodates* phenomena. On Hesse’s view, theory makes novel predictions by employing models via their neutral analogies. Models suggest possible similarities between the model and the system of interest. Of course, the fact that there is a neutral analogy does not guarantee the novel prediction will be confirmed; but the use of models is necessary for novel predictions to occur. As before, quantum mechanical theory has very few if any models in Hesse’s sense, but has made novel predictions and been strikingly confirmed. Thus, it is false that theories are strongly falsifiable only if they have a Hesse model.

Nonetheless, something like Hesse’s approach can be found to operating in areas of biology. For example, there are many analogies between biological systems and physical

systems. In modeling predatory-prey systems, we use analogies from statistical mechanics involving laws of mass action – predator and prey interact in proportion to their abundances as would molecules in an ideal gas. Similarly, the diffusion of dye particles due to Brownian motion is analogous to a set of populations at an initial gene frequency p “diffusing” away from that value due to random genetic drift (Roughgarden [1996, 69]). Lastly, evolutionary biologists have borrowed heavily from microeconomics and created evolutionary game theory where the concept of utility is analogized with that of fitness (Maynard Smith [1983]). We even see areas of biology borrowing from other areas. Paleontologist Jack Sepkoski [1976, 1978] argued that paleontological phenomena like speciation and extinction of higher taxa (orders in particular) studied in the fossil record are very similar to the species’ colonization and extinction in archipelagos studied in MacArthur and Wilson’s theory of island biogeography [1967]. Thus, Hesse’s general approach has important applications to the biological sciences. Let’s now turn to a more popular and extensive alternative to the Received View – what is called the Semantic View of theories.

IV. The Semantic View of Theories. The Semantic View of theories is probably the most popular approach to theories and models amongst philosophers of science (van Fraassen 1980, Suppe 1989). It has been endorsed by philosophers of biology with much vigor (Beatty 1979, Lloyd 1988, Thompson 1988). However, the Semantic View comes in at least two different varieties. On more “conservative” versions of the Semantic View, the notion of a model is a formal semantic one and the relation between models and empirical systems consists in isomorphisms (van Fraassen 1980, Lloyd 1988). On a “liberal” Semantic View, the notion of a model is simply an idealized, abstract structure and the relation between models and empirical systems is that of similarity (Giere 1988, 1999).

The Semantic View of theories developed explicitly as an attempt to reckon with the problems plaguing the Received View in the hope that many syntactical concerns would vanish. The impetus for the Semantic View comes from model theory. In formal semantics, a model for a set of sentences Γ is an interpretation \mathcal{I} in which all of the sentences are true. If Γ is true under \mathcal{I} , then we say \mathcal{I} *satisfies* Γ . However, in the semantics of formal languages, there are two different ways to construe what a model is. First, we can think of a model as an interpretation function which assigns objects to names, sets of objects to predicates, and n -tuples of objects to relations such that the relevant set of sentences are true. Second, we can think of the model as a set of objects that makes the sentences characterizing the theory true. As Elisabeth Lloyd writes,

Even in metalogic, though, “model” has two meanings: the term refers to either the assignment of terms in the theory to objects (the *interpretation*) or to the objects themselves (the *structures*) (1988, preface).

It is now apparent why the Semantic View is called the *Semantic View*. Its proponents claim that to understand scientific theories we should primarily focus on the semantics of scientific theories and less on their syntax. The relationship of interest is that of satisfaction—models make the theory expressed as a set of sentences true. As the Semantic View has developed the narrow, metalogical notion of models has been questioned (Griesemer 1990, Downes 1992, Giere 1988, 1999). Ironically, the criticisms have been that the metalogical concept of a model is excessively removed from the notion found in the sciences. I now want to turn to the relation between models and empirical systems on the Semantic View.

On the Semantic View of theories, theories are a “family of models.” Ronald Giere writes,

My preferred suggestion, then, is that we understand a theory as comprising two elements: (1) a population of models, and (2) various hypotheses linking those models with systems in the real world (1988, 85).

Giere calls these two components of scientific theories the *theoretical definition* and the *theoretical hypothesis* respectively. There are two important questions that arise in attempting to understand the Semantic View: what are the particular structures of a theory, and what is the relation between these structures and empirical systems?

The most popular answer to the first question is that the models are abstract entities characterized in terms of state spaces.¹ The variables of a system determine the state space. If there are n variables in the system, then the system has n dimensions and is a geometric n -space. The space is defined as the set of all the possible values that the variables can take. If each variable is given a determinate value, then there is a particular point in that space which is the *state* of the system. We can represent this state of the system as a vector and the history of any dynamical system as a sequence of such states or vectors. Likewise, there are also parameters that mediate the relationships between variables.

There are laws that govern the system as well. These laws of succession and coexistence are either deterministic or stochastic. These deterministic or stochastic succession laws determine how the system moves in the space. For deterministic laws, there is a sequence of states $\langle \mathbf{x}_1, \mathbf{x}_2, \dots, \mathbf{x}_n \rangle$ such that for each state \mathbf{x}_i , there is a single state \mathbf{x}_j such that the system moves from \mathbf{x}_i to \mathbf{x}_j . For stochastic laws, there is a sequence of states such that each state has a probability of moving to another state, or remaining in the same state, in the space. Laws of succession are generally encoded in the form of differential or difference equations. There are also laws of coexistence. These are rules that determine what

¹ One important alternative articulated by Patrick Suppes is that the models are set-theoretic structures defined by set-theoretic predicates. For example, a model of the kinetic theory of gas mentioned in §III, can be

regions of the state space the system can occupy. For example, Boyle’s ideal gas law $PV = rT$ is a law of coexistence. An ideal gas can only occupy the subspace where the equality is satisfied. It should be apparent that the laws governing a state space need not be metaphysical laws of nature. Whether this is a virtue or vice is subject to debate and one’s metaphysical predilections.

