EXPLORING THE NATURE OF MODELS IN SCIENCE, PHILOSOPHY OF SCIENCE, AND SCIENCE EDUCATION

BY

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ABSTRACT

The term *model* is ubiquitous in science, philosophy of science, and science education. Although there is no general consensus regarding its definition, the traditional approach in all of these disciplines has always been to view models as some kind of representation of reality. Recently, however, there has been a move towards non-representational and deflationary accounts of modeling that eschew the notion that models must come equipped with both necessary and sufficient conditions. Following the philosophy of the later Wittgenstein, I develop my own narrative concerning modeling called the integration account. The integration account maintains that models are comprised of various elements that are organized in a distinctive way in order to solve scientific problems. Some of these elements include, but are not limited to: theories, laws, theory-ladenness of ideas, choice, funding availability, feasibility, social relationships, and even serendipity.

The integration account encounters some difficulty when it is applied to the field of science education, in particular science teaching. The latter's pedagogical project appears to run contrary to the integration account's commitment to solving scientific problems. As a result, I propose that pedagogical models and scientific models be viewed as separate kinds of models, each replete with their own separate function. Scientific models should only be used by professional scientists to solve scientific problems and not used as teaching tools by science teachers. The reverse is also true. Pedagogical models can still be used by science teachers even if they have run their course when it comes to solving scientific problems.

For my wife

Amanda Belarmino

and my parents

Jesus and Nora Belarmino

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Chapter 1

Introduction

1.1 Plato's Model *Mythos*

In Book 7 of the *Republic*, Plato tells his audience an amazing tale. He welcomes them to use their imaginations to envision a cave that extends far beneath the Earth's surface. Deep inside this cave, towards the very bottom, there are very unique prisoners. These prisoners are shackled in a way that they are forced to stare at the cave wall in front of them; they cannot move their heads from side to side or even look behind them. Far behind the prisoners, towards the cave's entrance, is a burning fire. In between the fire and the prisoners there is a slightly elevated path. On this path, there are people carrying various statues of everyday objects from one side of the cave to the other. Now the statue carriers are holding the statues in such a way that the shadows of the statues are visible to the prisoners, but the statues themselves are not. Because the prisoners cannot turn their heads, they do not know who or what is producing the shadows. In fact, from their perspective, they do not even know that they are looking at shadows; for all they know, the shadows are the only genuine reality. One day, however, one of the prisoners breaks free from his shackles. Once unfettered, he immediately turns around and follows a path propelling him towards the entrance of the cave. As he is walking, he is astonished at what he sees; for he sees statues that resemble the ones he has been staring at his entire life. After considering the statues and their likenesses on the wall, he realizes that the two cannot be a coincidence. The objects on the wall are nothing more than the shadows of these statues. At this moment, the prisoner's entire reality is turned upside down.

Everything he thought was real was only a mere likeness, a dismal chimera. For his entire life he had witnessed the most elaborate puppet show ever produced, unfortunately none of it was real. He wants to turn back the hands of time, return to his rightful place in front of the wall, and call out the names of shadows again. But he knows that this is impossible. Instead, he summons up the courage to keep climbing towards the entrance of the cave. As he approaches the entrance, he sees a burning fire and realizes that it is the mechanism that has been perpetuating the farce known as his life. He wants to put it out, but he cannot. So he keeps on climbing. As he gets closer to the entrance to the cave, his vision becomes more and more impaired. For his entire life he had grown accustomed to looking at things in the dark; and now, all of a sudden, he is inundated by light. As he begins to walk outside, his vision gradually returns. At first he is only able to see the shadows of objects on the ground and the reflections of objects on the water's surface. Eventually he lifts his head and is able to look at the objects themselves, the ones producing the various shadows and reflections. He has never seen anything so beautiful in his entire life. But out of nowhere, he feels a sudden urge, an ineffable compulsion prompting him to gaze up to the heavens above him. And that is when he catches a glimpse of the sun, its radiance extending from one end of the horizon to the other. In that moment, he knows his journey is complete. What began as a day that was a kind of night, has become the one, true day.

Those remotely familiar with Plato's famous Allegory of the Cave recognize the philosophical impact that the myth has had on metaphysics, epistemology, aesthetics, politics, and education. What is rarely recognized, however, is the myth's influence on philosophy of science, in particular, the philosophy of modeling. The shadows on the wall of the cave are models of the statues being carried along the pathway, whereas the statues themselves are representations of the actual objects outside of the cave. Plato's point is that the objects outside represent the true Reality, while the shadows on the wall inside the cave are the farthest removed from the realm of immortal, incorporeal, and unchanging Forms. Now there is a great deal of metaphysics in Plato's myth, which makes it even more ironic that the scientific community, by and large, has adopted his philosophy of modeling. When Plato speaks of his Theory of Forms as being the ultimate Truth, he is referring to the abstract truths of mathematics. For Plato, all physical objects in the natural world are poor representations, or mere shadows, of their mathematical counterparts. Even though Plato developed his view of mathematics in the 4th century B.C.E, consider what Johannes Kepler, one of the most important astronomers to come after Nicholas Copernicus, said in the *Mysterium Cosmographicum* nearly two thousand years later, "The ideas of quantities have been and are in God from eternity, they are God himself; they are therefore also present as archetypes in all minds created in God's likeness."[1] Or, more recently, consider the words of theoretical physicist Paul Dirac, one of the pioneers of quantum mechanics, "As time goes on, it becomes increasingly evident that the rules which the mathematician finds interesting are the same as those which Nature has chosen."[2] For Plato, Kepler, Dirac, and many others, mathematics is more than just a mirror of nature—it is the one, True Nature.

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1. Johannes Kepler, *Mysterium Cosmographicum*, in Arther Koestler, *The Watershed: A Biography of Johannes Kepler* (New York: University Press of America, 1960), 65.

2. Paul Dirac, *Proceedings of the Royal Society of Edinburgh*, in "Science Quotes by Paul M. Dirac," Todayinsci, 1999 – 2017, accessed on August 10, 2017, https://todayinsci.com/D/Dirac_Paul/DiracPaul-Quotations.htm.

When I initially began this project, my intention was to defend Plato's mathematical approach to models and modeling. After years of research on the topic, I experienced a Kuhnian "*gestalt* switch" and actually reached the opposite conclusion: models, even mathematical models, are not mirrors of nature, and models do not need to represent. This view was largely influenced by the likes of Nancy Carwright, Nancy Nersessian, Mauricio Suarez, Mary S. Morgan, and Margaret Morrison, all of whom challenged the traditional, or Platonic, approach. In particular, I sympathize with Nancy Cartwright's simulacrum account of modeling where a model has "the form or appearance of a certain thing, without possessing its substance or proper qualities."[3] In other words, just because a model may bear similarities to its target, it does not necessarily follow that it represents it. Representationalists maintain that looking at a model gives us a glimpse into Nature, whereas I have come to the realization that looking at a model is more akin to "see[ing] through a glass, darkly."[4] The following exploration chronicles the anti-representationalist view I have adopted, along with its philosophical, scientific, and educational implications.

1.2 Summary of my Key Claims

This dissertation is formally divided into two separate, but essentially interrelated, parts. In Part I, chapters 2 – 4, I embrace the challenge of producing an adequate account of *modeling* conducive to scientists, philosophers of science, and science educators alike. My primary contribution is the development of the integration account of modeling whereby a model is best understood as a composition of its various components that

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[3] Nancy Cartwright, *How the Laws of Physics Lie* (Oxford: Clarendon Press, 1986), 17.

[4] 1 Cor. 13:12

come together in a unique manner in order to solve a scientific problem. These elements include, but are not limited to: theories, laws, theory-ladenness of ideas, politics, funding availability, feasibility, various social dynamics, personal motivation, choice, and even serendipity. The integrated account is both anti-representationalist and deflationary. It is anti-representationalist in that it denies that models can ever provide us with a true mirror of reality, and it is deflationary in the sense that it has conceded the search for both necessary and sufficient conditions regarding what a model is. The best way, then, to understand what a model is is through the concept of modeling spheres whereby different kinds of models are represented through concentric spheres. The most basic and universal models (e.g., physical, explanatory models) are located near the center spheres, while the more abstract and unorthodox conceptions (e.g., formal, mathematical models) retreat towards the outermost spheres. The idea is that even though the models within these inner and outer spheres are wildly different, they are all models nevertheless.

In Part II, chapters 5 – 7, I explore the possible application of the integration account to science education. This task proved to be a far more difficult one than I initially expected given the vastly different approaches to modeling between professional scientists and science educators, specifically teachers. On the one hand, professional scientists' penchant for problem solving makes them more likely to embrace the integration account given the latter's non-representationalist stance. On the other hand, science teachers are burdened with the difficult task of knowledge transmission, which usually involves the communication of scientific facts and information. Sadly, the integration account fits rather uneasily with most teachers' pedagogical goals given its imprecise and open-ended nature. Hence, my discovery, and this is the key take away from Part II, that professional scientists and science teachers use models differently to accomplish their specific purposes. The Bohr model of the atom might be a poor scientific model of the atom in that it egregiously misrepresents the behavior of electrons, but it can still be a useful pedagogical model for beginning students, especially when it comes to identifying the different parts of an atom. Contrarily, the liquid drop model of the atomic nucleus might be a prodigious scientific model given its ability to account for fission; be that as it may, its depiction of the atomic nucleus as a classical fluid might escape the comprehension of even the most advanced physics student.[5] In order to avoid any further confusion, I insist that we make a clear distinction between scientific and pedagogical models and avoid conflating the two.

1.3 Outline of the Study

Chapter 2 is going to investigate the three major approaches to representation: correspondence, denotation, and "models as mediators." The correspondence account will be divided into its stronger and weaker versions (isomorphism and similarity), both of which I will argue are insupportable. Next, denotation will be discussed, paying particular attention to R.I.G Hughes's DDI (denotation, demonstration, and interpretation) account. I will make the case that although denotation has some advantages over the correspondence theory, it is insufficient all the same. Finally, I will explore Mary S. Morgan and Margaret Morrison's "models as mediators" account. The "models as mediators" position is different from both the correspondence and denotation

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[5] Margaret Morrison, "One Phenomenon, Many Models: Inconsistency and Complementarity," *Studies in History and Philosophy of Science Part A* 42, no. 2 (2011): 343 – 349.

explanations in that it moves farther and farther away from the notion that models must be representations of a target system.

Chapter 3 is where I develop my own account of models and modeling. I will begin by using Marcel Bouman's account of modeling as a starting point and develop my position from there. Next, I will use Wittgenstein's conception of language-games to defend the deflationist and naturalistic attitude inherent in my integration account. Then, I will consider three criticisms against my position: (1) that I am guilty of the fallacy of composition, (2) that my account fails to distinguish models from theories, and (3) that I commit the naturalistic fallacy. After answering each of these charges in depth, I will explain the advantages that my integration account has over the three modeling accounts described in chapter 2. As part of my response to criticism (2), I formulate the modeling spheres model.

In chapter 4, I perform a close reading of Pierre Duhem's, *The Aim and Structure of Physical Theory*. In his book, Duhem dismisses mechanistic models on the grounds that they: concretize abstract phenomena, are illogical, and are dispensable in the long run. I will respond Duhem's criticisms one by one; in the process defending the viability of physical models and what Duhem derogatorily refers to as the "English mind." Towards the end of the chapter I will discuss the irony of Duhem's anti-modeling position and argue that if he were alive today, he would be at the forefront of the modeling movement.

Chapter 5 examines how educational researchers and practitioners approach modeling. The chapter begins by looking at the various ways models have been discussed in the science education literature. After the similarities and differences between the various conceptions are pointed out, they are all compared to the integration account provided in chapter 3. The chapter concludes by investigating teachers and students' perceptions of models. The research suggests that both appear to hold on to a realist epistemology, the purpose of which is to ascertain the ultimate Truth. During this discussion, an important distinction is made between scientific models (the models scientists use to solve problems) and pedagogical models (the models teachers create and use).

Chapter 6 includes an in-depth investigation of three modeling curriculums that have already been implemented in the classroom. The first case study involves middle school students creating mathematical models through the scientific method. The second case study observes high school engineering students participate in an engineering design challenge whereby they have to design a neighborhood playground. The final case study focuses on a group of upper secondary Biology students and their exposure to a model based teaching and learning strategy consisting of several distinct modeling stages. Each case study will be described in great detail and analyzed. Towards the end of the chapter I will comment on what I consider to be an alarming trend in STEM education, namely the proliferation of mathematical models at the expense of physical, mechanical models.

