Who is a Modeler?*

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December 1, 2004

Abstract

Standard accounts of the nature of scientific theories ignore a crucial distinction between modeling and other types of theory construction. This conflation badly distorts important contrasts among the goals, products, and practices of modelers and non-modelers. We can see this difference intuitively when we consider the approaches of theorists such as Vito Volterra and Linus Pauling on one hand, and Charles Darwin and Dimitri Mendeleev on the other. Volterra and Pauling were modelers; Darwin and Mendeleev were not. This paper develops an account of theory construction capable of capturing this distinction. The account distinguishes between modeling and non-modeling along two dimensions: the nature of the theory–world relationship and the norms which govern the construction of theoretical representations. By differentiating modeling from other forms of theorizing, we can gain greater insight into the process of theory construction, effective strategies of idealization, and what is really at issue in certain debates between theorists and experimentalists.

*I have benefited tremendously from the helpful comments of Jerome Berson, Barry Carpenter, Zolton Domotor, Edward Epsen, Ben Escoto, Melinda Fagan, Roald Hoffmann, Steven Gross, Jason Rheins, Elizabeth Lloyd, Tania Lombrozo, Angela Potochnik, Scott Weinstein and especially Peter Godfrey-Smith and Deena Skolnick. Earlier versions of this paper were presented at the Institute for Research in Cognitive Science (Penn), Indiana University, and the Center for Philosophy of Science (Pitt). I thank those audiences for vigorous discussion and helpful suggestions.

1 Introduction

After the first World War, there was a curious shortage of certain types of fish in the Adriatic sea. This seemed especially strange because during the war, fishing had dropped off considerably. Most Italians believed that this should have given the fish a chance to increase their numbers. The well-known Italian biologist Umberto D'Ancona was on the case. After carefully analyzing the statistics of fish markets he discovered a very interesting fact: the population of sharks, rays, and other predators had increased during the war while the population of squid, several types of cod, and Norwegian lobster had decreased. How could this be? Why did the small amount of fishing associated with the war favor the sharks?

D'Ancona brought this question to his father-in-law, Senator Vito Volterra, who held a chair of Mathematical Physics at Rome. Volterra approached the problem with what I will call the *modeling strategy.* He imagined a simple biological system composed of one population of predators and one population of prey. He attributed to these populations just a few properties and wrote down mathematical expressions describing them. After carefully studying the dynamics of his model populations, Volterra knew why war seemed to favor the sharks and he had good, yet surprising news: Resumption of heavy fishing would cause the populations to return to their pre-war proportions. (Volterra, 1926)

The strategy employed by Volterra is a common one found in scientific disciplines that face the difficulty of describing, explaining, and making predictions about complex phenomena. When faced with such complexity, theorists can employ one of several strategies: They can try to include as much complexity as possible in their theoretical representations. They can make strategic decisions about which aspects of a phenomenon can be legitimately excluded from a representation. Or they can study a complex phenomenon in the real world by constructing and then studying a model of the phenomenon. This practice is what I call *modeling.*

I believe that the philosophical literature lacks a fully adequate account of modeling and its distinctive features. While there are insightful discussions about the nature of models and proposals for understanding scientific theories as consisting of models, the *practice* of modeling is rarely discussed. The ubiquity and importance of modeling across the sciences makes this neglect especially unfortunate. In this paper, I characterize modeling as it is practiced in sciences such as biology, chemistry, and physics.

In the next section, I will contrast modeling with another form of theorizing which I call abstract direct representation (ADR). Section 3 discusses the characteristics necessary for an account of models to distinguish modeling from ADR. The two subsequent sections (§§4-5) articulate more fully what is distinct about modeling. The paper then concludes by considering the reasons why philosophers have yet to adequately describe modeling (§6) and who is not a modeler (§7).

2 The Essential Contrast

Modeling involves the indirect theoretical investigation of a real world phenomenon. First a theorist constructs a model which is similar, but less complex than the phenomenon of interest. She then analyzes the properties and dynamics of this model. In virtue of the relationship between the model and the world, the analysis of the model is also, indirectly, an analysis of the properties of the real world phenomenon. This is not the only way to study a phenomenon. Phenomena can also be analyzed directly, without the mediation of models. I will call this non-model based form of theorizing *abstract direct representation* (ADR). In order to contrast modeling with ADR, I want to consider two examples in detail: Volterra's discovery of the cause of the post-WWI fish shortage and Dimitri Mendeleev's discovery of the Periodic Table.

2.1 Modeling

Volterra was a modeler. He studied the dynamics of the real Adriatic fish by studying the properties of two model populations. Unlike the myriad properties possessed by two real populations of organisms, Volterra's model organisms possessed just a few properties such as an intrinsic exponential growth rate for the prey in the absence of predators and a constant death rate for the predators. (Roughgarden, 1979, 434)

Volterra gave a mathematical description to these populations in the form of two coupled differential equations:

$$\frac{dV}{dt} = rV - (aV)P \tag{1}$$

$$\frac{dP}{dT} = b(aV)P - mP \tag{2}$$

In these equations, V is the size of the prey population and P is the size of the predator population. The variable r stands for the intrinsic growth rate of the prey population and m, the intrinsic death rate of the predators. The other parameters (b and a) have to do with the functional response and correspond to the prey capture rate and the rate at which each predator converts captured prey into more predator births. (Roughgarden, 1979, 432)