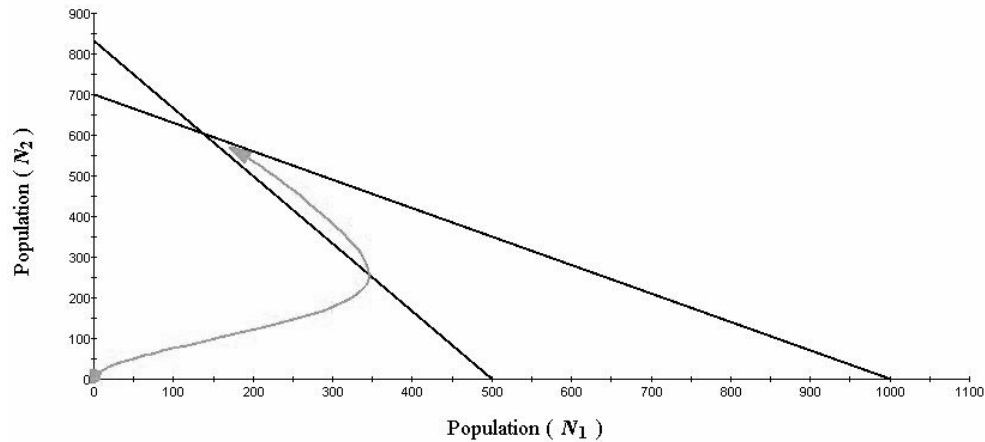
The idea of defining a model as a state space should resonate with theoretical biologists. Many of the classic models in theoretical biology are construed in just this way. For example, the Lotka-Volterra interspecific competition models are explicated as state spaces or as “phase portraits.” Without going into the details of how the model works, the model for two species is described by the following coupled differential equations.

$$\begin{aligned}\frac{dN_1}{dt} &= r_1 N_1 \left(\frac{K_1 - N_1 - \alpha_{12} N_2}{K_1} \right) \\ \frac{dN_2}{dt} &= r_2 N_2 \left(\frac{K_2 - N_2 - \alpha_{21} N_1}{K_2} \right)\end{aligned}$$

The state space is of two dimensions with two variables describing population densities N_1 and N_2 . In fact, this space can be depicted as a Cartesian coordinate system where N_1 is the abscissa and N_2 is the ordinate. The parameters of the model are the intrinsic rates of growth r_1 and r_2 of the two populations, and the competition coefficients α_{ij} which describe the per capita effect of an individual of species j on species i . The differential equations are the deterministic laws of succession for the system. Thus, points in the state or phase space represent the joint densities of the populations at a particular time.

construed as structure that is in the extension of the set-theoretic predicate $\langle P, T, s, m, f, g \rangle$. In this essay, I shall focus on the state-space approach for convenience.

Lotka-Volterra Competition: Phase Plane



Simulation from Populus© Software where $N_1(0) = 10$, $r_1 = 0.9$, $K_1 = 500$, $\alpha_{12} = 0.6$ and
 $N_2(0) = 20$, $r_2 = 0.5$, $K_2 = 700$, $\alpha_{21} = 0.7$.

At equilibrium $dN_i^* / dt = 0$, we have the following laws of coexistence.

$$\begin{aligned} N_1^* &= K_1 - \alpha_{12} N_2^* \\ N_2^* &= K_2 - \alpha_{21} N_1^* \end{aligned}$$

The joint values of N_1 and N_2 that satisfy both equations are regions of the state space that the system can occupy at equilibrium (otherwise known as “isoclines”).

There are two problems with this notion of model structure that I want to mention. First, if *some* models are not mathematical and state spaces are pieces of mathematics, then some models are not state spaces (though see §VII). Second, biologists and philosophers often agree that models are state spaces as in our example. However, this agreement may implicitly disguise a substantive disagreement. The metalogical concept of a model on the Semantic View may be distinct from the concept possessed by biologists. We should not confuse the sameness of the particular structures considered to be models with the sameness of the concept of models.

On the Semantic View, a sharp separation is made between theoretical definitions—abstract entities—and theoretical hypotheses—claims about the relationship between models

and empirical systems. There is controversy over exactly what this relationship is. The more conservative Semantic View claims that the canonical relation between model and world is that of an isomorphism. Hence, we must delve once again into model theory (see van Fraassen [1971, 107-108, 125-126]).

Let us represent a system or state of affairs as being composed of a set of individuals that bear certain relations to one another. Formally, we can represent such a system as a relational structure $\langle D, R_1, \dots, R_n, \dots \rangle$, where D is a set of individuals and R_1, \dots, R_n, \dots are relations on D . We can provide a model for a set of sentences by specifying a domain D and a sequence of relations on that domain $M = \langle D, R_1, R_2, \dots \rangle$. Suppose we have two models M and M^* such that $M = \langle D, R_1, R_2, \dots \rangle$ and $M^* = \langle D^*, R^*_1, R^*_2, \dots \rangle$. M and M^* are isomorphic if and only if there is a one-to-one function g from D onto D^* such that $\langle d_1, \dots, d_n \rangle \in R_i$ if and only if $\langle g(d_1), \dots, g(d_n) \rangle \in R^*_i$ for all members d_1, \dots, d_n of D . In essence, two models are isomorphic if and only if they are structurally identical.