In the final chapter, chapter 7, I revisit some of the dissimilarities between scientific and pedagogical models made in chapter 5, and extend my argument to student models as well. Just as a science teacher's knowledge does not mirror the knowledge of a practicing scientist, similarly a student's mental models does not mirror that of her teacher. As part of this conversation, I discuss some of the shortcomings of student mental models (e.g., they are oftentimes incomplete, limited, unstable, etc.) and a few of the complications that teachers have getting students to their “zone of proximal development” regarding them. Towards the end of the chapter, I suggest that even though we should view the relationship between scientist, teacher, and student models as a *societas* (i.e., one built on mutual respect), rather than a *universitas* (i.e., one built on a shared essence), it does not follow that I am advancing a relativistic theory of models and modeling. This chapter also includes why I do not believe that children should be thought of as little scientists, as well as further arguments supporting the scientific model / pedagogical model distinction.

Part I

Chapter 2

Three Accounts of Representation

2.1 Introduction

If you ask any number of scientists, science educators, and science teachers what a model is, the most popular response you will receive is that a model is a representation of its target. This answer seems innocuous enough, but ultimately it leaves much to the imagination because it fails to provide a robust account of representation. For example, the claim that the solar system model of the atom is a representation of a real atom is meaningless unless it is further specified in what respect the solar system model of the atom is a representation of a real atom. Fortunately, philosophers of science have recently become quite interested in the topic of scientific representation. Now after a thorough investigation of this burgeoning literature, three distinct accounts of representation have emerged: the correspondence account, the denotation account, and the models as mediators account.

2.2 The Correspondence Account

The correspondence account of representation maintains that models represent their targets by corresponding to them. Built in to this account of representation is the notion that there are degrees of correspondence ranging from perfect or ideal correspondence on one end of the spectrum to very little correspondence on the other end. No philosopher of science believes that models are or should be perfect copies of their targets for at least two reasons. First, in many instances it is physically impossible to perfectly recreate most target systems in science for a number of reasons: they are either too big or too small, we lack the requisite building material, or we lack the requisite "blueprints" for building a particular model. In the end, then, Paul Teller is correct in his observation that "The only PERFECT model of the world, perfect in every little detail, is, of course, the world itself."[1] This naturally leads to the second reason why models should not be thought of as perfect copies of reality: "One of the damn things is enough."[2] If the size of the solar system and the atom were manageable enough that we could study them directly without building models of them, then we would. But the fact of the matter is that their sizes are untenable and thus need to be modeled.

Now although perfect correspondence might be off the table, many of the proponents of the correspondence account believe that models only need to be identical to their targets in the only way that matters: structurally. In the literature, this view is referred to as the isomorphic account. According to isomorphism, some model x is a representation of its target system y if and only if x shares the same structure as y. This implies that the following relationships between models and their targets hold: (1) x and y must be symmetric, (2) x^1, x^2, and y must be transitive, and (3) x must be reflexive.

A model is symmetric to its target just in case x is a representation of y and y is a representation of x. Consider a physical model of the Golden Gate Bridge and the actual Golden Gate Bridge. Isomorphism requires that the physical model represents the actual bridge and that the actual bridge represents the physical model.

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[1] Paul Teller, "Twilight of the Perfect Idol," *Erkenntnis* 55, no. 3 (2001): 410.

[2] Anonymous in Nelson Goodman, *Languages of Art* (Indianapolis, IN: Hackett Publishing Company, Inc., 1976), 3.

Next, a model is transitive of its target if the following relationship holds: If some model x^1 represents a target y, and some other model x^2 also represents the same target y, then models x^1 and x^2 each represent one another. Let x^1 represent a drawing of the Golden Gate Bridge, x^2 represent a physical model of the Golden Gate Bridge, and y represent the actual Golden Gate Bridge. This means that if a drawing of the Golden Gate Bridge represents the actual Golden Gate Bridge, and a physical model of the Golden Gate Bridge also represents the actual Golden Gate Bridge, then the drawing must represent the physical model.

Lastly, isomorphism requires that models be capable of representing themselves. This simply implies that a drawing of the Golden Gate Bridge represents a drawing of the Golden Gate Bridge and the same applies to physical models as well.

The critics of isomorphism maintain that all of the preceding relationships fail in one way or another. The main argument against symmetry is that it is simply not the case that if some model x represents a target y, that target y represents model x. Although it is true that a physical model of the Golden Gate Bridge represents the actual Golden Gate Bridge, it does not follow that the actual Golden Gate Bridge represents the physical model. As a matter of fact, the actual Golden Gate Bridge does not represent anything; rather it is that which is represented. The problem with transitivity is that even if it is true that models x^1 and x^2 represent the same target y, it is not the case that models x^1 and x^2 represent one another. Even though it is true that the a drawing of the Golden Gate Bridge and a physical model of the Golden Gate Bridge both represent the actual Golden Gate Bridge, it is not true that the drawing and the model represent one another. The argument against reflexivity is that a model does not represent itself; rather it is always a representation of something else, in particular some kind of target. A physical model of the Golden Gate Bridge is not a representation of itself; on the contrary, it is a representation the actual Golden Gate Bridge.

At this juncture, supporters of isomorphism might argue that their critics are using the term "structurally" rather loosely. According to isomorphism, some model *x* is a representation of its target system *y* if and only if *x* shares the same **mathematical structure** as *y*. This clarification bodes well for isomorphism because the aforementioned criticisms of symmetry, transitivity, and reflexivity appear to no longer hold. Regarding symmetry, isomorphism now requires that the physical model and the actual bridge share the same mathematical structure (i.e., geometrical shape), which happens to be true. With respect to transitivity, it actually turns out to be the case that a drawing of the Golden Gate Bridge and a physical model of the said bridge share the same mathematical structure. Finally, it is ridiculous to suggest that a physical model of the Golden Gate Bridge does not have the same mathematical structure as itself because what other mathematical structure would it have other than its own?

The problem with all of these responses is that in the end they tell us very little, except that a drawing and a model of the Golden Gate Bridge exhibit the same geometric pattern as the real thing. But in 1956 and 1956 when the Army Corps of Engineers built a model of the San Francisco Bay in a warehouse to explore the possible repercussions of John Reber's plan to build two dams in the San Francisco Bay, their model needed to be more than just isomorphic to the Golden Gate Bridge and the rest of the San Francisco Bay—it also needed to be similar to the latter in other aspects as well. For instance, the Army Corps of Engineers built their model to a horizontal scale of 1:1000 and a vertical scale of 1:100.[3] The model was also scaled for slope and velocity (10:1), discharge rates (1:1,000,000), volume (1:100,000,000), and tidal cycle (1:100).[4] Nevertheless, as Mauricio Suarez makes clear, "isomorphism, which is well defined only as a relation between mathematical structures, does not apply to the relation between two physical objects".[5] So when we discuss the volume of the San Francisco Bay model in relation to the actual San Francisco Bay, what we are really doing is comparing a physical property of the two, which is not the same thing as comparing an abstract mathematical structure such as a geometric shape. Ultimately, the most that can be said about the San Francisco Bay model is that it is similar to the original, but in no way is it isomorphic to it.

In the end, the model built by the Army Corps of Engineers proved to be worth the time, effort, and money because it successfully predicted that Reber's plan would be a disaster if it was implemented.[6] The model demonstrated among other things that the dams would not only fail to create the freshwater lakes that it promised, but they would also bring in high-velocity currents into the Bay, rendering water traffic nearly impossible.[7] What Michael Weisberg's case study of the San Francisco Bay model shows is that similarity might be a more helpful way to think about representation than isomorphism because even though the Bay model was not isomorphic to the actual Bay,

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3. Michael Weisberg, *Simulation and Similarity: Using Models to Understand the World* (Oxford: Oxford University Press, 2013), 1, 7.

4. Weisberg, *Simulation and Similarity*, 8.

5. Mauricio Suarez, "Scientific Representation: Against Similarity and Isomorphism," *International Studies in the Philosophy of Science* 17, no. 3 (2003): 231.

6. Weisberg, *Simulation and Similarity*, 9.

7. Ibid.

it was similar enough to it that it was able to predict what would happen if Reber's plan did in fact come to fruition.

The leading proponent of the similarity view of representation is Ronald Giere who believes that the relationship between a model and the world is one of *fit*. Meaning, if a model is similar to its target in relevant ways then the fit between the model and the world is a good one and the model should be thought of as an accurate representation of the world. This is certainly what Paul Teller has in mind with his remark that a good model "succeeds in representing things as they are, in the way achieved by an accurate map, a true (enough) statement, and other sorts of accurate but not completely exact representations."[8] Although the Army Corps of Engineers model of the San Francisco Bay did not accurately represent all of the actual San Francisco Bay's properties, it was close enough to its likeness that it was able to predict what would happen were the actual Bay to be dammed up. To be disappointed with the model because it did not provide a completely accurate representation of the San Francisco Bay is tantamount to being upset with your smart phone map because it failed to inform you that the street you just turned right on is riddled with potholes. If models are constructed using the most accurate empirical and mathematical data and are similar to their targets in relevant ways, they should be thought of as fitting the world and accurately representing reality. To demand more of models is unreasonable.

The key word in the previous section is *relevant*. How do we decide which properties are relevant to a phenomenon and which are not? The intuitive answer is to appeal to the scientists themselves who employ empirical data to build models. For

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8. Paul Teller, "Fictions, Fictionalization, and Truth is Science," in *Fictions in Science: Philosophical Essays on Modeling and Idealization*, ed. Mauricio Suárez (New York: Routledge, 2009), 237.

example, the Army Corps of Engineers were the ones who decided that when building their model of the San Francisco Bay delta, velocity of water, discharge rates, volume, and tidal cycle had to be made to scale. In other words, they decided that these were the relevant properties that made their model a trustworthy representation of the real thing. In this particular instance there was no disagreement between empirical data and model, all of the data suggested that one kind of model be built with certain relevant properties. But such congruence between data and model cannot always be expected; sometimes different empirical data suggest contradictory models of a single phenomenon. As Margaret Morrison points out, this happens to be the case with the atomic nucleus.[9] Currently there are over 30 different models of the atomic nucleus, each of them based on different assumptions, with each providing a different kind of insight into nuclear structure.[10] Contrary to popular perception, we should not think of these models as succeeding one another in the sense that "inferior" models are continuously replaced by their "superior" counterparts. In fact, all of these models are currently being used by physicists for all sorts of purposes. The most explicit example that Morrison uses to demonstrate that the atomic nucleus is modeled in contradictory, yet empirically adequate, ways is her comparison of the liquid drop and shell models. The liquid drop model describes the nucleus as a classical (Newtonian) fluid consisting of protons and neutrons that randomly bump into one another.[11] The shell model, on the other hand, assumes that the atomic nucleus is a quantum body with protons and neutrons moving in

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[9] See Margaret Morrison, "One Phenomenon, Many Models: Inconsistency and Complementarity," *Studies in History and Philosophy of Science Part A* 42, no. 2 (2011): 342 – 351.

[10] Morrison, "One Phenomenon," 347.

[11] Ibid., 348.

well-defined orbits that hardly ever collide into one other![12] Nevertheless, despite their contradictory characteristics, both models are employed by the physics community. The liquid drop model successfully accounts for fission,[13] but it does not explain the "proton and/or neutron numbers at which the nucleus is particularly stable" (the so-called "magic numbers").[14] The advantage of the shell model is that it successfully explains the magic numbers,[15] but its account of asymmetric fission "is so a*d hoc* as to not really constitute an explanation at all."[16]

Let us now return to the issue of how to discern a model's relevant properties from its irrelevant properties. The suggestion that an appeal be made to experts using empirical data is untenable because we have seen that different experts use empirical data differently to construct contradictory models of the same phenomenon. So whereas some physicists might appeal to a liquid drop model of the atomic nucleus to account for fission, others might employ a shell model to explain its stability. But who is to say which feature is the most relevant? Is explaining fission more relevant than explaining nuclear stability or is it the other way around? One cannot take the easy way out and simply combine the models in order to highlight both fission and nuclear stability because the two models stand in explicit contradiction to one another. This complication

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[12] "Nuclear Models," *Oracle ThinkQuest*, http://library.thinkquest.org/3471/nuclear_models_body.html (accessed October 3, 2013).

[13] Morrison, "One phenomenon," 349 – 350.

[14] Daniela M. Bailer-Jones, *Scientific Models in Philosophy of Science* (Pittsburgh, PA: University of Pittsburgh Press, 2009), 191.

[15] Bailer-Jones, *Scientific Models*, 191.