Equations (1) and (2) describe a model in which the population of both predators and prey exhibit periodic oscillations in their sizes. Since there is no stable equilibrium described by these equations, we know that the size of the model populations will oscillate indefinitely. The equations do admit of one unstable equilibrium solution which has a useful property: it corresponds to the time-averaged size of the two model populations. (Hofbauer & Sigmund, 1998, 16) Thus the first step to solving D'Ancona's puzzle is to solve the equations for this equilibrium. We can do this by setting each differential equation to zero. After some algebra, we find that the equilibrium values are:

$$\hat{P} = \frac{r}{a} \tag{3}$$

$$\hat{V} = \frac{m}{ab} \tag{4}$$

Let's define ρ as the ratio of the average size of the predator population to the average size of the prey population ($\frac{\hat{P}}{\hat{V}}$). From equations (3) and (4) we can see that

$$\rho = \frac{rb}{m} \tag{5}$$

The next step is to consider how fishing affects the model populations. We can represent heavy fishing as corresponding to changes or perturbations of r and m. Specifically, heavy fishing decreases the prey growth rate (r) and increases the predator death rate (m). Inspecting ρ, the expression for the ratio of average densities, we can see that $\rho(\text{heavy fishing}) < \rho(\text{normal})$. (May, 2001; Roughgarden, 1979, 439) Since smaller values for ρ mean a larger relative size of the prey population, the population of prey will increase relative to the number of predators during times of heavy fishing.

These results led Volterra to his solution. His model predicted that heavy fishing favors the prey and light fishing favors the predator. Because WWI had slowed Adriatic fishing considerably, his model suggested that the shark population would be especially prosperous. This is not something that Volterra or anyone else would have expected a priori. However, armed with the dynamics of the model, he had a solution to this perplexing problem.

Not everyone was convinced by Volterra's argument. Egon Pearson argued that D'Ancona had not correctly analyzed the fishery data, failing to take into account changes in fishing techniques. (Pearson, 1927) But Volterra and D'Ancona continued to defend the model, arguing that it captured the core factors which accounted for the fluctuating predator and prey populations in the Adriatic. (Kingsland, 1995) This episode is one of the earliest applications of modeling in population biology and since then, modeling has become a central strategy employed by nearly every population biologist involved in theorizing.

Despite the importance of modeling in some theoretical disciplines, it is not the only way for theorists to study phenomena. Volterra solved D'Ancona's puzzle using a model, but Dimitri Mendeleev discovered the structure of the Periodic Table directly, without the mediation of a model.

2.2 Abstract Direct Representation

The story of Mendeleev's discovery of the Periodic Table has a humble beginning. When assigned to teach courses on inorganic chemistry at the University of St. Petersburg, Mendeleev found that there was no good inorganic chemistry textbook available. Inorganic texts lacked an organized and coherent structure from which to characterize the known elements and inorganic reactions. In order to deepen his and his students' understanding of the elements, Mendeleev wanted to develop a classification system that elucidated their underlying properties. This would allow for a more systematic understanding of the properties of each element, the reactions each element could participate in, and trends underlying these properties.

Mendeleev faced a daunting theoretical challenge: samples of the pure elements had many chemically important properties, any of which might form the basis of a classification system. One might sort elements by color, density, conductivity, ductility, melting point, or by their affinity to react with various reagents. In the end, Mendeleev decided to focus his attention on finding trends in the properties of valency, isomorphism, and most importantly, atomic weight. Atomic weight is a familiar concept, but valency and what 19th century chemists called 'isomorphism' may not be. Elements are said to be *isomorphic* when families of salts containing chemically similar, but distinct metals, form similar crystal shapes. (Brock, 1992, 158) 'Valency' refers to the combining ratio of an element. For example, carbon is tetravalent, meaning that it can combine with four equivalents of hydrogen.

Mendeleev's first step was to organize the elements by atomic weight. This gave him a one dimensional ordering of the elements which served as an initial organizational device, but did not reveal any information about the elements' underlying structure or unity. Focusing next on valency and isomorphism, Mendeleev tried to find other dimensions along which to organize the elements. In modern terms, we can think of his next step as trying to figure out where each *period* or row on the Periodic Table ended. Mendeleev literally put the names and properties of elements on cards and played "chemical solitaire" on long train journeys until he found a satisfactory ordering of the known elements. (Brock, 1992, 320)

On February 17, 1869, Mendeleev announced that he had discovered a law which ordered the elements according to their weight and properties. His ordering, which later became know as the Periodic Table of the Elements, organized the elements in order of atomic weight and then in columns or groups in terms of chemical properties.[1]

Mendeleev's achievement is best appreciated by considering the novel predictions he was able to make. In 1869, he noted that there were gaps in his table for three elements. On the basis of information about chemical trends encoded on the table, he predicted the existence of what he called eka-aluminium, eka-silicon, and eka-zirconium. The properties of these novel elements are listed in Table 1. Just a few years later, the elements gallium, scandium, and germanium were discovered and, as indicated on Table 1, their properties were in remarkable agreement with Mendeleev's predictions. (Scerri, 2001, ms.)

Mendeleev's Periodic Table has been continually revised as we have learned more about atomic structure. However, it still serves as one of the most fundamental organizing principles of inorganic chemistry and, in testament to this, can be found on the wall of nearly every chemistry classroom in the world. This achievement surely counts as one of the great triumphs of theoretical understanding in all of science.

Mendeleev's style of theorizing and theoretical achievement were quite different from Volterra's. Mendeleev examined the elements and their properties, abstracted away many of these properties, and then organized the elements such that the factors underlying their properties could be appreciated. This is not an example of modeling. Mendeleev represented chemical phenomena *directly*. His theoretical descriptions were abstract, but he accounted for observable properties of the elements themselves.

Volterra's achievement was quite different. He engaged in *indirect* representation of predator-prey phenomena via the constriction of models. His equations described mathematical models of biological populations and these

[1] In several instances, Mendeleev inverted the atomic weight ordering for the sake of chemical consistency. For example, by atomic weight alone, beryllium would have been in in the nitrogen group. However, its behavior is much more like that of magnesium, so Mendeleev placed it in the magnesium group.