Isomorphism is a very demanding relation to posit between two structures.² A case in point is if the Lotka-Volterra interspecific competition model above is isomorphic to some competing species, then their densities must respond instantaneously to one another, the real-world surrogates of the competition coefficients and carrying capacities must be constant in value, and the density-dependence linear. All of these assumptions are false in many if not all pairs of competing species. If this is true, then there can be no isomorphism between real competing populations qua relational structures and their mathematical counterparts.

² Isomorphism is an equivalence relation between relational structures. Hence, there can be no isomorphism between models and empirical systems since empirical systems are not relational structures. However, if we characterize the empirical system as a relational structure, then there can exist an isomorphism between the models qua relational structure and the empirical system qua relational structure (see Suppes 1962).

One way of dealing with this worry is implicitly suggested by proponents of the more conservative Semantic View. First, distinguish those elements of the model that are idealized and those that are not. We then claim there is an isomorphism, or homomorphism, between the non-idealized elements of the model and the relevant empirical objects, processes, and events. This essentially requires there only is a partial mapping of structure to structure. van Fraassen [1980] provides one way of doing this (see Suppes [1962] for a very different way). In essence, he suggests that empirical adequacy consists in a homomorphism, or in his terminology, an embedding between the empirical substructures of the model and the observable parts of the empirical systems. However, if the idealizations concern observables as well, then our model cannot be mapped onto the observational relational structure. For example, the parameters r_i and α_{ij} do not *always* concern unobservables – we can observe some organisms reproduction and competitive interactions.

Ronald Giere's more liberal account of scientific theories and models begins from a different starting point than the Received View or more conservative versions of the Semantic View. Giere thinks that both of these views are too removed from the structure of scientific theories *in scientific practice*. The first part of Giere's account of theories is that they are composed of abstract, idealized models. This is different from the conservative semantic view in that the models are *designed* to be idealized entities from the start. Moreover, Giere's position does not commit him to the claim that all models are state spaces or necessarily are mathematical even though some are. A model for Giere is simply an idealized, abstract object which can be used to represent an empirical system.

One similarity between Giere's account and the more conservative Semantic View is that in the case of state spaces or other mathematical structures described by equations these models are also metalogical models, namely, the equations describe the abstract objects

truthfully. Thus, phase spaces satisfy a set of sentences, the equations. However, since Giere denies that a model and an empirical system are ever, as a matter of fact, isomorphic, then the model is never isomorphic to the empirical system and hence the empirical system does not satisfy the equations. The empirical system is not a metalogical model for the equations. So, the fact that the equations are true of the phase space is a relatively trivial point and does not collapse his liberal account into the conservative semantic view.

The second part of Giere's account of theories concerns theoretical hypotheses or how models relate to the world. His account is different from the conservative semantic view because he does not believe that models are isomorphic or even homomorphic to empirical systems. He argues that because models are idealized the appropriate relationship is that of *similarity*.

Theoretical hypotheses are linguistic entities for Giere. They are propositions that, "model M bears a relationship to some designated system S ." Moreover, the hypothesis is true just in case M bears that relation to S and false if M does not. However, the relationship between M and S is not linguistic. Models are not sentences, statements, or propositions and hence are not the sort of things that are true or false. Likewise, the real-worldly relationship between models and empirical systems does not consist in some linguistic entity. Hence, a model, empirical system, and their relationship(s) are not the sort of thing that is true or false. It is correct that there are true propositions *about* models, empirical systems, and their relationship(s), but that is an altogether different sort of thing. If the relationship between M and S is not linguistic, then it must be something like a similarity.

Similarity is a troubling notion. Any two objects x and y will necessarily be similar in *some* respects and to *some* degrees. For a theoretical hypothesis to be non-trivial and non-vacuous, then we must specify the relevant respects and degrees of similarity. So, for Giere,

theoretical hypotheses must be of the form,

Such-and-such real system *S* is similar to a designated model *M* in indicated respects and degrees.

As an example, Giere claims,

The positions and velocities of the Earth and moon in the Earth-moon system are very close to those of a two-particle Newtonian model with an inverse square central force. [1988, 81]

A more colloquial way of putting this is “The Earth and moon form, to a very high degree of approximation, a two-particle Newtonian gravitational system” [1988, *ibid.*].³ Let us now consider the reception of the Semantic View by philosophers of biology specifically with respect to evolutionary theory and models. Many have argued that it captures the structure of evolutionary theory via models in a powerful and extremely useful way.

Many philosophers of biology (Beatty [1980, 1981], Lloyd [1988], and Thompson [1998]) have embraced the Semantic View of theories. They have offered several arguments to show how the Received View cannot make sense of biological theories – specifically, evolutionary theory – and how the Semantic View can. Two of the most significant arguments are as follows.⁴ First, according to the Received View, a theory is scientific only if it contains laws. However, evolutionary theory has no laws. However, the Semantic View does not insist that a theory is scientific only if it has laws. Hence, the Semantic View makes sense of how evolutionary theory is scientific. Second, evolutionary theorists often devise models independently of whether those models are isomorphic or fit empirical phenomena.

³ However, this raises problems for the Semantic View. Theories have various epistemic and semantic properties. For example, they can be the object of propositional attitudes, can be confirmed, and can have truth-values. On the Semantic View, theories are pairs of the form <relational structures, propositions>. Set-theoretical structures however cannot be believed, confirmed, or truth-valued. If theories do have these properties and the Semantic View’s “theories” cannot have these properties, then this spells problems. One could adopt a “model-based” propositional view of theories. Theories are sets of propositions where the form of the proposition is “Such-and-such real system *S* is similar to a designated model *M* in indicated respects and degrees”. This amendment would allow us to continue to take seriously our normal scientific practice but also grant the fundamental importance of models in the biological sciences and elsewhere.