[16] Morrison, "One Phenomenon," 350.

suggests another aspect of representation that takes us further and further away from a similarity account: choice.

The notion that choice plays a role in how a model represents its target poses a significant problem for proponents of the similarity view because it implies that similarity, by itself, is not enough to explain representation. Consider another example, the distance between an atom's nucleus and its electron(s). The most recent, accepted model of the atom in secondary physics textbooks usually depicts a tightly packed nucleus surrounded by an electron "cloud." The illustration of the electron as a cloud is intended to make the point that the location of the electron is never fixed and always probable. When teachers employ the electron cloud model they usually do so in order to steer students away from Niels Bohr's visually captivating solar system model of the atom where electrons are supposed to orbit the nucleus like planets orbiting the sun. In terms of describing the actual behavior of electrons, the electron cloud model is vastly superior. However, it certainly does not follow from this that the electron cloud model is an accurate description of what an atom is actually like because although it may correctly describe the behavior of electrons, it grossly distorts at least one important aspect of the atom: the atomic radius or the distance from the nucleus to the outermost edge of the electron cloud. When you look at illustrations of the electron cloud model in various textbooks, the boundary of the cloud is no more than a couple of centimeters away from the nucleus. This gives students the misleading impression that the atomic radius for any given atom is *not* exceptionally large, when in actuality the distance between the nucleus and the electron boundary is so expansive that no textbook illustration can do it justice without vulgar misrepresentation. A more empirically adequate analogy suggests that if we were to use a golf ball to represent the nucleus, we would have to place the outermost boundary of the cloud 1.5 miles away![17] So although it is the case that the electron cloud model is similar to a real atom when it comes to its description of electron behavior, it is vastly dissimilar to it as a representation of the atomic radius, atomic diameter, and the atomic nucleus.

My point, then, is two-fold: first, any model is going to leave out significant (maybe even essential) properties when attempting to represent its phenomenon; and second, these omissions are usually intentional. The first point undermines the similarity account of representation because any model that is both like *and unlike* its target in one or many relevant ways is ultimately not similar to its target. The second point regarding intentionality and choice also undermines the similarity account because it brings attention to the fact that it is the modeler who decides which properties are relevant and which are irrelevant for her specific purposes. A physicist interested in exploring fission will be more inclined to adopt the liquid drop model of the nucleus, whereas a physicist exploring nuclear stability will adopt the shell model. Both of these physicists are aware of the shortcomings of their particular models; nevertheless they employ them because the models further their understanding of the properties and processes that they are interested in exploring. The isolation of such properties, however, is tantamount to a concession that for modelers, the models come first and not the representation of a target system.[18] In the words of solid state physicist John Bolton, "It's not the real world. It's a

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[17] "Introduction to Atomic Structure," *SmartTutor*, http://lc.brooklyn.cuny.edu/smarttutor/corc1322/ASIntro.html (accessed October 3, 2013).

[18] Tarja Knuttila, "Representation, Idealization, and Fiction in Economics: From the Assumptions Issue to the Epistemology of Modeling," in *Fictions in Science: Philosophical Essays on Modeling and Idealization*, ed. Mauricio Suárez (New York: Routledge, 2009), 218.

toy world, but you hope it captures some central aspects of reality, qualitatively at least, how systems respond."[19]

Morrison's example of the atomic nucleus provides an example of how different data suggest different models of the same phenomenon. But what about cases where scientists have the same data, yet proceed to build different models? Do these kinds of examples also undermine the thesis of relevant similarity? I believe that they do. The thesis of underdetermination, first set forth by Pierre Duhem, argues that there can be an infinite number of alternative hypotheses for any given data set because there is no way that the data, by itself, can confirm one hypothesis against its alternatives. According to the underdetermination thesis, the successful fit between empirical data and a hypothesis does not amount to much because there are several competing hypotheses that can be said to fit the data. The only conclusion that can be inferred from a fit between a model and the data is that the model fits the data; no inference can be made that the model represents what the world is actually like. In a recent article on astrophysics and cosmology, Stephanie Ruphy argues that underdetermination is rampant in her field, two examples being the evolution of the universe and the shape of the Milky Way galaxy.[20] The problem with these models is not just that there are different models that fit the data, but it is also the case that the models themselves are built upon sub-models that are also underdetermined. Take for instance, the Millenium Run, an ambitious computer simulation of the evolution of the universe a few hundred years after the Big Bang to the

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[19] Bailer-Jones, *Scientific Models*, 179.

[20] Stephanie Ruphy, "Limits to Modeling: Balancing Ambition and Outcome in Astrophysics and Cosmology," *Simulation Gaming* 42, no. 2 (2008): 177.

present.[21] According to Ruphy, the Millenium Run is suggested by at least eight different sub-models: Friedmann-Lemaitre models, inflation, cold dark matter models, model of galactic morphological transformation, model of star formation, model of gas cooling, models for dust obscuration, and population synthesis models,[22] all of which are underdetermined. But it gets even more interesting. The Millenium model depends on semi-analytic galaxy formation modeling, which assumes the existence of dark matter, which is inferred from the existence of Big Bang inflation, which relies on the existence of Friedmann-Lemaitre universes.[23] However, at no point in this chain of model dependency do we get a model that has escaped the charbydis of underdetermination. The Friedmann-Lemaitre model assumes the untestable philosophical hypothesis that we are not privileged observers (otherwise known as the 'Copernican Principle').[24] The inflation model has the same explanatory power and empirical support as the topological defect model.[25] And according to modified forms of gravitational equations, dark matter need not exist.[26] Given that each subsequent model is dependent upon another underdetermined model, it is hard to take seriously the claim that the Millenium Run is a simulation of how the universe a*ctually* evolved. At most, it is a model that represents a mix bag of empirical data, mathematical models, philosophical assumptions, and human choice. Now I am in no way want downplaying the magnificence of the simulation and

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[21] 21. Ruphy, "Limits to Modeling," 179.

[22] 22. Ibid., 180.

[23] 23. Ibid., 183.

[24] 24. Ibid., 181.

[25] 25. Ibid., 182.

[26] 26. Ibid.

what it can teach us about our universe, but at the same time we should be weary of the claim that it represents the real thing.

The most common and obvious reply to the underdetermination argument is that its conclusion is trivial and uninteresting.[27] Although it might be true that there are an infinite number of different interpretations for any given data set, it does not follow that there are an infinite number of *good* interpretations. In fact, the number of good interpretations will be quite limited. Thus, some models might be better than others by virtue of their simplicity, while others might be better because they lead to more productive research programs. A good model, then, in addition to being similar to its target in relevant ways must also separate itself from other possible models by appealing to such features as: simplicity, fruitfulness, testability, etc.

The response to the trivial and uninteresting argument is that it is self-defeating because it undermines one of the key theses that those unfriendly to underdeterminism implicitly maintain: that relevant similarity provides both a necessary and sufficient condition for a source to represent its target. Because a good representational model depends on relevant similarity *in addition to* simplicity, fruitfulness, and/or testability, it follows that relevant similarity cannot, by itself, possibly establish both necessity and sufficiency. This leaves those sympathetic to the correspondence theory of representation with two options: either accept the underdetermination critique and continue to hold the view that relevant similarity is both necessary and sufficient for substantive representation, or give up on necessity and sufficiency altogether in order to deal with the

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[27] Samir Okasha, *Philosophy of Science: A Very Short Introduction* (Oxford: Oxford University Press, 2002), 73.

imbroglio of underdetermination. Because each of these options present some kind of Faustian bargain, it is difficult to imagine how correspondence theorists will proceed.

A second argument against underdetermination is that it is an impatient overreaction by anti-realist philosophers of science who would rather make hasty epistemological judgments concerning the impossibility of knowledge instead of allowing the process of scientific discovery to run its course. A classic example in the history of science is the geocentrism versus heliocentrism debate. It is well known that when called upon to reconcile geocentrism with the retrograde motion of the other planets against the background stars, Ptolemy argued that when the other planets orbited around the earth they would break from their orbits, travel in a circular motion, and then continue along their original paths. By introducing the concept of the epicycle Ptolemy was able to account for the same data (the retrograde motion of the planets) as Copernicus's competing heliocentrism theory. Because both theories appeal to the same evidence to defend their theories, both theories are underdetermined by the evidence. Anti-realists believe that underdetermination poses a grave epistemological problem in that it introduces an unavoidable skepticism regarding scientific theories. Realist philosophers, on the other hand, believe that this kind of underdetermination is innocuous. Even if geocentrism and heliocentrism are underdetermined by the data, it does not follow that both theories are equally persuasive. On the one hand, Ptolemy's epicycle solution to retrograde motion is clearly an ad hoc attempt to save geocentrism. On the other hand, Copernicus's heliocentric theory is supported by additional evidence such as predictive success,[28] consistency with Kepler's laws of planetary motion,

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28. John Worrall, "Underdetermination, Realism and Empirical Equivalence," *Synthese* 180, no. 2 (2011): 159.

Galileo's discovery of Venus's phases, and parsimony. Realists suggest that this additional evidence gives us good reasons to prefer heliocentrism over geocentrism despite their data equivalency. In the end, there really is no problem of underdetermination.

The heart of this argument is that a lot of scientific theories and models that are underdetermined in the short run are not underdetermined in the long run because of the availability of new empirical data made possible by new technology, such as Galileo's telescope. While this explanation may apply to geocentrism, it is certainly not ubiquitous for all theories and models. One example that immediately comes to mind is the Millenium Run model alluded to above. For the sake of argument, suppose that scientists discover evidence against the existence of dark matter. The following three scenarios then become plausible. First, scientists sympathetic to the Millenium Run simulation will simply assimilate the new data to fit their model. Regarding the Millenium Run, Ruphy maintains that "adjustments by fine tuning or by addition…can be obtained without altering previously chosen key ingredients of the model."[29] In other words, the initial inclination of scientists is to try to fit the incoming data to the model that they already have in place. Second, after futile attempts to fit the new data to the model, a Kuhnian revolution occurs where the Millenium Run model is overthrown for a different model that better fits the non-dark matter data. This model is thought to be the new best available model of the evolution of the universe. The final scenario that can be envisioned is that after the Millenium Run model is overthrown, the non-dark matter model of the universe gives birth to several different sub-models, which in turn suggest several different models of the evolution of the universe, all of which are equally

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[29] Ruphy, "Limits to Modeling," 190.

empirically plausible. So in contrast to the second scenario where only one model is produced that fits the data, this last scenario produces several competing models that can all equally accommodate the new data.

Those unfriendly to the thesis of underdetermination believe that the second scenario is probably the likeliest of the three. Because of its fit with the new evidence, the new model will become the paradigm of cosmology and astrophysics so long as it is able to accommodate all of the data (both old and new) better than other competing models. Once it is unable to do this, the model choice process starts all over again until a new model captures the imagination of the field. Unfortunately, this account of model adoption paints far too a rosy portrait of how models are adopted by scientists. Model choice is messy business, especially in cosmology and astrophysics. As Ruphy attests,

> Had the cosmologists chosen different options at some stages in the model-building process, they would have come up with a different picture of the evolution of cosmic matter. And the point is that those alternative pictures would be equally plausible in the sense that they would also be consistent both with the observations at hand and with our current theoretical knowledge.[30]

So there is more to model choice, at least in cosmology and astrophysics, than simply adopting the model that best fits the new data because multiple models will end up meeting this criterion even if some of the older ones no longer do. If it turns out that the dark matter model of the universe no longer provides the best fit with the data, we should expect several models to emerge instead of just one. These models will depend on several different sub-models, all of which will be equally empirically adequate. When the model adoption process reaches this point, which model a scientist chooses will depend less and less on empirical adequacy (because several models will meet this criterion) and more

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30. Ibid, 184.

and more on “subjective factors”[31] that cannot be adjudicated with empirical evidence alone such as simplicity or an individual scientist’s theory-ladenness. All of this suggests that scenario number three is actually the most plausible description of what happens when new empirical data is introduced to older models.

This brief look at the correspondence account of representation, in either its stronger isomorphic manifestation or weaker similarity version, shows that it is imbued with flaws. For now, we must look elsewhere for a viable account of representation, which takes us to the denotation account.