	Predicted	*Discovered*
	Eka-aluminium (1871)	**Gallium (1875)**
Atomic Weight	68	69.9
Specific Gravity	6.0	5.96
Atomic Volume	11.5	11.7
	Eka-boron (1871)	**Scandium (1879)**
Atomic Weight	44	43.79
Specific Gravity	3.5	3.86
	Eka-silicon (1871)	**Germanium (1886)**
Atomic Weight	72	72.3
Specific Gravity	5.5	5.47
Boiling Point	<100 deg.C	86 deg.C
Density	1.9	1.89

Figure 1: Mendeleev's Predictions

models were similar in certain respects to real biological systems, but the equations were not direct representations of any real system. It was only in virtue of the similarity between the models he had characterized and real populations of fish in the Adriatic Sea that Volterra could answer D'Ancona's query. He produced nothing analogous to the Periodic Table; his characterization of the population dynamics of the Adriatic were made indirectly.

In comparing the cases of Mendeleev and Volterra, we can see that a central contrast between their theoretical styles involves the directness of representation. Mendeleev characterized the elements themselves, while Volterra characterized mathematical models, which were similar to real phenomena. In order to make this contrast clearer, we need to examine the structure of scientific models and the role they play in indirect representation.

3 Scientific Models

This section describes the nature of scientific models, the most conspicuous product of modeling. Unlike the very modest literature about styles of theorizing, there is a vast literature about models, and many accounts of models have been offered. This section draws heavily on accounts of models offered by Cartwright (1983), Giere (1988), and Lloyd (1994), but contains additional features which I believe are necessary in order to characterize modeling.

Models are abstract or physical structures that can potentially represent real world phenomena. Many different things can serve as models including physically constructed scale models, model organisms, and trajectories through a state space. While models always have the potential to represent some real world phenomenon, they do not always do so.

Most accounts of scientific models state that models are constructed, originating in a scientist's mind, computer, or workshop. This is a massive oversimplification. Scientific practice shows us that both abstract, mathematical models and physical models can be *discovered* as well as constructed. Biologists make use of discovered physical models in the form of model organisms, an example of which is the fruit fly. No one has constructed a fruit fly from its component molecules. Yet a fruit fly or a population of fruit flies can serve as a very useful model of other organisms. Similarly, mathematical models can also be discovered, but in a somewhat more subtle way. Many of the mathematical structures used in modeling are discovered in the sense that other researchers had first developed their basic mathematical structure. For example, population biologists often borrow mathematical structures from statistical physics for processes such as diffusion.

Theorists often study the properties of models by studying representations of the models, which I will call *model descriptions*. For abstract models, model descriptions usually take the form of equations, but graphs and other kinds of representations can also serve as model descriptions. The typical form of a model description for a physical model is a picture or blueprint, but they can also be represented with equations, computer programs, or other abstract descriptions. Since abstract models do not physically exist in the world, they can only be studied and manipulated through their descriptions. Physical models can be physically constructed and examined directly, hence their model descriptions are often less important than the descriptions of mathematical models.

In some accounts of models, model descriptions are taken to literally *define* models. (Giere, 1988, 83) The relationship is possibly expressed in this way in order to avoid using the term 'satisfy,' the relationship between set-theoretic models and sentences. But 'definition' is too narrow because model descriptions often lack the precision to pick out a single model. Rather than characterize the relationship between models and their descriptions as one of definition, let us characterize the relationship between the description and the model as one of *specification*. This highlights the fact that the relationship is weaker than definition or satisfaction, but that models are picked out by their descriptions.

Modelers often use models in order to learn about real world phenomena. In these cases, the model must be *similar* to the real world phenomenon in certain appropriate respects. As Quine pointed out (1969), similarity is a very vague notion and we therefore should not be content with such a simple formulation of the model–world relationship. There are at least two ways we can give a more detailed articulation of the notion of similarity. Some philosophers such as Giere (1988) argue that the appropriate relationship between models and the world is one of *structural similarity*. On his view, models are concrete yet imaginary structures. Their similarity to real world phenomena lies in some parts of the imaginary structure literally having similar properties to parts of the real world phenomenon. Other theorists such as van Fraassen (1980) and Lloyd (1994) conceive of similarity more abstractly, describing it as a relationship between mathematical properties of the model and the real world phenomenon described mathematically. For the purposes of explicating the distinction between modeling and ADR, one needs some notion of similarity, but either of these is acceptable.

An important difference between my account of models and the standard ones in the literature is that on my view, the model–world relationship is partially dependent on the intentions of the modeler. (Weisberg, 2003; Godfrey-Smith, ms.) Models do not have a single, automatically determinable relationship to the world. Different modelers employing the same model may intend different parts of it to correspond with different parts of a real world phenomenon. Additionally, different modelers may adopt different standards of adequacy. Some may require the model to faithfully represent the causal structure of the relevant phenomenon as well as make quantitatively accurate predictions. Others may only require that the model make accurate predictions. And still others may only require predictions in qualitative agreement with the properties of real world phenomenon.

The relevant intentions of the modelers are included in what I will call the *construal* of the model.[2] The construal of a model is composed of four parts: an *assignment*, the model's intended *scope*, and two kinds of *fidelity criteria*. The scope and assignment set up the connection between parts of the model and parts of the real world phenomenon. The fidelity criteria are the standards theorists use to evaluate a model's ability to represent real phenomena.

[2] I have greatly benefitted from discussing modelers' intentions with Peter Godfrey-Smith, who coined the term 'construal' to refer to these intentions.