The Semantic View distinguishes between theory (theoretical models) and empirical applications (theoretical hypotheses) and the Received View cannot. Hence, the Semantic View makes better sense of evolutionary theory than the Received View. Let's consider each argument in turn.

John Beatty has been a forceful proponent of the claim that evolutionary theory lacks laws. A law of nature in his view is a universal or statistical generalization that supports counterfactuals, makes no essential reference to particular objects, and is necessarily true. However, generalizations of the form, "All members of species *S* have trait *T*" fail to be laws. First, they mention particular objects – species (especially if species are individuals). Second, a law of this form would not have counterfactual force. For any trait, evolutionary process like selection, mutation, and drift can eliminate such characters from the species. Beatty writes, "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, 407).

The conclusion that philosophers of science like J. J. C. Smart [1966] have drawn is that evolutionary theory simply is not scientific given that its lawlessness. Natural history does not science make. However, Beatty argues *given that* evolutionary theory is scientific; the Semantic View makes much better sense of evolutionary theory and its lack of laws than the Received View. The laws on the Semantic View are laws of succession and coexistence which govern the behavior of mathematical objects not necessarily empirical phenomena. Hence, there need not be any biological laws per se.

This argument can be criticized in at least two ways. First, there may be no laws concerning specific taxa (see Lange [1995] though); however, it does not follow that there

⁴ My presentation and criticisms of these arguments is indebted to Marc Ereshefsky's [1991].

are no evolutionary laws.⁵ For example, there might very well be laws over *evolutionary functional kinds* such as host and parasitoid, predator and prey, *r*-selected and *K*-selected species. Second, suppose that a mathematical model is isomorphic to some empirical phenomena. Thus, just as the mathematical structure will satisfy the laws of succession and coexistence of the model, so will the empirical phenomena. Hence, the laws will be true of the empirical system of interest. Thus, the claim that evolutionary theory semantically construed has no “metaphysical” laws is false.

One might charge that given biological models are idealized, then the mathematical models will not be isomorphic to the empirical system. Thus, this criticism is moot. However, this will not allow us to dodge the charge that evolutionary theory has no laws. The laws will be *ceteris paribus* laws – if the system of interest meets the boundary and idealizing conditions, then it would behave in such-and-such a way. For example, if a population at a locus with two alleles *A* and *a* is subject to no mutation, is extremely large, has no migration, etc. then its allele frequencies will change in accordance with the following law of succession.

$$p' = \frac{p^2 w_{AA} + 2pqw_{Aa}}{\bar{w}}$$

Beatty argues against the first criticism by claiming that even evolutionary laws over functional kinds will be false. For example, consider Mendel’s first law – we now know of instances where it is false – in cases of meiotic drive. The truth of law depends on how widespread such cases are. Hence, Mendel’s first law is not necessarily true. However, this argument seems to confuse the boundary and *ceteris paribus* conditions being satisfied and

⁵ See Brandon, Beatty, Sober, and Mitchell in *Philosophy of Science*, 1997, Issue 64 for a discussion of evolutionary laws.

the law being true. Let's now turn to the second argument for the Semantic View and evolutionary theory.

Biologists often develop evolutionary models and only then attempt to determine if the models have empirical applications. For example, there are a large number of theoretical models to explain the existence of sexual reproduction (Williams 1975; Maynard Smith 1978). Likewise, there are models that describe natural selection operating at a variety of units or levels – the allele, gene, chromosome, individual, trait-group, deme, species, ecological communities, etc (Brandon and Burian 1984). In both cases, the models are developed independently of empirical application. It is only then that they are evaluated in terms of their fit to the phenomena of interest. The Semantic View makes a distinction between theoretical models and hypotheses – it preserves this “division of labor”; however, the Received View does not. That is, a scientific theory requires that the theoretical and observational terms being interpreted empirically. Thus, the Semantic View makes better sense of biological practice than does the Received View.

However, the issues are not so clear. Consider the following schematic correspondence rule (where C represents a condition, T a theoretical term, and O an observational term) $\forall x [Cx \rightarrow (Tx \equiv Ox)]$. Correspondence rules do provide *empirical interpretations* but they need not provide *empirical applications*. They tell us, according to the theory, what *would* happen if the relevant set of conditions is met. Of course, whether things are as the theory says they are is an empirical matter and is settled by testing the theory. Similarly, a model on the Semantic View must have empirical meaning otherwise, it is a piece of pure mathematics. However, if it is empirically meaningful, then both the Received and the Semantic View can preserve the difference between a theory and a theory's empirical application. As Paul Thompson writes,

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, 72)

Though the Semantic View has been especially popular amongst philosophers of biology, some philosophers recently have argued that it is deeply flawed. One such group of philosophers is Nancy Cartwright, Margaret Morrison, and Mary Morgan. Philosophers of biology have not picked up on this trend like philosophers of physics and economics have, but it is important to understand their views.

V. Models as Mediators. The “models as mediators” group has provided important criticisms of the Semantic View (Cartwright et. al. [1995], Morgan and Morrison [1999]). This new program is not in the business of providing a “theory of models”. Rather, it is an attempt to understand how models are constructed and function as they do in mediating between theory and phenomena. Margaret Morrison and Mary S. Morgan write,

Although we want to argue for some general claims about models – their autonomy and role as mediating instruments, we do not see ourselves as providing a ‘theory’ of models. The latter would provide well-defined criteria for identifying something as model and differentiating models from theories. [1999, 12]

Nonetheless, the models as mediators group do seem to provide an *implicit* or *functional* characterization of what models are. If we attempt to make explicit such a functional characterization, it would be the following:

A model is that which is constructed and functions as a representation that allows one to learn about theory and phenomena in a way that is partially independent (autonomous) from both.