2.3 The Denotation Account

According to the denotation account of representation, a model should be thought of as a sign or symbol that refers to the target being represented. To be clear, denotation is not a concept originating in the philosophy of science. It is a borrowed concept from art (usually referring to paintings) to describe the relationship between a work of art and what it represents. For example, it is well known in the art world that Picasso’s *Guernica* symbolizes both the bombing of the town of Guernica and the rise of fascism in Europe.[32] The point is that whenever a learned viewer looks at Picasso’s masterpiece, her thoughts are immediately directed towards the bombings and Fascism. A model in science is supposed to refer in the same way. Anyone familiar with Galileo’s diagrams in *Discourses Concerning Two New Sciences* knows that he used vertical lines to refer to time intervals and horizontal lines to refer to an object’s speed.[33] Galileo’s intent was that

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[31] Thomas Kuhn, “Objectivity, Value Judgment, and Theory Choice,” in Martin Curd, J.A. Cover (eds.), *Philosophy of Science: The Central Issues* (New York: W.W. Norton & Company, 1998).

[32] Suarez, “Scientific Representation: Against Similarity and Isomorphism,” 236.

the mere sight of these lines would elicit thoughts of time and speed. So in both art and science, there are objects (paintings and models) that refer, or more particularly, symbolize something else (the rise of Fascism and time intervals). This form of representation is called denotation.

One thing is clear, for R.I.G. Hughes and Caroline Elgin denotation in science does not involve resemblance. Both are in agreement with philosopher of art Nelson Goodman, "that no degree of resemblance is sufficient to establish the requisite relationship of reference. Nor is resemblance *necessary* for reference…Denotation is the core of representation and is independent of resemblance."[34] Nevertheless, Goodman himself is not of the belief that denotation supplies a necessary condition for representation. To make this point, Goodman introduces the case of fictions. His most famous examples are unicorns and pictures of "Pickwick."[35] According to Goodman, paintings of unicorns and Pickwick do not denote or signify anything because unicorns and Pickwick do not exist, but it does not follow that they are not representations. They are representations, but not the kind we are accustomed to. Van Gogh's self portrait is a *representation of* Van Gogh. A portrait of Pickwick, on the other hand, is not a representation of Pickwick; on the contrary it is better understood as a Pickwick-representation. The difference between the two kinds of representation is that Van Gogh's self portrait picks out Van Gogh in the world such that were we to come across a picture of Van Gogh we could identify the man in the picture as Van Gogh. Pickwick-representations and unicorn-representations obviously do not pick out and select anything

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[33] R.I.G. Hughes, "Models and Representation," *Philosophy of Science* 64 (1997): S237.

[34] Nelson Goodman, *Languages of Art*, 5.

[35] Ibid., 26.

in the world, but this does not mean that they are not representations; they are just empty representations.

Axel Gelfert agrees with Goodman that there are special kinds of representation that do not involve denotation, but argues that when it comes specifically to scientific representation, denotation "may still turn out to be a central ingredient."[36] In particular, Gelfert strongly believes that R.I.G Hughes's DDI (denotation, demonstration, and interpretation) account makes a convincing case for denotation by eschewing Goodman's worry that anything can stand as a symbol for anything else. This is possible according to the DDI account because denotation, by itself, does not carry the entire burden for representation; instead representation in science is understood to be a tri-partite schema that equally involves denotation, demonstration, and interpretation. Only when all three of these components are satisfied can there be any kind of meaningful representation. In Gelfert's words, "demonstration and interpretation may provide the DDI account with the internal resources to keep 'in check' whatever element or arbitrariness is introduced by denotation."[37]

To understand how demonstration and interpretation are supposed to keep denotation 'in check' we can look at Hughes's primary example in his article, figure 49 from Galileo's *Dialogues Concerning Two New Sciences*. For Hughes, the geometrical diagram is a clear example of denotation because it is a sign that signifies something else. As a whole the figure is intended to show that the distance traveled by a uniformly accelerating object starting at rest and an object already moving at uniform speed will be

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[36] Axel Gelfert, "Mathematical Formalisms in Scientific Practice: From Denotation to Model-Based Representation," *Studies in History and Philosophy of Science Part A* 42, no. 2 (2011): 278.

[37] Gelfert, "Mathematical Formalisms in Scientific Practice," 276.

the same so long as the final accelerated speed of the former is twice the final uniform speed of the latter.[38] At this point, the critic of denotation will argue that all she sees are horizontal and vertical lines, some parallelograms and some triangles. In other words, denotation by itself gives us very little. In order for it to be a meaningful *scientific representation* we need to be convinced of two things: first, that the proof demonstrated by the diagram is true and second, that this is actually how objects in the real world behave. Hughes believes that the first worry can be allayed by the second component of his DDI account, demonstration. Galileo demonstrates that the distance travelled by an object starting at rest will be the same as an object already travelling at a uniform speed by transforming the kinematics problem into a geometry problem.[39] In Figure 47, the area of parallelogram AGFB represents the total distance traversed by an object already travelling at a uniform speed, while triangle AEB represents the total distance traversed by an object starting at rest. Simple geometry allows us to infer two conclusions: (1) the distance travelled by both objects are equal, and (2) the final speed of the object that starts from rest (BE) is double that of the object already travelling at a uniform speed (BF). If we accept Galileo's geometrical re-presentation of the problem along with the basic axioms of geometry and the propositions that follow from them, then we must similarly accept the truth of the proofs generated from such diagrams.

The final component of the DDI triad is interpretation. It should be noted, however, that Hughes deviates from the term's ordinary usage. Whereas the usual understanding of the term means to make some proposition comprehensible, Hughes

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[38] Hughes, "Models and Representation," S236.

[39] Ibid., S237.

refers to interpretation as that which is supposed to yield the predictions of a theory.[40] And we can see from Galileo's diagrams that they do just that. However much we increase or decrease the amount of time it takes for an object to traverse a given space (represented by AB), its final speed will always be double that of an object already in uniform motion.

Galileo's diagrams support Gelfert's observation that Hughes's DDI account provides a much stronger version of representation than denotation alone. Demonstration and interpretation (or prediction) provides proponents of denotation a way to combat Goodman's notorious charge that anything can represent anything else. When non-experts encounter Galileo's diagrams for the first time they simply have to take his and other experts' word for it that certain lines and geometrical figures represent what they are supposed to. This certainly does not bode well for denotation. But once we add Hughes's additional requirements that models should also be able to justify their theories and make novel predictions, we can begin to understand the appeal of the DDI account.

Regardless, the DDI account is not without its shortcomings as well. First, let us begin with the notion of interpretation, better understood as prediction. Hughes states that, "Only after interpretation can we see whether theoretical conclusions correspond to the phenomena, and hence whether the theory is empirically adequate."[41] Now I agree with Hughes that we should have more confidence in our models when they yield accurate empirical predictions. However, this does not mean that we have accurately connected the model to reality. Hughes himself concedes as much when he refers to

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[40] Ibid., S333.

[41] Ibid.

Galileo's diagrams as "representations of representations."[42] The latter description is apt given that Galileo's geometrical diagrams are replete with idealizations such as frictionless motion that have no correspondence in the real world. But as science shows us time and time again, this does not prevent us from making accurate empirical predictions. For example, real life pendulums: have a bob with mass, have a suspension wire with mass, must take into account the friction between the bob and the suspension wire, the suspension wire and the support, and most significantly, air resistance. Theoretical pendulums, on the other hand, are point masses and frictionless. Nevertheless, emendations can be made to the acceleration of gravity equation so that it takes into account such things as bob mass and air resistance thus bringing the theoretical pendulum more and more in line with its real world counterpart. Returning to Galileo's diagrams, emendations can similarly be made to them so that they more accurately reflect the behavior of phenomenological objects and not idealized ones. But does any of this get us any closer to the idea that models accurately represent the real world? I am in agreement with Nancy Cartwright that it does not because whatever corrections are made to idealized diagrams and equations to make them more empirically accurate are made "from the ground up, so-to-speak, and not from the top down…What we do instead is to add a phenomenological correction factor, a factor that helps produce a correct description, but that is not dictated by fundamental law."[43] Because these alterations are driven by the world and how it actually behaves and not the models themselves, they cannot be said to represent items in the actual world, which is what they were supposed to represent all along.

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[42] Ibid., S329.

[43] Nancy Cartwright, *How the Laws of Physics Lie* (Oxford: Clarendon Press, 1986), 111.

All of this returns us to Goodman's argument regarding things that do not exist in the real world (i.e. fictions). Recall that Goodman's stance on fictions is that they can be represented, but not denoted, rendering them empty representation. If we take Goodman seriously, then we must treat scientific fictions as we would literary and artistic fictions; namely as representations devoid of denotation. But this undermines Gelfert's insistence that scientific representation is best understood through Hughes's DDI account because scant attention would be paid to scientific fictions such as idealizations and approximations which have no correspondence in the real world, yet scientific practice would be impossible without. Hughes himself is silent about whether or not scientific fictions can denote, but he clearly does not have any kind of ontological issues regarding the idea that some models can serve as models for other models. Quoting Hughes, "the DDI account readily accommodates a hierarchy of this kind. For the model used at an early stage of theorizing (model$_1$) can itself be the subject of another model at a deeper level (model$_2$)."[44] If my interpretation of Hughes is correct, model$_2$ is "at a deeper level" because it produces more empirically accurate predictions than model$_1$, which is just another way of saying that model$_2$ better corresponds to the world than model$_1$. Whereas Hughes and Gelfert believe that the introduction of such correction factors brings these models closer to reality, I maintain that the very incorporation of these factors support the opposite view that models represent entities that do not really exist. In other words, models are about fictions.

At the most basic level, denotation involves symbols symbolizing a target. Among Goodman's many contributions to our understanding of denotation include the idea that denotation only involve entities that exist. Fictional entities such as unicorns and

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[44] Hughes, "Models and Representation," S335.

Pickwick can be unicorn-representations and Pickwick- representations, but they cannot be representations of unicorns and representations of Pickwick. Using an example from the history of science, Maxwell's ether model is best understood as an ether-representation and not a representation of ether because the ether does not exist. The fact that this statement is true and cannot be disputed is beside the point, however. I maintain that a lot of the confusion surrounding modeling arises when denotation moves to the center of representation and *mutatis mutandis* the center of modeling. If we are adamant about following Goodman's emphasis on denotation, then Maxwell's ether model is not technically a model, neither are any of Galileo's diagrams, nor is the equation for the simple pendulum one either. But this seems counterintuitive. If all of the latter are non-models, then what exactly are they? The truth of the matter is that they are models and it is only Goodman's theory of denotation that is causing confusion. What often gets overlooked in Goodman's analysis of denotation as an alternative to the correspondence theory is that it shares one glaring similarity with its rival: both assume that models necessarily involve targets, the difference between the two accounts being that one believes that representation is predicated on correspondence, while the other does not. Even though denotation has moved past correspondence, it has not been able to circumvent its reliance on target systems. The 'models as mediators' account presented in the next section takes us in this very direction.

2.4 Models as Mediators Account

A third approach to modeling that has become more and more popular in the past fifteen years is the models as mediators view put forward by Mary S. Morgan and Margaret Morrison. According Morgan and Morrison, models neither correspond to their targets, nor do they denote them. A better to way to understand the relationship between models and their targets is through the concept of mediation. The two authors make it explicitly clear that they do not have in mind a hierarchy where theory is at the top, the empirical world is at the bottom, and models are located somewhere in between the two. Nor do they have in mind a horizontal relationship with theory and the empirical world are situated on opposite sides of a spectrum with models positioned somewhere in the middle. Instead, "Because models typically include other elements, and model building proceeds in part independently of theory and data, we construe models as being outside the theory-world axis."[45] This brief remark contains two of the key elements in the models as mediators view: (1) autonomy and (2) combination. Although these two elements appear contradictory, they are not. Morgan and Morrison believe that models are autonomous in the two-fold sense that they are not completely deduced from theory and not completely dependent on empirical evidence. For example, despite the fact that we know that the atomic nucleus is a quantum system, scientists still use the classical (i.e. non-quantum) liquid drop model to explain fission. If scientists were only concerned with deducing models from theories, then the liquid drop model would not be considered a potential model because of its blatant contradiction with quantum mechanics. The fact that the liquid drop model is still viable today supports the idea that model construction is not completely determined by theories. At the same time, models are not fully dependent on empirical data either. When Watson and Crick were building their DNA models, they notoriously disregarded Rosalind Franklin's x-ray evidence that suggested that DNA did

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[45] Mary S. Morgan and Margaret Morrison, "Models as Mediating Instruments," in *Models as Mediators: Perspectives on Natural and Social Science*, ed. Mary S. Morgan and Margaret Morrison, (Cambridge: Cambridge University Press, 1999), 17-18.

not have a helical structure.[46] Had they been seduced by the x-rays they might have abandoned the helix thesis altogether. Instead, Watson famously retorted to Franklin's face that "she was incompetent in interpreting X-ray pictures"[47] and that it would benefit her to "learn some theory".[48] Watson and Crick's success in discovering the structure of DNA demonstrates that scientists do not strictly rely on empirical data to guide the construction of models. Together both of these examples reinforce Morgan and Morrison's position that models are both theory-autonomous and empirical data-autonomous entities.