The first aspect of a model's construal is its assignment, which is the specification of the target phenomenon and the explicit connection of each part of the model to a part of the target phenomenon. Sometimes one part of a model and one part of a real world phenomenon seem to be such a perfect fit to one another that a particular assignment seems to force itself on us. Both concrete models of airplanes and real airplanes have wings, fuselages, and wheels. This immediately suggests that the assignment would map model wing to real wing, model fuselage to real fuselage, model wheel to real wheel, etc. But sometimes the assignment is more complex. For example, chemists often use a harmonic oscillator model to model covalent bonds. Besides the basic geometry of a spring (connectedness between two points), there is much less pre-theoretic similarity between a spring and a bond. It is only after the modeler chooses an explicit assignment that we know how to coordinate the model and the real world phenomenon.

The second component of a model's construal is the model's intended scope. Since models generally represent features of actual and possible phenomena in simplified ways, we know that nearly every model leaves some factors of the phenomenon out of its representation. A modeler's intended scope tells us the aspects of phenomena intended to be represented by the model.

Consider Volterra's predator-prey model. The model itself only describes the size of the predator and the prey population, the natural birth and death rates for these species, the prey capture rate, and the number of prey captures required to produce the birth of a predator. (Roughgarden, 1979, 267) It contains no information about spatial relations, density dependence, climate and microclimate, or interactions with other species. If the scope of the model is such that we intended to represent those features, Volterra's model does a poor job because it would indicate that there is no density dependence, no relevant spatial structure, etc. By choosing a very restrictive scope, we indicate that Volterra's model is not intended to represent these features.

The third and fourth aspects of a model's construal are its fidelity criteria. While the scope and construal describe how the real world phenomenon is intended to be represented with the model, fidelity criteria describe how similar the model must be to the world in order to be considered an adequate representation. There are two types of fidelity criteria: *dynamical fidelity criteria* and *representational fidelity criteria*.

Dynamical fidelity criteria tell us how close the output of the model must be to the output of the real world phenomenon. It is often specified as an error tolerance. For example, a dynamical fidelity criterion for a predator–

prey model might state that the population size of the predators and prey in the model must be ± 10 of the actual values before we will accept the model.

Dynamical fidelity criteria only deal with the output of the model, its predictions about how a real world phenomenon will behave. Representational fidelity criteria are more complex and give us standards for evaluating whether the model makes the right predictions for the right reasons. These criteria usually specify how closely the model's internal structure must match the causal structure of the real world phenomenon to be considered an adequate representation.

In summary, an account of models adequate for characterizing the practice of modeling must have the following characteristics:

1. Models can be physical or abstract.
2. They can be constructed or discovered.
3. Descriptions of models should be distinguished from models themselves.
4. Models and model descriptions have a many-many relationship.
5. The relationship between models and the world is one of similarity and different accounts of this similarity relationship are possible.
6. The model–world relationship is partially determined by the construal, which depends on the intentions of the model user.
7. The construal along with the world determines whether or not any real phenomenon is represented by the model.

4 Modeling and Indirect Representation

Modelers engage in both direct and indirect representation. This distinguishes them from ADRs who only engage in direct representation. In this section, I will give a more detailed explanation of what I mean by directness and indirectness and explain how modelers' use of indirect representation distinguishes them from ADRs.

A *direct representation* is a set of equations, statements, or pictures that is meant to specify the nature and properties of another structure. In ADR, this structure is a real world phenomenon. Mendeleev's Periodic Table, for example, is a representation of trends in chemical reactivity, valence, and crystal structure.

Modeling also relies on direct representation; however, modelers directly represent models, not real phenomena. Volterra constructed a mathematical model of a population of predators and prey and represented this model directly with two differential equations. Boeing engineers draft blueprints and technical illustrations of precise scale-models of airplanes, which they may go on to test in wind tunnels. Their blueprints and other illustrations serve as direct representations of the model airplane. Each of these cases involves direct representation of a model by giving a model description for the model.

Much day-to-day practice of both modeling and ADR involves the mathematical analysis of direct representations. In examining a mathematical representation of either a model system or a real system, a theorist might ask: What is the system's behavior in the long run? Does it reach a steady state? Does it oscillate? Is it extremely sensitive to the initial conditions? In ADR, theoretical practice terminates with this kind of analysis. Once the representation of the phenomenon is analyzed, theoretical investigation can move elsewhere.

On some occasions, modeling also stops with this type of analysis, either because the model was examined for its intrinsic interest or because the kind of phenomenon described by the model is not found in the world. However, modeling usually involves constructing models in order to learn about real world phenomena. This means that there is further representational work to be done after the model is described and characterized. When a theorist uses a model in order to study a real phenomena, she engages in *indirect representation*.

Indirect representations are similar to direct representations in that they specify the nature and properties of other structures. However, indirect representations specify the properties of other structures via the mediation of a model. When a theorist wants to draw on her analysis of models in order to learn about a real phenomenon, she has to find a model that is appropriately similar to the phenomenon of interest. In virtue of this similarity, the model description and other aspects of the direct representation of the model can also serve as indirect representations of the target phenomenon. The following examples illustrate the role models play in indirect representation.

Molecular models are often used in the course of explaining molecular structure. Say a chemist is interested in explaining the structure of a series of substituted cyclohexane molecules. In particular, she wants to understand the effect that different substitutions will have on cyclohexane's three-dimensional structure. She might employ a classical molecular model, one that treats bonds as rotating springs and atoms as somewhat elastic balls. Using a set of techniques collectively referred to as *molecular mechanics*, the theorist can calculate the minimum energy conformation of substituted cyclohexane models. These models are structurally similar to real world substituted cyclohexanes, whose structure might be determined by NMR, crystallography, or some other spectroscopic technique. If the model's structure is similar to the real substituted cyclohexane structure, then the theorist can use some of the factors which predict and explain the structure of the molecular *model* such as torsional strain, van der Waals strain, or steric hindrance to explain the structure of the actual substituted cyclohexane molecules.[3]

Another example of indirect representation is the analysis of an ecosystem such as an aphid/wasp predator–prey system. Imagine that we want to get rid of aphids with an insecticide, but every time we use one, we find that the aphid population actually increases in size. Realizing that a Volterra or Volterra-like model has a structurally similar dynamic to the aphid/wasp system, we decide to see what we can learn by analyzing this model and its description. As it turns out, we learn that the *Volterra principle* holds of this model. (Roughgarden, 1979, 439) For a certain class of predator/prey systems, the use of a general pesticide will actually have the result of increasing the relative abundance of the prey population. So by looking at the exact results about the behavior of a model system, we learn something important and in this case very practical, about the behavior of a real system.