In essence, models are *technologies*. They are devices that allow one to connect abstract theory and the phenomena of interest. This approach is a form of instrumentalism – though not of the sort philosophers typically discuss. As Cartwright, Shomar, and Suárez write,

I want to urge instead an instrumentalist account of science, with theory as one small component. Our scientific understanding and its corresponding image of the world is encoded as much in our instruments, our mathematical techniques, our methods of approximation, the shape of our laboratories, and the pattern of industrial developments as in our scientific theories. My claim is that these bits of understanding so encoded should not be viewed as claims about the nature and structure of reality which ought to have a proper propositional expression that is a candidate for truth or falsehood. Rather they should be viewed as adaptable tools in a common scientific tool box. [1995, 138]

Why should we believe that models are “mediators” between theory and world? Why should we accept at least the “partial independence” of models from both? One common argument for the position is that theory rarely *applies* directly to phenomena of interest. The conceptual resources of the theory are simply too abstract to characterize actual empirical systems. The only way in which this can be done is through something that mediates between the two – namely, a model. However, models must be at least partially independent of the theory and phenomena. If they are not, then we run into the problem of theory-ladenness *and* phenomena-ladenness; the model will be theory or phenomena and hence objectivity is lost. Thus, models must mediate between theory and world and do so in a way that makes them at least partially independent.

Proponents of the models as mediators approach are not engaged in creating a theory of models as we mentioned before. Most of their work consists in case studies demonstrating how such models work and why they are needed (see Morgan and Morrison 1999 for several case studies). The models as mediators have been severe critics of the Semantic View. Cartwright, Shomar, and Suárez write that according to the Semantic View,

Theories have a belly-full of tiny already formed models buried within them. It takes only the midwife of deduction to bring them forth. On the Semantic View, theories

are just collections of models; this view offers then a modern Japanese-style automated version of the covering-law account that does away even with the midwife. (Cartwright, Shomar, and Suárez [1995, 139])

They go on to argue that “theories plus auxiliaries do not imply data – or better following Matthias Kaiser’s advice in this volume, ‘phenomena’ – even in principle” [1995, 139]. The charge is that the Semantic View is incapable of accommodating the independence and autonomy of models in science. On the Semantic View, theories are in part families of models. Thus, any model must be a member of the family or “derivable” from such a family. However, there are models – often phenomenological models – that are not derivable from theory. Hence, then the Semantic View is false.

The models as mediators have offered several examples of these sorts of cases. In 1934 Fritz and Heinz London provided a model of superconductivity (Cartwright, Tomar, and Suárez [1995]). Mercury when cooled below $4.2K^{\circ}$ will have its electrical resistance drop to near zero so long as it is not in the presence of a strong magnetic field. It turns out that there is a critical phase transition for particular temperatures where it becomes superconductor. One phenomenon of superconductivity that needed to be accounted for is called the *Meissner effect*. This effect occurs when there is a sudden expulsion of magnetic flux from a superconductor when it is cooled below its transition temperature.

The traditional approach was to devise an “acceleration equation” from classical electromagnetic theory. However, London and London realized that the traditional theory-driven account could not account for the Meissner effect. They arrived at model that was independent of (and incompatible with) classical electromagnetic theory, which could account for the Meissner effect. The new model was not simply some “de-idealized” version of the classical theory nor was it built from “theoretical grounds” provided by the theory. Thus, this episode appears to speak against the Semantic View.

Of course, proponents of the Semantic View have responded to this example and ones like it. Newton C. A. de Costa and Stephen French [2004] reply to the example with,

Let us suppose it is true that models exist that are developed in a manner that is in some way independent of theory. Still, they can be represented in terms of structures that satisfy certain Suppes predicates... Whether a model is obtained deductively from theory or by reflecting on experiment, it can be brought under the wings of the Semantic Approach by representing it in structural terms. And there is a general point here: Surely no one in their right minds would argue that all model development in the sciences proceeds deductively! [2003, 55]

Thus, the Semanticists essential claim is that they can concede the partial independence of theory and models. Thus, for any model there is some theory to which it belongs; however, there need not be some *single* such family for most models of a domain. In effect, they are claiming that there are *different kinds of theories* – abstract, phenomenological, data, etc. Of course, one might worry that this is a misuse or trivialization of the term ‘theory’.

One problem with applying the models as mediators approach to the biological sciences is that there are few if any *fundamental theories*. Generally speaking, models and claims about them are all we have. There is then no gap between fundamental theory and phenomena. However, one can still make many of the same sorts of proposals given simply a hierarchy of more or less abstract biological models. Let us focus on a commonly discussed example by both the Semanticists and M&Ms – Newton’s second law of motion and harmonic oscillators. We will then connect the example to biology. Newton’s second law can be written as:

$$F = ma = m \left(\frac{d^2x}{dt^2} \right)$$

where m is mass, x is position, and t is time. The force acting on a body is equal to the mass times acceleration. Suppose we want to model a linear oscillator where the force on a particle

is proportional to the negative displacement of the particle from its rest position. The second law for this linear restoring force is

$$F = ma = m \left(\frac{d^2x}{dt^2} \right) = -kx$$

where k is the constant of proportionality. So, we have added more detail by adding a specific force function for a linear oscillator.