The remark that "models typically include other elements" means that models are not constructed on the basis of a single factor such as theory alone or empirical evidence alone, etc. Rather, the mediator view argues that model-building depends on a combination of several elements in addition to theory and empirical evidence, including but not limited to: mathematical formalism, metaphor, analogy, story, scientist's preference, aesthetic parameters, etc. According to economist Marcel Boumans, "Model building is like baking a cake without a recipe. The ingredients are theoretical ideas, policy views, mathematisations of the cycle, metaphors, and empirical facts."[49] A great example of scientists combining these various and often disparate elements to form a model is the Watson and Crick model of DNA. As already discussed above, that particular construction process showed the scientists involved commit to a helical theory of DNA despite the availability of non-helical theories, as well as X-ray data

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[46] Samuel Schindler, "Model, Theory, and Evidence," *British Journal of Philosophy of Science 59*, no. 4 (2008): 627.

[47] James D. Watson, *The Double Helix* (New York: Simon & Schuster, 1996), 166.

[48] Ibid.

[49] Marcel Boumans, "Built-in Justification," in Morgan and Morrison, *Models as Mediators*, 67.

problematizing their preferred theory. Put differently, Watson and Crick's approach to model-making was overtly theory-laden. But it does not follow that they were wholly averse to empirical evidence. Although they did not take too much stock in the X-rays of the A form of DNA, they had the completely opposite reaction to the X-rays of the B form. According to Watson, "With the A form, the argument for a helix was never straightforward...With the B form, however, mere inspection of its X-ray picture gave several of the vital helical parameters."[50] So it was not the case that Watson and Crick were dependent on theory alone. They relied on empirical evidence, but were selective as to what kinds of evidence they would allow to affect their model. In addition to theory and empirical evidence, the famous double helix model was aided by another key ingredient, luck. Watson's discovery that adenine bonded naturally with thymine and guanine with cytosine was not derived from any theory; in fact Watson's "like with like prejudices" was an obstacle that had to be overcome. Watson's personal account of the discovery in *The Double Helix* does not suggest that the pairing was based on any kind of empirical evidence either. In the end, he was able to make the discovery by simply "shifting the bases in and out of various other pairing possibilities"[51] until all the hydrogen bonds appeared to form naturally. In a February 2005 Ted talk Watson reaffirmed the fortuitous nature of the base pairing by explaining to the audience in attendance that "We got the answer on the 28th of February 53 and it was because of a rule which is to me a very good rule, never be the brightest person in a room."[52] Watson

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[50] Watson, *The Double Helix*, 169.

[51] Ibid., 194.

[52] James Watson, "How We Discovered DNA," (TED Talk, TED conference, Monterey, CA, February 2005), http://www.ted.com/talks/james_watson_on_how_he_discovered_dna?language=en#t-621954 (accessed September 29, 2014).

was referring to the fact that he was not professionally trained as a chemist, which led him to the rather unorthodox method of changing the base pairings around until they fit together in a way that was not coerced. Because of its non-scientific connotations, many scientists, philosophers, and educators downplay the role that luck plays in the formation of scientific theories and/or the construction of models by banishing it to the context of discovery abyss. It is clear to me that the DNA example forces us to reconsider this notion.

Another important dimension of Morgan and Morrison's models as mediators view is their insistence that models are instruments of theory production. In particular, they believe that models help scientists further develop their theories. The DNA example discussed throughout this chapter is a prime example of how scientists use models to do this. Watson and Crick had made up their minds early on that the structure of DNA had to be helical. This led the two to their model-building method whereby any empirical evidence was either assimilated or neglected depending on whether or not it supported or rejected their helical theory. The more traditional route to theory and model formation is to gather all of the available data, make sure that the latter does not contradict each other, and then propose a theory and/or build a model based on the consistent empirical evidence. As described above, this was Rosalind Franklin's approach to science; and from Watson and Crick's perspective it was also what prevented her from immediately embracing the helical structure. The two discoverers of the DNA, inspired and empowered by the example of Linus Pauling and the discovery of the alpha helix through model building, believed that "careful model building could embody constraints that the final answer had in any case to satisfy. Sometimes this could lead to the correct structure, using only a minimum of the direct experimental evidence."[53] Put differently, model construction requires the adoption of several rules (or constraints) that limit, and therefore guide, the kinds of models that can be built. Watson and Crick took advantage of these constraints and used them to discover the structure of DNA.

At the same time, Morgan and Morrison are also adamant that models are not purely instruments; they are also partial representations of the world and of theories. The Army Corps of Engineers model of the Golden Gate Bridge and surrounding bay-delta area is a wonderful example of a model attempting to accurately represent the world. The model possessed several relevant characteristics that allowed it to be compared to the actual bay-delta area:

- The model was built to horizontal and vertical scale.[54]
- It was filled with salt water just like the Pacific Ocean.[55]
- It was built with a pumping system to simulate the tidal cycle.[56]
- The model included freshwater pipes to simulate river and steam flows into the Bay.[57]
- The velocity of the water was built to scale.[58]
- The volume of water was built to scale.[59]

But even the most faithful models provide only partial representations. The bay-delta model was built out of concrete slabs, screws, and butumious joint material[60]; the actual bay-delta area formed naturally out of dirt, rock, and sand. The model used a pumping

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[53] Francis Crick, *What Mad Pursuit* (New York: Basic Books, Inc., 1988), 60.

[54] Weisberg, *Simulation and Similarity*, 7.

[55] Ibid., 8.

[56] Ibid., 7.

[57] Ibid., 8.

[58] Ibid.

[59] Ibid.

[60] Ibid.

system to simulate the tidal cycle, while the actual tidal cycle is caused by the gravitational force of the moon. The model used freshwater pipes to bring in freshwater; the actual bay-delta uses streams and rivers. Despite these differences, the bay-delta model was relevant enough that it demonstrated what would have happened had Reber's plan to dam up the bay come to fruition. In this sense it fulfilled its dual function as both instrument and representation.

Just as models are used to represent the world, they are also used to represent theories. Recall once again the liquid drop model of the atomic nucleus. Because the model treats the atomic nucleus as a classical rather than a quantum phenomenon, it cannot be a true representation of what the nucleus is actually like. So if the liquid drop model is not a representation of the world, then what exactly is it a representation of? Morrison argues that it is a representation of strong force theory, the view that the atomic nuclei are bound together by the residual strong force that is a minor residuum of the strong force that binds quarks together to form protons and neutrons.[61] Yet, the static liquid drop model is at best only a partial representation due to the existence of other strong force models, such as Bohr's compound nucleus model, that emphasize the theory's more dynamic features.[62]

In layman terms, a mediator is someone who brings two or more parties together with the hope of reaching a compromise. This usually means that no one side will entirely get their way and everyone will end up sacrificing something important in the process. According to Morgan and Morrison, models should be thought of in the same manner—

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[61] Morrison, "One Phenomenon," 347.

[62] Roger H. Steuwer, lecture notes titled "An Act of Creation: The Meitner-Frisch Interpretation of Nuclear Fission," (University of Minnesota), http://quantum-history.mpiwg-berlin.mpg.de/news/workshops/hq3/hq3_talks/17_stuewer.pdf (accessed August 31, 2014).

as some kind of compromise between our theories and the world. This is a bold interpretation of modeling that refuses to allow models to be strictly deduced from theories or solely inferred from empirical evidence alone. According to this view, models will always be a compromise between theories, data, metaphors, and mathematical formalism. Similarly, models should never be thought of as only instruments or just representations. The models as mediators approach acknowledges the representational power of such instruments, as well as the instrumental power of such representations.

2.5 Conclusion

Recall that I began this chapter by trying to understand what makes a model, a model. Because most of the discussion surrounding models involved the seemingly innocuous claim that models are supposed to represent their targets, this inevitably led me to a thorough investigation of representation. Three popular accounts were subsequently discussed: correspondence, denotation, and models as mediators. The strong version of the correspondence account (isomorphism) was argued to be too strong, requiring all models to share the same mathematical structure as their targets. Though correspondence theory's weaker version, similarity, provided us with an improved understanding of models, it yielded difficult problems of its own, including the bothersome problem of underdetermination. All of these issues with the correspondence theory led me to consider an alternative way to explore the nature of models, that of denotation. What makes denotation appealing is that it avoids the notion of resemblance altogether, making it possible for a model to represent a target without necessarily sharing in its likeness. Denotation, however, does not embrace the notion of fictions as models because even though the latter involve a special kind of representation, they do not denote (recall

Goodman's examples of unicorns and Pickwick). At the same time, scientific fictions have played an integral role in the production of scientific knowledge, especially in highly theoretical fields like quantum mechanics where physicists are able to make highly accurate predictions about the behavior of subatomic particles despite knowing very little about their nature. This suggests that the benefit of studying scientific models lies in what we can learn from them instead of becoming enmeshed in the quagmire of representation.

The strength of the models as mediators approach is that it is supported by the way science is actually practiced. This is clearly demonstrated in the numerous case studies in Morgan and Morrison's book, as well as the DNA example in this particular chapter. Whereas the syntactic and semantic approaches to modeling both maintain that there is an inextricable link between models and theories, the mediator view significantly weakens the dependency of the former on the latter without severing the tie completely. Although this is the foundation of what Morgan and Morrison refer to as the autonomy of models, it is more accurate to characterize the relationship between the two as partial autonomy given the fact that theories are usually involved in the process of model construction to some degree.

Morgan and Morrison are adamant that their work does not include a "'theory' of models"[63] that provides "well-defined criteria for identifying something as a model and differentiating models from theories."[64] The next chapter of this project will take up the challenge of formulating a theory of models, but will eventually reach the same conclusion as Morgan and Morrison regarding the difficulty of distinguishing models from theories. Because theories are involved in the construction models and models are

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[63] 63. Morrison and Morgan, "Models as Mediating Instruments," 12.

[64] 64. Ibid.

involved in the construction of theories, it is all but impossible to identify where one begins and the other ends, hence the problem of differentiation. Whereas Morgan and Morrison believe that models mediate between theories and the world, I contend that the purpose of a model is not so much mediation as it is solving scientific problems. At any rate, there is one idea that both of our accounts can agree on: scientific modeling must move on to a post-representation phase.

Chapter 3

The Integration Account of Modeling

3.1 Towards an Integration Account

3.1.1 The Integration Account of Modeling – Version 1.0

If the correspondence, denotation, and models as mediators accounts all provide an inadequate account of models and modeling, then we must begin where we left off and ask, “What is a model?” As a point of departure I would like to begin with the account of modeling put forth by Marcel Boumans in his article “Built-in Justification.”[1] Boumans argues for an integration account of modeling whereby a model is simply understood to be the product of several elements interacting with one another. The importance of this interaction is that each of the elements involved places a restriction on what the end product (i.e. the model) eventually looks like. When combined, Boumans believes that these various restrictions produce a model with “built-in justification.”[2] In Boumans’s own words, “Model building is like baking a cake without a recipe. The ingredients are theoretical ideas, policy views, mathematisations of the cycle, metaphors and empirical facts.”[3] To continue the baking metaphor, just as different cake ingredients are mixed together in unique ways to make different kinds of cake; similarly, theories, data, and metaphors can all be integrated in various ways to create unique models. Bouman’s point regarding “built-in justification” is that for each model the theories involved restrict how the data can be interpreted, the data restrict what theories can be proposed, and the

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[1] Marcel Boumans, “Built-in Justification,” in *Models as Mediators: Perspectives on Natural and Social Science*, ed. Mary S. Morgan and Margaret Morrison, (Cambridge: Cambridge University Press, 1999), 66.

[2] Boumans, “Built-in Justification,” 66.

[3] Ibid., 67.

metaphors used restrict how the data can be interpreted by the theory. One of Bouman's main points is that there is no set recipe for building a model because the ingredients involved will dictate how the model comes together in the first place. For example, Boumans points out that one of the key ingredients in some economic models is current economic policy. The idea that some models might be influenced by policy should remind us of Reber's plan to build two dams in the San Francisco Bay area. If it was not for Reber's plan and the growing support that the plan seemed to be receiving, the Army Corps of Engineers would never have built their model of the San Francisco Bay in the first place. So in this particular instance, a model was largely influenced by policy. But if we consider the DNA model, I am unfamiliar with any government policy influencing Watson and Crick's construction of the double helix. In fact, Watson has made it abundantly clear that one of his primary motivations (i.e. model ingredients) was making the discovery before anyone else so as to win the Nobel Prize. In one model a certain ingredient might be essential, while in another it might not even exist. This, however, does nothing to their ontological status as models. Both are still models, just made up of different ingredients, integrated in their own unique way.