In both of these cases, the end product of theoretical inquiry is a description of a phenomenon, but one that is given indirectly via the mediation of a model. This description can serve as a description of the real phenomenon because of the phenomenon's similarity to the model. Had these theorists engaged in ADR, they would have attempted to describe the actual phenomenon without the mediation of the model, perhaps relying more heavily on statistical inference.

At this point, one might wonder whether a model is really necessary in this account of theoretical practice. A model description that is a direct representation of a model can function as an indirect representation of a real world phenomenon when the model is sufficiently similar to that phenomenon. But then isn't the model description just an approximate, direct representation of the real phenomenon? Can't we just dispense with the

[3]Molecular mechanics and other techniques of conformational analysis are reviewed in Carroll (1998), Carey and Sundberg (2000), and Lowry and Richardson (1997).

model? This is a serious challenge to the account of modeling I am offering here as well as to attempts by philosophers such as Cartwright, Morrison, and Morgan who emphasize the unique role models play in theoretical inquiry. While addressing this challenge in its entirely calls for its own paper, I believe one can respond as follows.

Since I am offering an account of a theoretical practice, not a rational reconstruction of a finalized theory, the appropriate locus of investigation is the activities theorists actually engage in. Let us consider three reasons for modeling: to describe a real-world phenomenon, to study a general hypothesis, to study a phenomenon which is known to not exist.

First consider the case where a theorist uses a model in order to describe a selected real world phenomena. Even though the theorist has pre-selected a phenomenon in the world to study, the early stages of modeling involve the analysis of the structure and dynamics of a model. During this time, the theorist will not know how similar the model is to the real world phenomenon of interest. Her primary object of study is the model. Since she does not know how similar the model is to the real world phenomenon, the model description and the analysis of the model cannot really be approximate ADRs of the phenomenon. At the end of a theoretical investigation, when the model has been determined to be similar enough to the real phenomenon, it may be possible to treat the model description as an approximate description of the real world phenomena. But this is really irrelevant to what I have been discussing. I am concerned with the practice of modeling, not possible rational reconstructions after the fact. Thus even when theorists intend to describe real world phenomena, the practice of modeling cannot be described without talking about models and indirect representations.

The second kind of modeling is even more awkward to try to redescribe as ADR because is has little to do with any particular real world phenomena. Some modelers construct models to explore or illuminate a hypothesis. For example, population biologists often examine very simple models of sexual and asexual reproduction in order to better understand the evolution of sex. (Roughgarden, 1997, x) These models are not intended to describe any actual organism; they are far to simple for that. Their importance lies in helping us to understand very general facts about the differences between sexual and asexual reproductive systems. In this type of case, the model just is the object of study. It does not make sense to even consider dispensing with the model, because that is what the whole investigation is about.

Finally, it is also possible to engage in the study of a phenomenon that is known not to exist. A. S. Eddington once wrote: “We need scarcely add that the contemplation in natural science of a wider domain than the actual leads to a far better understanding of the actual." (1929, 266) Agreeing with Eddington, R. A. Fisher explained that the only way to understand why there are always two sexes involved in sexual reproduction is to construct a model of a three-sexed sexually reproducing population of organisms. Constructing a model of such a phenomenon is the only way to study it because, by stipulation, the phenomenon does not exist. Modelers are often interested in phenomena such as three-sex biology, perpetual motion machines, or non-aromatic cyclohexatriene because, insofar as we can understand why these phenomena do not exist, we will have gained a better of understanding of the phenomena that do exist. But again, there is no way to describe such a theoretical investigation without the mention of models. Thus I conclude that the suggestion to recast the practice of modeling without the model is hopeless.

5 Representational Ideals

Modeling and ADR also differ in the demands of their *representational ideals*, the goals governing the construction, analysis, and evaluation of theoretical representations. Pre-philosophical reflection and much of the literature about the structure of theories assume that scientists always aim at complete representation. Let's call this representational ideal COMPLETENESS. I will use COMPLETENESS as a running example through this section in order to illustrate many of the properties of representational ideals. However, since COMPLETENESS is only adopted by practioners of ADR, not modelers, I will close the section by discussing several modeling representational ideals.

According to COMPLETENESS, the best theoretical description of a phenomenon is a complete representation. The relevant sense of 'completeness' has two components: First, each aspect of the target phenomenon must be directly mappable onto an aspect of the representation. Additionally, anything external to the phenomenon that gives rise to its properties must also be included in the representation. Structural and causal connections within the target phenomenon must be reflected in the structure of the representation. The second component has to do with the *fidelity* of the representation. According to COMPLETENESS, the best representation is one which represents all aspects of the target phenomenon with an arbitrarily high degree of precision and accuracy.

COMPLETENESS can be used to illustrate the most important property of representational ideals: they serve as goals that guide rational modeling. Although this seems fairly trivial, the issue turns out to be more complex when we consider an interesting fact about the ideal of complete representation: COMPLETENESS is not a goal that theorists believe they can achieve. Unless extremely self deceived, theorists know that for most phenomena, it will be impossible to generate complete representations. In adopting COMPLETENESS, a theorist is *aiming* for complete representation, but knows she will not achieve it. Given this fact, how can COMPLETENESS serve as a goal?