We can also adjust our model so that it is a model of a simple pendulum. Suppose we have a pendulum of length l subject to a uniform gravitational force, $-mg$. A pendulum moves horizontally and vertically. Let us just consider the horizontal component of the motion. The downward gravitational force $-mg$ is partially balanced by the tension along the string S which has a magnitude of $mg\cos(a)$ where a is the angle of displacement. Let us suppose the horizontal component is $-S\sin(a)$. Since $\sin(a) = x/l$, then the equation of motion for x is

$$m \left(\frac{d^2x}{dt^2} \right) = -mg \cos(a) \sin(a) = -x \left(\frac{mg}{l} \right) \cos(a)$$

So, the force is the negative downward gravitational force divided by the length of the pendulum multiplied by the tension of the string. We have defined a force function for a simple pendulum. We can also offer a convenient approximation at this point. If the angle of displacement a is small enough, then $\cos(a) = 1$. Thus, our new equation for a simple pendulum now is

$$m \left(\frac{d^2x}{dt^2} \right) = -x \left(\frac{mg}{l} \right)$$

Finally, consider the case of a damped linear oscillator. Suppose we have a pendulum for which there is air resistance. Let us assume that the friction is a linear function of velocity. So, we have the equation

$$m \left(\frac{d^2 x}{dt^2} \right) = -x \left(\frac{mg}{l} \right) + bv$$

where bv is the friction term. If the friction is significant, then the $x(t)$ cycles will decrease over time (informally, the pendulum's swing decreases with time). We could also add more details; for instance, a driving force that could counteract the friction, but we now can see how the different force functions for the general equation are specified. Moreover, we can see how we can make a simple system like our pendulum into a more complex system by adding things like friction.

What is fundamental in this case is that the assumptions needed to arrive at a linear harmonic oscillator, a simple pendulum, or a damped harmonic oscillator did not follow from Newton's Second Law alone or Newtonian mechanics narrowly construed. We had to make substantive assumptions even some which were only approximately true from our knowledge of oscillators. Thus, we need mediating models at the interface of fundamental theory and phenomena. Now let's see the same point in the context of population biology.

To see the argument, consider a relatively simple example – Lotka-Volterra predator-prey model. The model assumes that $dV/dt = f(V, P)$ and $dP/dt = g(V, P)$; the instantaneous rates of change of the prey (“victim”) population V and prey population P respectively, are functions of prey and predator abundances. In this respect, it is like Newton's second law $f = ma$. However, just like Newton's law, we must specify the “acceleration” term or the functional form of the expressions. To derive the model, let us make the following assumptions:

- Growth of prey population is exponential in absence of predators;
- Predator declines exponentially in absence of prey;
- Individual predators can consume an infinite number of prey;
- Predator and prey encounter one another randomly in a homogenous environment;
- Individuals in the predator and prey populations respectively are ecologically and genetically identical;

So, if we let r represent the intrinsic growth rate of the prey, α represent the capture efficiency of the predator, β represent the conversion efficiency of the predator, and q represent the mortality rate of the predator, then have the following model where V is the prey population and P is the predator population.

$$\begin{aligned}\frac{dV}{dt} &= rV - \alpha VP \\ \frac{dP}{dt} &= \beta VP - qP\end{aligned}$$

In effect, we have used a “law of mass action” in deriving the model (an analogy from chemistry and physics!). The interactions between predator and prey are proportional to their respective abundance. Notice that we are assuming that in the absence of predator, the prey grows exponentially. This is completely unrealistic so we can build into our model density-dependence of the prey population. Thus, we have:

$$\begin{aligned}\frac{dV}{dt} &= rV \left(1 - \frac{V}{K}\right) - \alpha VP \\ \frac{dP}{dt} &= \beta VP - qP\end{aligned}$$

We can also incorporate phenomena like predator satiation since no predator can consume an infinite number of prey. If we let with k be a parameter representing the maximum feeding rate and D is the half-saturation constant, then

$$\frac{dV}{dt} = rV \left(1 - \frac{V}{K} \right) - \left(\frac{kV}{V+D} \right) P$$

$$\frac{dP}{dt} = \beta \left(\frac{kV}{V+D} \right) P - qP$$

What is crucial to realize in each of the cases it that we started with our basic theory Lotka-Volterra “theory” and we developed models one with an assumption of logistic growth on the part of the prey and the other incorporating both logistic growth and predator satiation. However, to arrive at these models we had to make substantive empirical assumptions we could not deduce from our theory. Thus, one might allege the Semantic View cannot account for partial independence of models and theory – even in the context of biology.

VI. Material Models. There is one last topic that I want to consider – material models. Material models take us as far from the traditional concerns of philosophers of science as any we have considered. Jim Griesemer is a philosopher of biology who has proposed “...a picture of model-building in biology in which manipulated systems of material objects function as theoretical models” [1990, 79]. One can “abstract” through a material object a structure independent of a propositional representation. Material models provide a *presentative* role in theory development. These models serve theoretical functions in virtue of close connection to the phenomena under investigation and thus there is no need to build a formal apparatus to represent the system of interest.