For the most part, I agree with Bouman's integrated account of modeling. My only concern with the view is that Bouman's list of possible model components is far too limited. If by "ingredients" Bouman is referring to the myriad of ideas, concepts, processes, and physical objects that are involved in constructing a model, then there is certainly far more that goes into model construction than just: theoretical ideas, policy views, mathematical formulations, metaphors and empirical facts. In addition to the components already on Bouman's list we need to add: laws of nature, experiments, instruments, research programs, tacit knowledge (including metaphysical assumptions), funding, interpretation, the historical development of a field and the theories within it, as well as any fortuitous, non-rational contingencies that may have contributed to the construction of a model (i.e. luck). In the end, even this list is not comprehensive and exhaustive enough, which is precisely why model construction is such a capricious and frustrating exercise.

The integration account is shared by Gerald Holton, who remarks,

> When you ask, “What is light?” the answer is: the observer, his various pieces and types of equipment, his experiments, his theories and models of interpretation, *and* whatever it may be that fills an otherwise empty room when the lightbulb is allowed to keep on burning. All this, together, is light.[4]

I agree with Holton that the textbook definition of light as “electromagnetic waves in the visible spectrum”[5] is simplistic to the point that “too much is left out.”[6] Light is a contradictory phenomenon whose interpretation depends in large part on the theoretical perspective of the scientist who is studying it. Maxwell believed light to be a wave phenomenon because he could not envision how else a light source could arrive at its target, unless it was brought there by some kind of physical phenomenon (i.e. the ether). On the other hand, the later Einstein, more the realist than the empiricist,[7] believed that

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[4] Gerald Holton, *Thematic Origins of Scientific Thought: Kepler to Einstein*, Rev.ed. (Cambridge, MA: Harvard University Press, 1988), 104.

[5] *Collins Dictionary of Science* (Glasgow: HarperCollins Publishers, 2003), 337.

[6] Holton, *Thematic Origins*, 104.

[7] Holton, *Thematic Origins*, 237 – 279. For a different perspective on Einstein’s philosophy of science, see Don A. Howard, "Einstein's Philosophy of Science," in *The Stanford Encyclopedia of Philosophy*, ed. Edward N. Zalta (Fall 2017 Edition), forthcoming, accessed September 2, 2017, https://plato.stanford.edu/archives/fall2017/entries/einstein-philscience/. Howard disagrees with Holton that Einstein’s philosophy of science can be divided into two stages: the early positivist years and the later realist years. Instead, Howard argues that Einstein’s philosophy of science is an eclectic one, deriving from many philosophical influences.

light could be explained without resorting to physical waves, so he proposed the particle theory of light whereby light was thought to consist of 'packets' of energy called photons. In Einstein's view, electromagnetic waves were not a property of the ether; rather they were a property of space-time itself.[8] With this radical interpretation of the nature of reality, Einstein undercut the empiricist need for "bodies acting directly on one another"[9] because electromagnetic waves were "no longer a manifestation of a process taking place in an underlying medium."[10] Holton refers to such grand theoretical assumptions as *themata*. *Themata* are the

> fundamental presuppositions, notions, terms, methodological judgments and decisions…which are themselves neither directly evolved from, nor resolvable into, objective observation on the one hand, or logical, mathematical, and other formal analytical ratiocination on the other hand.[11]

In other words, *themata* are neither synthetic nor analytic (to use Kant's terms), meaning they cannot be justified by either experience or logical deduction. Nevertheless, these *themata* "are commonly regarded as essential features of reality within an epistemic community, which form the basis of intelligibility in any account of reality."[12] Any attempt to describe a scientific phenomenon outside the context of its *themata* will inevitably be incomplete because these *thematic* principles are fundamental to any

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8. Nancy J. Nersessian, "Aether/Or: The Creation of Scientific Concepts," *Studies in History and Philosophy of Science Part A* 15, no. 3 (1984): 201.

9. Arthur I. Miller, *Insights of Genius: Imagery and Creativity in Science and Art* (Cambridge, MA: The MIT Press, 2000), 20.

10. Nersessian, "Aether/Or," 207.

11. Holton, *Thematic Origins*, 41.

12. Hasok Chang, Inventing Temperature: Measurement and Scientific Progress (Oxford: Oxford University Press, 2004), 91. Chang uses the term *ontological principles* instead of *themata*, but both refer to the non-logical, non-experiential assumptions underlying a scientist's work.

scientific phenomenon. Thus I am in complete agreement with Holton that any robust conception of what a model is must include the *themata* that serves as its groundwork.

This leads me to my first iteration of what a model is, the integrated account – version 1.0:

> *The integrated account – version 1.0: A model is a collection of elements (e.g., theoretical ideas, policy views, mathematical formulations, metaphors, empirical facts, instruments, the historical development of a field, serendipity, and themata, etc.) all interacting with one another.*

3.1.2 The Integration Account of Modeling – Version 2.0

So far, I understand a model to be the end result of several elements interacting with one another. It would be impossible to create an exhaustive list of these elements here, but some of the more notable ones mentioned above include: instruments, experiments, theories, historical and social contexts of a discovery, tacit knowledge, luck, and *themata*. And yet, I have a nagging suspicion that in its present form this definition of models is only partially adequate. What is missing is some sense of purpose. It is true that models are created when a myriad of distinctive elements come together, but for what purpose? I believe that Bailer-Jones is on the right track when she states that the purpose of a model is to provide their creators and users with some kind of epistemic access.[13] However, I disagree with her when she further specifies that the epistemic access models give us is access to some kind of phenomenon.[14] First, as I argued earlier in this study, not all models are created with a phenomenon (or target) in mind. Second, and more importantly, the notion of epistemic access has serious realist overtones that I vehemently

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[13] 13. Daniela M. Bailer-Jones, "Models, Metaphors, and Analogies", in *The Blackwell Guide to the Philosophy of Science*, ed. Peter Machamer and Michael Silberstien (Malden, MA: Blackwell Publishers Ltd, 2002), p. 108

[14] 14. Bailer-Jones, "Models, Metaphors, and Analogies,",p. 108.

disagree with. If the access in question is not to some kind of phenomenon, then what do models provide access to? The answer is: answers to scientific problems. When I think of models as granting human beings epistemic access, I do not think of them as somehow penetrating phenomena and giving us a glimpse into Platonic (metaphysical) or Aristotelian (physical) essences. As anti-climactic as it may sound, when it comes down to it, models help scientists solve problems and therein lies their benefit.

> *The integration account – version 2.0: A model is a collection of integrated elements, interacting with one another, whose purpose is to help scientists solve different kinds of scientific problems.*

At this juncture, it should be noted that my conception of problem-solving is slightly different than Thomas Kuhn's notion of "puzzle-solving" as articulated in *The Structure of Scientific Revolutions.* When Kuhn articulates that successful scientists prove themselves to be expert puzzle-solvers, he is specifically referring to problems *only* found in normal science and not extraordinary science or the science of scientific revolutions.[15] For Kuhn, puzzle-solving involves "Bringing a normal research problem to a conclusion…in a new way,"[16] and not aiming "to produce major novelties, conceptual or phenomenal."[17] The notion of problem-solving I am using in this project applies to problems in both normal and revolutionary science. My primary concern here is the application of models to solve *any* kind of scientific problem, traditional or novel.

3.1.3 The Integration Account of Models - Version 3.0

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[15] Thomas Kuhn, *The Structure of Scientific Revolutions* , 3rd ed. (Chicago: The University of Chicago Press, 1996), 36.

[16] Kuhn, *Structure of Scientific Revolutions*, 36.

[17] Ibid., 35.

Another important element of models is their temporary nature. This is to be expected if we accept the problem solving function of models in the previous section. Quoting Thomas Nickels, "Heuristics is not single, fixed doctrine; rather, it consists in growing and changing families of (often content-specific) search procedures which can be adapted to the special problem at hand."[18] Once models help scientists solve their problems they can be discarded. Similarly, when scientists realize that the model that they have adopted is not moving their research program forward, they can discard that particular model and move on to one with more epistemic potential. Such is the life of models—they are fleeting entities whose existence depends on what problems need to be solved. In science, problems are changing all of the time; it should come as no surprise then that the models accompanying them are just as transient.

This idea is wonderfully explicated by Dudley Shapere who recognizes that,

> The methods we employ lead us to new beliefs, which in turn lead us to modify those very methods, sometimes replacing them with new ones. The result has been a growing integration of method with belief. A cycle of mutual adjustment of beliefs and methods has thus become a characteristic of the scientific enterprise...[19]

What Shapere is discussing here is a hermeneutic circle of method and belief. First, our methods help produce new beliefs within us. Next, these newly acquired beliefs suggest the development of different methods to help us make sense of these newly acquired beliefs, which in turn produce even more new beliefs, etc., etc. This is also how model production happens to work. A scientific problem requires the production of certain models, which then suggest a host of novel problems to be solved, which again require a

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[18] Thomas Nickels, "'Twixt Method and Madness," in *The Process of Science*, ed. Nancy J. Nersessian (Dordrecht, The Netherlands: Martinus Nijhoff Publishers, 1987), p. 47.

[19] Dudley Shapere, "Method in the Philosophy of Science and Epistemology: How to Inquire About Inquiry and Knowledge", in Nersessian, *The Process of Science*, p. 5.

new set of wholly distinctive models, etc. etc. The main point is that model production is not merely the logical conclusion of a one-way deduction that begins with a problem and ends at an inevitable model. As Majorie Grene so eloquently recognized, "The sciences are too many-faceted to subject themselves to a program of monolithic unification…[T]he orderliness of parts of it [the natural world] not only may be studied in different ways and/or at different levels, but *exist* in different ways and/or at different levels."[20] Grene's idea that nature exists and can be studied at a plurality of levels indicates that our models of it will inevitably be both variegated and ephemeral. This brings me to my third iteration of what a model is.

> *The integrated account – version 3.0: A model is a temporary collection of integrated elements, interacting with one another, whose purpose is to help scientists solve different kinds of scientific problems.*

3.2 A Deflationist Attitude

The integration account takes a deflationary attitude towards models and modeling.[21] According to Suarez, a deflationist strategy "entails abandoning the aim of a substantive theory to seek universal necessary and sufficient conditions that are met in each and every concrete real instance of scientific representation." Simply put, a deflationist attitude has forsaken the hope of ever establishing any kind of necessary and sufficient conditions for what it is to be a model. Because models are far too variegated and heterogeneous types of entities, it is utterly impossible to isolate a few of their properties and argue that they constitute a model's essence. A look at the DNA, Bay Area, and simple pendulum models certainly attest to this. The DNA and Bay Area

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[20] Majorie Grene, "Historical Realism and Contextual Objectivity: A Developing Perspective in the Philosophy of Science," in Ibid., p. 77.

[21] Mauricio Suarez, "An Inferential Conception of Scientific Representation," *Philosophy of Science* 71, no. 5 (2004): 770.

models are similar in that they are both physical models, yet they have different purposes. The DNA model explains the structure of DNA while the Bay Area model was used to predict what would happen if dams were built in the Bay Area per Rebus's plan. On the other end of the spectrum we have the model of the simple pendulum, which is neither a physical object nor an abstract idea; instead it is a mathematical equation. As such, its purpose is less explanation and more prediction. This brief sketch alone has eliminated four possible necessary and sufficient components. Not all models have to be physical objects because the model of the simple pendulum is not a physical object. At the same time, not all models have to be mathematical equations because even though the construction of both the DNA and Bay Area models required complex mathematical equations, their ultimate manifestations are physical, tangible objects rather than an abstract equation. Two other candidates for necessity and sufficiency are explanation and prediction, but these are both inadequate as well. The DNA model explains but does not predict, while the simple pendulum models predict, but do not explain. Yet, all of the physical structures and mathematical equations discussed above are universally recognized in the literature as models.

What we need, then, is an approach to definitions that is not married to the idea of necessary and sufficient conditions. One philosopher who maintains such a viewpoint is Ludwig Wittgenstein. Wittgenstein argues that we eschew talk of necessary and sufficient conditions altogether in favor of a "family resemblance" approach. According to the latter, concepts such as game and family all exhibit a series of overlapping and crisscrossing similarities with no one feature making us use the same word for all.[22]

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[22] Ludwig Wittgenstein, *Philosophical Investigations*, trans. G.E.M. Anscombe (Oxford: Basil Blackwell Ltd, 1958), 31 – 32e.