There are actually two ways that it can serve as a goal in cases of sub-optimal representations. Although extremely demanding, COMPLETENESS is not simply an all-or-nothing standard. Rather, it sets up a scale with which one can evaluate all representations including sub-optimal ones. If a theorist wants to compare several representations of the same phenomenon and has adopted COMPLETENESS, she has a straightforward way to do so. The closer a representation comes to completeness, the better it is.

Even more importantly, COMPLETENESS and other representational ideals are similar to what Kant called *regulative ideals*. They do not describe a cognitive achievement that is literally possible; rather, they describe a target or aim point. They give a theorist guidance about what she should strive for and the proper direction for the advancement of her research program. If a theorist adopts COMPLETENESS, she knows that she should always strive to add more detail, more complexity, and more precision to her representations. This will bring her closer to the ideal of completeness, although she will never fully realize this goal.

At the beginning of this section, I claimed that modelers do not adopt the same representational ideals as ADRs. In fact, one of the most distinct properties of modeling is that COMPLETENESS is not adopted.This does not mean simply that modelers idealize, and fall short of giving complete representations. Falling short of complete representation is also characteristic of ADR even when COMPLETENESS is adopted. Modelers do not even *aim* at complete representation. They explicitly acknowledge from the outset that their models are intended to be incomplete. Hence idealization is built in to their representational ideals.

Since there are many ways to idealize, there is no single representational ideal characteristic of all modeling. What modelers' representational ideals do have in common is that they deviate from the goal of complete representation in some way or other. They may also set additional goals not related to complete representation. What follows is a partial list of representational ideals associated with modeling.[4]

[4]While theorists do adopt these ideals, they often do so implicitly. The names attached to these ideals have been adopted for convenience and are not to my knowledge found

SIMPLE	Use the simplest possible model that still reproduces the qualitative behavior of the phenomenon of interest.
P-GENERAL	Sacrifice other desiderata to maximize the number of actual & non-actual phenomena the model applies to.
O1-CAUSAL	Restrict the causal factors included in the model to the first order causal factors.
RELAX	Allow the appraisal of models with lower standards of fidelity.
MAXOUT	Maximize the precision & accuracy of the output.
IMPRECISE	Describe models with imprecise model descriptions.
NARROW	Allow restrictions in the scope of models.

This partial list of representational ideals is restricted to very general strategies of idealization. When we start thinking about specific kinds of idealizations that arise in the context of particular sciences, the list gets much larger. For example, a chemist might restrict herself to classical molecular models, neglecting quantum effects. A population biologist might systematically neglect spatial structure in models of selection, predation, and competition, even though she knows this excludes many biological facts. Meteorologists might construct models in which the earth is represented as a perfect sphere with no geographical features. Such possibilities greatly expand the list of possible representational ideals for modelers.

Given that there are many possible representational ideals for modelers, how do modelers know which ideals to pick? Is their choice merely a historical contingency of their modeling tradition? While there are undoubtedly historical facts that partially explain why modelers pick the representational ideals that they do, I think that it is possible to give an analysis of the underlying rationality of their choices that goes beyond any historical contingencies. Such an analysis has yet to be produced in its entirety, but several studies have addressed the issue in a preliminary way. (Wimsatt, 1987; Weisberg, 2003, forthcoming)

I think that the proper way to approach this issue follows Richard Levins' insightful discussion in "The Strategy of Model Building in Population Biology" (1966). Although not usually understood in this way, one important insight of Levins' article was that it showed us the proper way to study the representational ideals of modelers. The first step in understanding how modelers choose ideals, what Levins calls the 'strategies of model building,' is to understand the constraints on successful representation. Levins focused

elsewhere in either the philosophical or scientific literature.

exclusively on what he called the *tradeoffs* faced by modelers — sets of properties which cannot be simultaneously realized to the maximal degree. A more expansive analysis would also take into account other ways that properties of models and their descriptions can constrain one another. Once we understand the constraints faced by modelers, we can show why adopting a particular representational ideal is appropriate for a particular scientific goal.

To make these ideas more concrete, imagine a theorist interested in studying the dynamics of predation. This theorist did not have in mind any particular predator-prey system; rather, she wanted to learn more about particular kinds of interventions on the dynamics of predator-prey systems. For example, say the theorist wanted to know about the effect of using a pesticide that affects both species on a two-species system, what ecologists call a *general pesticide*. Because the most important consideration in this kind of analysis is learning about the dynamics of the maximum number of predator–prey systems both actual and possible, the theorist's best strategy is to adopt P-Gen as a representational ideal. The goal of this analysis does not require giving a maximally complete representation of an actually existing system. Instead, it requires studying the maximum number of possible systems, even if some predictive power if sacrificed. Hence P-Gen is more appropriate than Completeness.

If one adopts P-Gen, then, following Levins' strategy, one would begin by thinking about the tradeoffs that affect the maximal achievement of the relevant type of generality, what I call *p-generality*. In an earlier analysis (2003), I argued that one could increase p-generality by using either more complex or less precise model descriptions. One can also increase p-generality by using a set of models which embody different basic assumptions about the structure of predation.

Analyzing a set of predator-prey models to discover the effect of general pesticides has actually been carried out and is presented in textbooks on the theory of population biology. In a well-known treatment of the issue, Roughgarden considered Volterra's original model as well as ones including density dependence of the prey's intrinsic growth rate and predator satiation. She reports that in each kind of model predator-prey system, a general pesticide will increase the relative number of the prey, a result which is called the *Volterra Principle*. This result is true of each type of model that she investigated. Consequently, it holds true of many possible predator-prey systems. For a very large set of possible predator-prey systems, the use of a general pesticide would increase the relative population size of the prey, which is usually the unwanted pest. (Roughgarden, 1979, 441) This analysis increased p-generality by considering a set of possible predator-prey systems using models that make different basic assumptions. Roughgarden did not try to craft any particular model that captured the details of any particular predator-prey system with high fidelity.