For example, consider James Watson and Francis Crick’s material model of the structure of DNA. Watson learned that the physical chemist Linus Pauling had discovered the structure of a protein molecule α -keratin. Pauling discovered its structure by using physical, scale models of the molecule. Not only did this suggest that DNA would be double helical, but also a methodology for discovering its structure. Watson writes,

I soon was taught that Pauling's accomplishment was a product of common sense, not the result of complicated mathematical reasoning. Equations occasionally crept into this argument, but in most cases words would have sufficed. The key to Linus' success was his reliance of the simple laws of structural chemistry. The α -helix had not been found by only staring at X-ray pictures; the essential trick, instead, was to ask which atoms like to sit next to each other. In place of pencil and paper, the main working tools were a set of molecular models superficially resembling the toys of preschool children. We could thus see not reason why we should not solve DNA in the same way. All we had to do was to construct a set of molecular models and begin to play—with luck, the structure would be a helix. [1968, 50-51]

In their final two-chain model, Watson and Crick modeled DNA molecules with sugar-phosphate “backbones” and the adenine, cytosine, guanine, and thymine bases directed inward with metal plates and wire in a structure that stood six feet tall.



Watson (left) and Crick with their DNA model
© Antony Barrington Brown, 1953

This metal model made sense of the amount of water in the open spaces of the molecule, the X-ray diffraction data from Rosalind Franklin, and also Chargaff's rules.

One interesting case study of Griesmer's is that of the remnant models of the naturalist Joseph Grinnell. Grinnell was the first director of the Museum of Vertebrate Zoology at the University of California, Berkeley. Grinnell was particularly interested in expanding evolutionary theory by understanding the “evolution” of the environment – that which drives natural selection. Given California's then pristine state, one could take an

inventory of the vertebrate fauna and the state could be used as a “ecological-evolutionary laboratory” [1990, 81].

Grinnell believed that the environment could be classified according to the causes of the presence and absence of particular species in specified locations. These environments would be so classified according to physiological limits of temperature tolerance of the taxa themselves. Hence, his basic data consisted in the presence or absence of taxa at particular locations and times accompanied by information about the environment. Grinnell then could construct life-zone maps of patches of homogeneity of ecological factors and thus identify the causes and patterns of selection in action.

Models for Grinnell then consisted in a remnant model – a specimen of a taxa with identifying tags tying them to a place, their taxonomic status, and a set of recorded environmental data. A theory could be presented by specifying a set of such models at different locations and times and then accompanied by ecological causal factors by placing them in a Grinnellian hierarchy. Thus, models could be preserved in the Museum of Vertebrate Zoology. Griesemer argues that this is significant for the following reasons.

This is significant because changes of theoretical perspective about the nature of species can be taken into account by pulling the specimens back out of their drawers or off the shelves and reanalyzing the model in terms of a different set of taxonomic designations. This is not possible in the isomorphic *formal* model because once the *information* is recorded that members of a particular taxon were present in a location, there is no recourse—through that information alone—to revise the assessment of specieshood that underlies it, should the theoretical perspective on the nature of species change [1990, 820].

Thus, Grinnell pursued a strategy of “vicarious” material model-building. He created an institution of such modeling through the practices of “...collecting, note-taking, labeling, cataloging, preserving, and storing” [1990, 83].

One of the important things about Griesemer’s analysis is that it can be seen from the point of view of both the Semantic View and the Models as Mediators programs. First,

one can recognize that Grinnell's material models can be used as the basis for theoretical hypotheses about various causal factors shaping taxa. Thus, we have the standard distinction between theoretical models and theoretical hypotheses. However, one wonders what the relation between models and the world is on this account since the models are *part* of the world. From the point of the Models as Mediators program, Grinnell can be understood as working hard both privately and via his home institution to create models that are independent of any theory of the environment.

Recently Netwon C. A. de Costa and Steven French [2004] have argued that material models are analogue models and analogue models can be captured under the Semantic View through the notion of a *partial structure*. In effect, we have relational structures whose domains consist in material objects (or other objects in formal analogies) where each relation R in the structure is actually an ordered triple $\langle R_1, R_2, R_3 \rangle$. Thus, R_1 has in its extension those objects that are known to have the relevant property (positive analogy). R_2 has in its extension those objects known to not have the relevant property (negative analogy). Lastly, R_3 has in its extension the set of objects for which we do not know whether it has the relevant property (neutral analogy). Thus, one can then use the notion of isomorphism, or a less stringent mapping, to capture the relevant similarities between domains. Whether this approach and its notion of "partial truth" will make sense of material models is something left to investigate.

However one ultimately understands material models, they provide resources for reevaluating standard philosophical views.

Instead of reconstructing theories, the new work aims to interpret a variety of representational practices in parallel with increased attention in cognitive psychology to mental maps and 'visual thinking', and in sociology to scientific practice [2004, 433].

Griesemer argues ultimately that 3-dimensional models will force philosophers to come to terms with the heuristics of model building [2004]. In order to understand how a material model represents the world, one must recognize both how the object is made and for what purposes it is made. Thus, an account of material models requires much deeper understanding of scientific practice – one that does not just consider word-world relations.

VII. Conclusion. In this essay, I have attempted to survey the recent and not-so recent work of philosophers of science and biology on models. We have considered models as analogies, relational structures, partially independent representations, and material objects. Whether there is an extant account that can make sense of the bulk of models in biology remains to be seen. However, as we noted in the Introduction, there is much work to be done. Moreover, we have barely touched on the functions that models play in biology, on how they provide explanations, how they can be tested, and the trade-offs that may exist in model-building.