Applied to the concept model, this means that there is no single feature or combination of features that helps us characterize a model as a model; instead, it is all but inevitable that some models (m_1) will be similar to other models (m_2) with respect to some characteristic x, but different from other models (m_3) with respect to characteristic y. For Wittgenstein, even though m_1, m_2, and m_3 fail to have a single or a cluster of essential characteristics in common, they are all nevertheless appropriately called models.

3.3 Criticisms of the Integration Account of Modeling

There are three primary objections to the integration account of modeling. First, it is guilty of the fallacy of composition; second, the account does not help us distinguish models from non-models; and third, it commits the naturalistic fallacy.

3.3.1 Parts/Wholes

The most obvious objection to the integration account is that it commits the fallacy of composition. According to this fallacy, a whole is much more than simply the sum of its parts. For example, if we ask someone what a car is and she answers that a car is made up of: an engine, spark plugs, a timing belt, an air filter, muffler, tires, doors, steering wheel, seats, etc., we would argue that she has not adequately answered the question. Although it is true that a car is composed of all of these parts, it does not follow that this is all there is to being a car. Even the most exhaustive list of components will not tell you anything about the purpose of a car, which seems to be inimical when considering what a car is. Similarly, the integration account may tell us all about the different components that go into constructing a model, but this does not mean it tells us what a model is. According to Morrison and Morgan, models are important because they "are a source of both explanatory and predictive power,"[23] while Suarez maintains that their significance has more to do with their ability to help agents make inferences about the target being modeled.[24] If the integration account of modeling does not tell us anything about a model's ability to explain, predict, or draw inferences, then its applicability is severely limited and thus, ultimately flawed.

3.3.2 Models vs. Non-models

The integration account of modeling provides us with a poor account of modeling because it does not allow us to distinguish models from other scientific concepts like theories and laws. For instance, if we take the definition of model in section 4.1.3 and substitute the word 'theory' for 'model,' we have a working definition of 'theory': *A theory is a temporary collection of integrated "items" whose purpose is to contribute to human knowledge by helping scientists solve different kinds of scientific problems, while at the same time also making them aware of untenable research programs.* A good definition provides both necessary and sufficient conditions for a concept. The integration account of modeling supplies us with the former, but not the latter and that is why the terms 'model' and 'theory' become interchangeable.

3.3.3 Naturalistic Fallacy

A third criticism of the integration account is that it is guilty of the naturalistic fallacy. According to one version of the fallacy, the difficult task of evaluating the validity of *x* is replaced by an explanation of how *x* comes to be. For instance, naturalized epistemology is concerned with all of the psychological processes that produce our

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[23] 23. Morrison and Morgan, "Introduction," in Morgan and Morrison, *Models as Mediators*, 6.

[24] 24. Suarez, "An Inferential Conception of Scientific Representation," 773.

beliefs; it is not at all concerned with justification. Quoting W.V.Quine, "The stimulation of his sensory receptors is all the evidence anybody has had to go on, ultimately, in arriving at his picture of the world. Why not just see how this construction really proceeds? Why not settle for psychology?"[25] Some philosophers, however, doubt if naturalist epistemology is even epistemology at all. Traditionally understood, epistemology has been the study of knowledge and its conditions. It is concerned primarily with two questions: (1) What is knowledge?, and (2) What is the criteria for knowledge?. According to Jaegwon Kim, if philosophers follow Quine's lead and abandon these questions altogether, then not only will epistemology go out of business, but knowledge itself also will because knowledge and justification are "inseparably tied."[26] Traditional epistemologists agree that naturalized epistemology is an informative discipline with far reaching consequences regarding how we come to our beliefs, but because it does not concern itself with normative and evaluative concerns, it is not, and will never be, epistemology per se.

The more traditional way of framing this criticism is to argue that the integration account falls under the aegis of the context of discovery and not the context of justification. Those who oppose a naturalized epistemology believe there is a strict dividing line between how a theory is discovered and its justification. Because the integration account only concerns itself with the psychological, social, and historical processes that lead to a discovery of a theory or a production of a model, it is a purely descriptive enterprise. The context of discovery, however, has nothing to do with the

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[25] W.V. Quine, "Epistemology Naturalized," *Knowledge: Readings in Contemporary Epistemology*, ed. Sven Bernecker and Fred Dretske (Oxford: Oxford University Press, 2000), 269 – 270.

[26] Jaegwon Kim, "What is 'Naturalized Epistemology?'," in Bernecker and Dretske, *Knowledge*, 286.

context of justification. Those sympathetic with the context of justification believe that it is one thing to describe the various contexts within which a scientific theory or model arises, wholly another to argue that the theory or model itself is acceptable. According to Harvey Siegel, “What is crucial for epistemology is not the actual train of thought which culminates in an epistemologically potent pronouncement; rather, epistemology is concerned with evaluating, with establishing the potency of, that pronouncement.”[27] Put simply, discovery is not in the justification business.

Just as Kim accused Quine of naturalizing epistemology and in the process giving up on the most important task of epistemology (i.e. justification), it might similarly be argued that in settling for a naturalized account of models I am giving up on one of the most significant issues in scientific modeling and that is developing a criteria for models. Those critical of naturalizing epistemology argue that it is one thing to list all of the different components that go into constructing various models and wholly another to list all of the conditions that distinguish models from non-models and good models from bad ones. Naturalized epistemology might be fruitful for burgeoning fields such as the cognitive science of science, but it is not very helpful for the field of scientific modeling that approaches scientific models from a strictly philosophical point of view.

3.4 Response to Criticisms

As formidable as these critiques are, each one can be met with a response. What is particularly interesting about the following replies is that they all share the same pattern; namely, that all of the so-called objections are not even objections at all.

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[27] Harvey Siegel, “Justification, Discovery, and the Naturalizing of Epistemology,” *Philosophy of Science* 47, vol. 2 (1980): 299.

3.4.1 Parts/Wholes

The composition fallacy is based on the unfounded assumption that there must be more to the whole than the sum of its parts. But this need not be the case. Reductionist theories in the philosophy of mind argue that the mind is just the brain and that the brain is just a complex organization of synapses and firing neurons. The burden of proof is on the anti-reductionist to demonstrate that there is more to the mind than just the brain. So far this has been an exercise in futility for the anti-reductionist who nevertheless believes that there must be something more to the mind than just synapses connecting and neurons firing. Wishful thinking, however, is not an argument. Similarly, those opposed to the integration account have to demonstrate that there is more to a model than its various parts (e.g. theories, laws, background assumptions, etc.) without resorting to the notion that there just has to be something more. If they believe that scientific models must have an essence, they must not only articulate the essence of such models, but also demonstrate how such an essence is possible.

3.4.2 Models vs. Non-models

As I noted in section 3.2, the integration account is largely influenced by the philosophy of the later Witgenstein who stayed away from the project of delineation (i.e. definition) altogether because he thought it was an exercise in futility. For Wittgenstein, there are two ways you can try to explain to someone what a game is who does not already know what it is. The first is to provide a definition of game and label all of the activities that conform to that particular definition as a game. For example, if someone defines a game as any competitive activity, this means that the boundary separating games from non-games is that of competition. But a problem immediately arises with this definition and all subsequent definitions: they ultimately lack necessity and sufficiency. To claim that competition is a necessary and sufficient condition for games means that not only are all games competitive, but also that everything that is competitive is a game. It is clear that both of these statements are false. There are some games that are non-competitive, for example a child who throws a ball up in the air and tries to catch it is not trying to win at anything. It is also the case that not every competitive activity is a game because standardized test taking, although highly competitive, is usually not thought of as participating in a game. In the words of Wittgenstein, "we can draw a boundary—for a special purpose. Does it take this to make the concept usable? Not at all!"[28] This quote's message is two-fold. First, it reminds us that any attempt to define what a game (or model) is will inevitably be a human, and thus a social, construction. This point is important because it raises doubts about any concept that claims to possess intrinsic qualities. And second, Wittgenstein suggests that if the world has gotten along just fine using the word 'game' without necessarily adopting a universal definition of it, then definitions might not be as significant as their proponents take them to be.

At this juncture the anti-naturalist might make a concession. She will concede that definitions are human constructions that we create for our edification, but argue that this by itself is not enough to deter us from the human project of constructing definitions. Contrary to Wittgenstein, some propose that we should draw a boundary around the word game and argue that the child throwing the ball in the air is not playing a game, while students who take standardized tests are embroiled in a very high stakes game. In other words, just because there are different definitions of a concept in circulation, it does not follow that all definitions are created equal—some definitions are *better* than others. In

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[28] Wittgenstein, *Philosophical Investigations*, 33e.

fact, some historians of science would argue that concept development in science is a cumulative enterprise where our more primitive understanding of a concept is continually replaced by more refined conceptualizations that arise out of new experimental data and/or the application of rigorous mathematical analysis. Take, for example, the development of what a field is. Faraday is usually acknowledged as the foremost pioneer when it comes to our modern day understanding of what a field is. He believed that a field was made up of "physical lines of magnetic force"[29] that transmitted magnetic and electric forces.[30] Maxwell is believed to have improved upon Faraday's conception by formulating a mathematical representation of Faraday's lines of force.[31] Ironically, Maxwell developed his mathematical equations by employing various physical analogies. First, he envisioned Faraday's lines as tubes filled with an incompressible fluid.[32] Next, he envisioned this fluid to be made up of geometrically arranged "vortices" or elastic cells that rotated with angular velocities representing the intensity of the electromagnetic field.[33] To prevent neighboring cells from hitting one another, Maxwell imagined "electrical particles"[34] to function as idle wheels between them.[35] These "idle wheels" represented the electric current.[36] As fantastic as these analogies sound, he was able to

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[29] Michael Faraday as quoted in Nersessian, "Aether/Or," 185.

[30] Nersessian, "Aether/Or," 185.

[31] Ibid., 193.

[32] Ibid., 187.

[33] Daniela M. Bailer-Jones, *Scientific Models in Philosophy of Science* (Pittsburgh, PA: University of Pittsburgh Press, 2009), 25.

[34] Nersessian, "Aether/Or," 191.

[35] Bailer-Jones, *Scientific Models in Philosophy of Science*, 25.

[36] Ibid.

use them to mathematize Faraday's physical lines of force in a manner consistent with previously known electromagnetic data.[37] After Maxwell, the next major contributor to our modern day conception of a field was the Dutch physicist, Hendrik Lorentz. Lorentz agreed with Maxwell that the electromagnetic field was a state of the ether, but contra Maxwell he did not believe that the ether was a mechanical substance.[38] Another way of saying this is that Maxwell believed that his theory of electromagnetism was consistent with classical (i.e. Newtonian) mechanics while Lorentz clearly saw that it was not.[39] This leads us to Einstein who is often credited with our most current understanding of what a field is. Contrary to both Maxwell and Lorentz, Einstein viewed the very idea of the ether to be "superfluous."[40] According to Einstein, the electromagnetic field was not a state of some material substance called the ether; rather it was its own material substance replete with the hallmarks of matter: energy and momentum.[41]

The point of this example is that although Faraday, Maxwell, Lorentz, and Einstein's conception of field contain criss-crossing and overlapping similarities, it does not follow that all of their conceptions are equal. In fact, in the end, the only correct formulation is Einstein's. So even though we speak of Faraday, Maxwell, and Lorentz as contributing to our modern conception of what a field is, technically speaking none of them discuss fields per se, only Einstein does. This implies that the Wittgensteinian philosopher of science is mistaken when he argues for multiple formulations of a

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[37] Ibid.

[38] Nersessian, "Aether/Or," 198.

[39] Ibid., 197.

[40] Ibid., 201.

[41] Ibid., 207.

scientific concept such as a field or model. Even though numerous iterations of a scientific concept are developed throughout the history of science, empirical evidence, consistency with theories and laws, along with rigorous mathematical analysis will usually single out a superior conception in the long run. Anti-naturalists can breathe a sigh of relief because philosophy of science's normative project has been vindicated; the task of evaluation does not have to give way to description.