What if a theorist was interested in the use of pesticides and the dynamics of predation, but for very different reasons? Imagine a population biologist acting as an advisor to the Department of Public Health during a disease epidemic. In this imaginary case, it is known that rats are the vector of the disease and the population biologist's task is to help construct an effective program of rodenticide. In such a case, p-generality is irrelevant, or at least a secondary consideration. Correctly representing of the exact nature of the predator–prey coupling between rats and cats is also of secondary importance. The most important goal is to make highly precise and highly accurate quantitative predictions about the effects of particular pesticides on the population of rats. In such a case, it would be most appropriate to adopt an ideal such as MaxOut.

Although modeling predation is a relatively simple example, it exemplifies several important properties of modeling. In modeling, the choice of a representational ideal is not automatic; it must be based on careful consideration of the modeler's goals. Further, the best way to connect modeling goals with choices of representational ideals is through an analysis of the constraints faced by modelers. Connecting the analysis of tradeoffs and other constraints to the various goals of modeling will allow us to develop a set of rules governing the rational choice of representational ideals.

6 Why the Essential Contrast was Overlooked

Having laid out the main characteristics that distinguish modeling from ADR, it is time to take a step back and ask why this contrast was overlooked in earlier literature. I believe that two main factors have contributed. Accounts about the structure of theories tend to either ignore models altogether or focus exclusively on models. The second reason is that the most commonly discussed examples in the theories literature obscure the differences between modeling and ADR.

One can classify philosophical accounts of scientific theory into two groups — one that treats theories as being composed of sets of propositions and one that treats theories as being composed of sets of models. Falling in to the first category are classical accounts of the structure of theories such as the ones offered by the logical empiricists, Bayesian accounts of confirmation, and Kitcher's account of the evolution of theoretical practice (?, ?). These theorists do not attribute any role to models in the structure of theories or theoretical practice. In most instances, this neglect of models comes from the contention that a rational reconstruction of theories can be done in a logically formal way, where theories are understood as an axiomatic system or as a set of explanatory patterns. Whatever the reason for neglecting models in giving an account of theories, it is not hard to see why you cannot give an adequate account of modeling if you do not attribute any importance to models themselves.

A second group of accounts about the structure of theories is often called the *semantic view*. Closely associated with the writings of Patrick Suppes and co-workers, (Suppes, 1960) semantic theorists have reconstructed physical, psychological, and biological theories as sets of models, rather than as sets of axiomatic sentences. (Domotor, 2001) There is actually a good deal of variety among proponents of the semantic view as to the nature of scientific models. Those most closely associated with Suppes have tended to argue that scientific models are simply logician's set-theoretic models. Later philosophers in this tradition tended to describe models in terms closer to those actually used by scientists either conceiving of models as trajectories through a state space (Fraassen, 1991; Lloyd, 1994) or as imaginary, yet concrete constructed systems (Giere, 1988).[5]

While these philosophers were right to emphasize the role models play in theoretical inquiry, they make a surprisingly similar mistake to the one made by logical empiricists. Whereas Hempel, Nagel, Kitcher, and others ignore the importance of models, semantic view theorists treat all theory as being comprised of models. For example, Giere says that "a theory [is] comprised of two elements: (1) a population of models, and (2) various hypotheses linking those models with systems in the real world." (1988, 85) This over-emphasizes the importance and ubiquity of models.

Fundamentally, both classical and semantic accounts of theories suffer from a common problem: they both treat theorizing as a single kind of activity with a single product. Although a legitimate simplification for some purposes, this distorts the nature of theories and theorizing. As I hope to have demonstrated in this paper, there are significant differences between modeling and ADR. A complete account of theories and theorizing needs to explain both.

The second reason the distinction between modeling and ADR has been

[5]For a review of the diverse accounts of models defended by proponents of the semantic view see Downes (1992).

obscured is because of some particular characteristics of modeling in physics, from which the most commonly discussed examples are drawn. Modeling and ADR are harder to distinguish in physics because of the background theory that physicists deploy when constructing their models. When physicists confront a new physical phenomenon that they need to account for, they already have a relatively short list of physical forces and entities from which they can construct a model. Each encounter of a new planet, plasma, or semi-conductor does not require new mathematics and new concepts to be developed. Physicists' extensive experience with these kinds of entities allows them to construct models by analogy to other known cases. They also possess powerful and well-understood theories such as Newtonian Mechanics, General Relativity Theory, and Quantum Mechanics from which a mathematical description of the relevant model can be drawn. This is not to say that modeling is easy; it just means that physicists can usually draw on background theory and prior experience in constructing their models.

The situation looks very different in population biology. While Volterra could draw on some background knowledge about ecology, he did not have a comprehensive and systematic theory of population dynamics that strongly delimited the processes and entities he could appeal to in constructing and characterizing his models. As a result, his construction of predator-prey models looks a lot more autonomous from background theory than modeling in physics does. This difference is illusory; physicists' models are no less autonomous than population biologists' models. However, it is easier to appreciate the fact that physicists construct models and engage in modeling when one examines examples drawn from outside physics.

While it is true that much of the philosophical literature has neglected the distinction between modeling and ADR, there are several philosophers who have come close to articulating the distinction. Mary Morgan, Margaret Morrison, and Nancy Cartwright offer accounts of models that capture some aspects of the distinction I have developed in this paper.