Bibliography

- Achinstein, P. [1967]. *The Concepts of Science*. Baltimore: John Hopkins Press.
- Beatty, J. [1980]. .Optimality-Design and the Strategy of Model-Building in Evolutionary Biology, *Philosophy of Science* 47: 532-561.
- _____. [1981]. “What is Wrong with the Received View of Evolutionary Theory”, in P. Asquith and Giere, R. *PSA 2*, East Lansing: Philosophy of Science Association.
- Braithwaite, R. [1962] “Models in the Empirical Sciences”, in E. Nagel, P. Suppes, and A. Tarski (eds.), *Logic, Methodology, and Philosophy of Science*. Stanford University Press: 224-231.
- Brandon, R. and R. Burian [1984] *Genes, Organisms, Populations: Controversies over the Units of Selection*. Cambridge, MIT Press.
- Campbell, N. R. [1920] *Physics: The Elements*. Cambridge, Cambridge University Press.
- Carnap. R. [1939] *Foundations of Logic and Mathematics*. Chicago, University of Chicago Press.
- Cartwright, N. Shomar, T., and Suárez, M. [1995] “The Tool Bos of Science: Tools for Building of Models with a Superconductivity Example”, in W. E. Herfel et a. (eds.), *Theories and Models in Scientific Processes*. Editions Rodopi, 137-149.
- De Costa, N. C. A. and French, S. [2004] *Science and Partial Truth: A Unitary to Models and Scientific Reasoning*. Oxford, Oxford University Press.

- Downes, S. [1992]. "The Importance of Models in Theorizing: A Deflationary Semantic View", *PSA* 1992 1: 142-153
- Duhem, P. [1954] *The Aim and Structure of Physical Theory*. Princeton, Princeton University Press.
- Ereshefsky, M. [1991], "The Semantic Approach to Evolutionary Theory", *Biology and Philosophy* 6: 59-80.
- Giere, R. [1988]. *Explaining Science*. Chicago: University of Chicago Press
- _____. [1999]. *Science without Laws*. Chicago: University of Chicago Press.
- Goodman, N. [1976]. *Languages of Art: An Approach to a Theory of Symbols*. Indianapolis, Hackett.
- Griesemer, J. [1990] "Material Models in Biology", *PSA 1990*, Vol. 2, pgs. 79-93. East Lansing: Mich. Philosophy of Science Association.
- _____. [2004] "Three-Dimensional Models in Philosophical Perspective", in de Chadarevian, S. and N. Hopwood (eds.) *Models: The Third Dimension of Science*, Stanford, Stanford University Press.
- Hempel, C. [1967] *Philosophy of the Natural Sciences*. Englewood Cliffs, N. J., Prentice Hall.
- Hesse, M. [1966] *Models and Analogies in Science*. Oxford, Oxford University Press.
- Lange, M. [1995] "Are There Natural Laws Concerning Particular Biological Species?" *The Journal of Philosophy* XCII, 430-451.
- Lloyd, E. (1988) *The Structure of Evolutionary Theory*. Princeton, Princeton University Press.
- MacArthur, R. H. and E. O. Wilson [1967] *The Theory of Island Biogeography*. Princeton, Princeton University Press.
- Maynard Smith, J. [1978] *The Evolution of Sex*. Cambridge, Cambridge University Press.
- Morgan, M. and M. Morrison (eds.) *Models as Mediators*. Cambridge, Cambridge University Press.
- Morrison, M. [1999] "Models as Autonomous Agents", in M. Morgan and M. Morrison (eds.) *Models as Mediators*. Cambridge, Cambridge University Press.
- _____. [1983] *Evolution and the Theory of Games*. Cambridge, Cambridge University Press.
- Nagel, E. [1961] *The Structure of Science*. New York: Harcourt, Brace and World.
- Roughgarden, J. [1996]. *Theory of Population Genetics and Evolutionary Ecology: An Introduction*. New York: MacMillan.
- Ruse, M. [1973] *The Philosophy of Biology*. London, Hutchinson and Co. Ltd.
- Salmon, W. [1984]. *Scientific Explanation and the Causal Structure of the World*. Princeton: Princeton University Press.
- Sepkowski, J. [1978] "Species Diversity in the Phanerozoic—Species-Area Effects", *Paleobiology* 2: 298-303.
- _____. (1978) "A Kinetic Model of Phanerozoic Taxonomic Diversity. I. Analysis of Marine Orders", *Paleobiology* 4: 223-251.
- Smart, J. J. C. [1966] *Philosophy and Scientific Realism*.
- Suppe, F. [1977]. *The Structure of Scientific Theories*. Urbana: University of Illinois Press.
- _____. [1989]. *The Semantic Conception of Theories and Scientific Realism*. Urbana: University of Illinois Press.
- Suppes, P. [1957] *Introduction to Logic*. Van Nostrand.
- _____. [1962]. "Models of Data", in E. Nagel, P. Suppes, and A. Tarski (eds.) *Logic, Methodology, and Philosophy of Science*. Stanford: Stanford University Press.
- _____. (1967). "What is a Scientific Theory", in Morgenbesser, S. (ed.) *Philosophy of Science Today*, New York: Basic Books.
- Thompson, P. (1988). *The Structure of Biological Theories*. SUNY Press.

- van Frassen, B. [1971] *Formal Logic and Semantics*. New York, Macmillan.
- _____. [1980] *The Scientific Image*. Oxford, Clarendon Press.
- Watson, J. [1968] *The Double Helix*. New York, Atheneum.
- Williams, M. B. [1970] "Deducing the Consequences of Evolution", *Journal of Theoretical Biology*, 29: 343-385.
- _____. [1973] "Falsifiable Predictions of Evolutionary Theory", *Philosophy of Science* 40: 518-537.
- Williams, G. C. [1975] *Sex and Evolution*. Princeton, Princeton University Press.
- Woodger, J. H. (1929) *Biological Principles: A Critical Study*. London, Routledge and Kegan Paul.
- _____. (1952) *Biology and Language*. Cambridge, Cambridge University Press.