Like the proponents of the semantic view, Morgan and Morrison argue that models play an indispensable role in theoretical science. But unlike proponents of the semantic view, Morgan and Morrison do not understand theoretical models as theory itself; rather, they emphasize the autonomy of models as *mediators*. By characterizing models as mediators, Morgan and Morrison want us to see models as playing a role in theory that is analogous to the role instruments play in experimentation. Models are connected to theory, but are autonomous. They are constructed with the aid of background theory, but are neither constitutive of background theory nor are they directly derivable from it. (1999)

The person who comes closest to developing the view of modeling I have defended is Nancy Cartwright. Her clearest articulation of the practice of modeling comes in her presentation of the *simulacrum* theory of explanation. In a simulacrum explanation, a theorist begins by constructing a model of the phenomenon that she wants to explain. This model is behaviorally similar to some phenomenon in the real world. After constructing this model, the theorist derives the behavior of the *model* from fundamental theory. For example, if we are modeling a molecular bond as a harmonic oscillator, we derive the behavior of the model bond using the classical mechanical description of a simple spring. Cartwright argues that by giving a covering-law explanation of the model system, we have explained the behavior of the much more complicated real world phenomenon. (Cartwright, 1983)

Cartwright's account of explanation is fascinating in its own right, but its importance for us is the picture it gives us of modeling. In Cartwright's view, the modeler represents a real world phenomenon indirectly through the use of a model. The modeler then makes claims about the behavior and structure of her model. In virtue of the similarity between the model and the real world, these are also claims about a real world phenomenon. This is the essence of modeling as I understand it. Although under-appreciated in the literature, I think Cartwright has correctly articulated this core aspect of model-based explanation. Despite this, she errs along the lines of Suppes and van Fraassen implying that theoretical representation almost always involves the construction of a model. As such, Cartwright cannot fully distinguish between modeling and ADR.[6]

7 Who is Not a Modeler?

I want to close this paper with a brief discussion of the common mistake of treating all instances of idealization and abstraction as modeling. All theoretical representation involves abstraction, the process of systematically ignoring aspects of the phenomenon of interest. This is just the difference between theorizing and simply giving a report about the raw data. Almost

[6] I put the point in this way because of the distinction Cartwright makes between phenomenological laws and fundamental laws. She argues that particular phenomena can be described by highly complex and truthful phenomenological laws. This construction could be seen as a kind of ADR, but one that was highly specific to particular phenomena. Although this technically would allow her to make the distinction between ADR and modeling, Cartwright believes that scientific explanation is done using fundamental laws and models. Even if she allowed there to be a distinction between modeling and ADR, only modeling would get to count as explanatory theory.

every scientific paper that is published involves some form of abstraction, since this allows raw data to be examined for patterns. Take Mendeleev as an example. Although a practitioner of ADR, not modeling, Mendeleev relied heavily on abstraction. He synthesized the data from studies of many samples of the elements, then abstracted away almost all of the properties of the elements, leaving only atomic weight and a few others. This process of abstraction is what allowed him to determine the correct ordering of the elements on the Periodic Table. I have very deliberately chosen to call Mendeleev's theoretical style *abstract* direct representation.

A more common mistake is to associate all instances of idealization with modeling. Prima facia, this association seems more plausible, because modeling does depend on idealization in an important way. However, practitioners of ADR also engage in idealization. So what is the difference?

In modeling, one chooses a *representational ideal* that includes idealization; it is not simply accepted as inevitable. One accepts idealization as a strategic choice and decides which aspects of a phenomenon are going to be represented and at what level of fidelity. In ADR, we might be absolutely certain ahead of time that the representations we write down are going to be idealized, but if we accept the norm of COMPLETENESS, higher degrees of fidelity are always preferable. ADR and modeling thus differ in their attitude toward idealization. ADRs know some degree of idealization is inevitable, but usually work toward eliminating it. Modelers embrace the necessity of idealization and try to make strategic decisions about which kinds of idealization will help further their scientific goals.

8 Conclusion: Who is a Modeler?

The structure-of-theories literature typically assumes that there is just one kind of theorizing, and hence just one structure of theories. While I have not said much about the structure of theories in this paper, I have argued that the first assumption is a mistake: modeling and ADR are distinct kinds of theorizing. In order to understand modern theoretical practice, one must recognize that only some theorists are modelers.

Two main factors distinguish modeling from ADR. First, modelers engage in indirect representation. When they want to describe a real phenomenon, they begin by constructing or discovering a model. This model is described and analyzed, which can be done mathematically, pictorially, or even verbally. If the model is found to be appropriately similar to the real world phenomenon of interest, then the modeler's representation and analysis of the model is also an indirect representation of the real world phenomenon. However, sometimes a model is analyzed to answer very general questions that are not about any specific real world phenomenon or that are about a phenomenon that is known not to exist.

The second characteristic of modeling has to do with the modeler's choice of representational ideals. Unlike many ADRs, modelers never adopt the ideal of COMPLETENESS when using models to represent a target phenomenon. Instead of aiming at complete representation, modelers choose representational ideals that involve some kind of idealization. This can take the form of including structure known not to be in the target, leaving out structure which is known to be in that target, and choosing less stringent fidelity criteria. Modelers make strategic choices about which representational ideal to adopt by considering the particular demands of their research interests. This flexibility about representation is not possible in ADR because COMPLETENESS is nearly always adopted.

Although modeling and ADR may not uniquely divide up the domain of theoretical practice, they are two of the most important kinds of theorizing. Future accounts of theories and theorizing should carefully distinguish between them and further elucidate their properties. Only after we have a better handle on the diversity inherent in theoretical practice, will we be in a position to answer some of the most philosophically challenging issues about theorizing including when modeling is an advisable strategy, what kinds of models to construct, and how to go about choosing the most fruitful representational ideals.

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