But this foregoing analysis is only appropriate if we are absolutely certain that Einstein's conception of a field is correct once and for all. Unfortunately, modern science does not bear this out. In fact, the most up-to-date science seems to have brought us full-circle, back to talk about the ether. A telling quote by Einstein himself suggests that even he never quite made up his mind about the ether:

> According to the general theory of relativity, space without ether is unthinkable; for in such space there not only would be no propagation of light, but also no possibility of existence for standards of space and time, nor therefore any space-time intervals in the physical sense.[42]

According to Nobel Prize winner Frank Wilczek, the "stuff" that constitutes physical reality:

- Fills space and time.[43]
- Each segment of space and time is made up of this "stuff."[44]
- Is made up of unpredictable quantum activity.[45]
- Renders the cosmos a multilayered, multicolored superconductor.[46]
- Contains a metric field that gives space time rigidity and causes gravity.[47]

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[42] Albert Einstein, as quoted in, Frank Wilcznek, *The Lightness of Being: Mass, Ether, and the Unification of Forces* (New York: Basic Books, 2010), 97.

[43] Wilcnek, "The Lightness of Being," 111.

[44] Ibid.

[45] Ibid.

[46] Ibid.

- Has a universal density.[48]

Throughout the history of science, this primary ingredient has had several appellations, some of the most recent being space-time and quantum field. However, one of its earliest designations (which is making a comeback by the way) is simply referred to as the ether.

If a highly technical concept like ‘field’ is still resisting definition, then most of our concepts (including ‘model’) cannot be quantified into a single, self-sustaining definition. The best we can do is follow Wittgenstein’s advice and provide examples, pointing out similarities and differences along the way. This is the strategy adopted by the integrated account of modeling. Rather than providing a formula for differentiating models from non-models or good models from bad ones, the integrated account is content with listing all of the elements that go into the construction of a model and discussing how those elements come together to produce the model in question.

3.4.2.1 The Modeling Spheres Model (MSM)

Related to the criticism that the integrated account does not help scientists and philosophers distinguish models from non-models is the condemnation that its nebulous nature makes it of very little use to science educators, who are the ones burdened with navigating children through the very distinctions I refrain from making. Experts in science education believe that as children progress through the nature of science (NOS) curriculum they need to be able to distinguish not only theories from laws, but also models from other non-models (e.g., theories) as well. Unfortunately, the integrated account either keeps the conversation idle or moves it in reverse. A truly student-friendly

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[47] Ibid.

[48] Ibid.

account of modeling will not only reflect pertinent issues in the philosophy of science literature, but will also help students differentiate between what is and is not a scientific model.

Now I have made my position clear: I refuse to draw a clear demarcation line between models and theories because I believe that no such demarcation line exists. However, for NOS curriculum purposes, it would be helpful for science students and their teachers to be exposed to the kinds of models that are employed by professional scientists and also discussed by members of the philosophy of science community. For this very reason I have created the Modeling Spheres Model or MSM for short (Figure 3.1). The MSM consists of three spheres of models, each sphere being an extension of its predecessor. Sphere 1 models are what scientists, philosophers of science, science educators, and science students normally think of as models. They are: visual, explanatory, used to make prediction, based on theories, physical, can be run, imitate phenomenon, and have a purpose. The example par excellence of such a model is Watson and Crick's double helix model of DNA. Sphere 2 models share some, but not all, of their characteristics with their sphere 1 counterparts. For instance, sphere 2 models are purposeful, based on theories, and run, but unlike sphere 1 models, the models in sphere 2 are mentally run as opposed to physically run. Sphere 2 models also tend to be non-physical abstractions that idealize nature using *ceteris paribus* conditions, rather than describing how nature actually works. One example that immediately comes to mind of a sphere 2 model is Maxwell's model of the ether. By the time we arrive at sphere 3 models, the term 'model' has lost most of its ordinary, colloquial (i.e. sphere 1) connotation. Like sphere 1 and 2 models, sphere 3 models retain the purpose of solving scientific problems. Unfortunately, the similarities end there. Sphere 3 models are mathematical models that demonstrate mathematical relationships between different phenomena (e.g., force, mass, and acceleration). These models are neither physically nor mentally run; they are calculated. Some examples include Kepler's Laws of Planetary Motion, as well as Maxwell's Laws of Electromagnetism.

It should be reiterated that the MSM is not intended to be interpreted as a model hierarchy ranging from simple models to more and more complex ones, though I recognize that such an interpretation is all but inevitable. Sphere 1 models are the models that specialists and laypersons alike think about when they conceive of the term 'model'. Sphere 3 models are recognized primarily by initiated scientists as models, whereas the layperson would have a difficult time wrapping her mind around why models in this sphere are even called models in the first place. At the same time, sphere 3 models are *not* any more real than either sphere 1 or sphere 2 models. All of the models, in all of the spheres, are all equally models. The differences in their perception are more the result of historical circumstances regarding various elements in science, history of science, and philosophy of science, ranging from empirical success to the ubiquity of mathematical formalism across the sciences.

THE MODELING SPHERES MODEL (MSM)

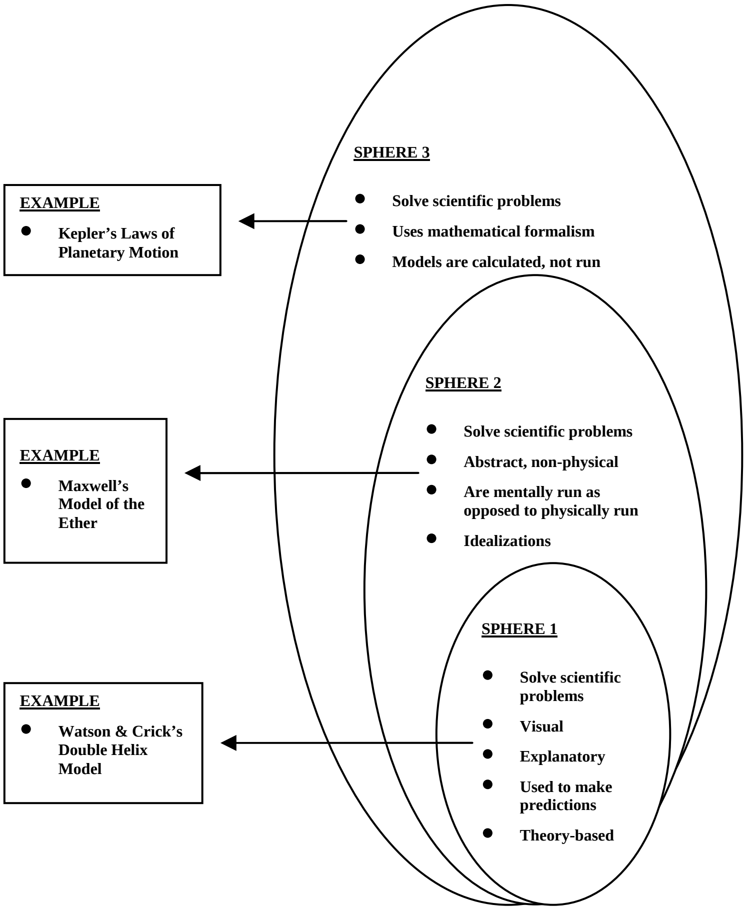

*Figure 3.1*Modeling Spheres Model (MSM)

3.4.3 Naturalistic Fallacy

According to the criticism in 3.3.3, the integration account is also wanting in that it fails to demarcate good models from bad ones. A good definition of model allows us to point to a particular model and say 'That is a good model' and explain what makes it good. My reaction to this criticism is one of vexation because the integration account has an unambiguous response to what makes a model a good model—it solves scientific problems. The double helix model of DNA is a good model because it solves the problem of the structure of DNA. Maxwell's ether model of was a good model because it helped him solve the problem of expressing Faraday's line of force in purely mathematical terms. Similarly, the Army Corps of Engineers' model of Reber's plan was a good model because it solved the problem of what would happen if Reber's plan to dam up San Francisco bay actually went into effect. Good models help scientists solve problems, bad ones do not.

Perhaps it is not that simple. Ptolemy's model and Copernicus's model both solved the problem of Mars's retrograde motion in radical ways. One was content with adding epicycle after epicycle until his model matched what was occurring in the sky, while the other displaced the Earth as the center of the universe and replaced it with the Sun. Surprisingly, both of the models make similar predictions, but only Copernicus's model represents reality. This example demonstrates that when choosing between models, we cannot simply select the one that solves a particular problem because it is quite possible that there are several models that can fulfill this function. Additional criteria that philosophers of science use to differentiate good models from bad ones include: explanatory power, predictive power, fruitfulness, parsimony, and relationship to the truth.

The idea that several, inconsistent models can represent a single phenomenon is unproblematic to the integration account. Physicists do not seem as worried as philosophers are that "there is currently no coherent account of the atomic nucleus."[49] That is because physicists are primarily concerned with solving problems and will use any model of the atomic nucleus that suits their purposes, even if that means appealing to contradictory models. According to Morrison, "from different perspectives phenomena

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49. Margaret Morrison, "One Phenomenon, Many Models: Inconsistency and Complementarity." *Studies in History and Philosophy of Science Part A* 42, no. 2 (2011): 343.

will have different characteristics; hence, we needn't assume there is only one correct model for a physical system. We can use quantum models in some contexts and classical models in others, even if we're dealing with the same phenomena."[50]Feyerabend agrees. In his view, knowledge is "an ever increasing *ocean of mutually incompatible alternatives*, each single theory, each fairy-tale, each myth that is part of the collection forcing the others into greater articulation and all of them contributing, via this process of competition, to the development of our consciousness. Nothing is ever settled, no view can ever be omitted from a comprehensive account."[51] Inconsistency, rather than being a detriment, is necessary for knowledge production.

This criticism of the integration account arises because there are still many philosophers of science and scientists out there who are still clinging to the teleological assumption made by scientism and positivism: that the history of science is a story of unrelenting progress whereby our understanding of scientific concepts is constantly improving with each subsequent era, compelling human beings closer and closer to a single, absolute truth or something very similar to it. While I fully acknowledge that our scientific knowledge has grown immensely since the Renaissance, it does not necessarily follow that scientists are inching us closer and closer to what the world is actually like, which is just another way of acknowledging that scientific knowledge does not necessarily entail the truth. Newton's laws of motion and Einstein's special theory of relativity have both been empirically successful in terms of the predictions they have been able to produce. Because of this success it is impossible to deny that both have expanded our knowledge of the universe more than we could have possibly imagined, yet

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[50] Ibid.

[51] Paul Feyerabend, *Against Method*, 4th ed. (London: Verso, 2010), 14.

neither Newton's laws nor Einstein's special theory of relativity can be used to accurately describe the behavior of subatomic particles, the foundation of all matter. Through Newton and Einstein our knowledge has been expanded, but it does not follow that either has presented us with a mirror of nature.

Indeed, Kuhn says as much towards the end of *Structure of Scientific Revolutions* when he makes the analogy between evolution and science.[52] Kuhn argues that change in scientific theories is similar to the idea of mutations in evolution in the sense that both are *non-teleological*. That is, both do not progress towards a specific end; rather, both just change and this change can be interpreted as either good or bad, but inherently it is neither. Now philosophers of science sympathetic to scientism might want to argue that a species' or idea's survival demonstrates its superiority to its predecessors, but this again is just interpretation. Survival does not carry with it some kind of special normative and evaluative force. The fact that a species, theory, or model has survived tells us nothing more than that the latter has survived; from this we cannot make any further inferences about the way the world actually is or ought to be. As tempting as it is to collapse epistemology into ontology, we must keep the two enterprises separate.

3.4.3.1 Feyerabend's Normalized Naturalism

Feyerabend believes that not only is description the most we can do, but it is also what we *should* do. In other words, in an ironic twist, he is making a prescription out of description! In *Against Method* he makes the audacious claim that dividing epistemology "by an order that contains discovery on one side and justification the other would have

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[52] Kuhn, *Structure of Scientific Revolutions*, 170 – 173.

ruined science".[53] It would have ruined it because some scientists would never have gone forward with their theories and models because they knew that neither was prepared for justification. Consider the case of Copernicus. Copernicus did not publish *On the Revolutions* until the end of his life, and even then it was done reluctantly only after Rheticus's prompting. According to Dava Sobel, "By his own admission, he did not feel so confident of his own work as to care nothing for others' opinions of it."[54] Arthur Koestler makes the even stronger claim that, "The whole evidence indicates that it was not martyrdom he feared but ridicule—because he was torn by doubt regarding his system, and knew that he could neither prove it to the ignorant, nor defend it against criticism by the experts."[55] In the end, "the Copernican system, bristling with inconsistencies, anomalies, and arbitrary constructions, was equally unsatisfactory, most of all to himself."[56] Recall that Ptolemy's system had been making accurate predictions for well over a thousand years before Copernicus developed his bold hypothesis. And even after the latter's work was published, sailors and astronomers alike abandoned his planetary tables and returned to earlier ones.[57] Had Copernicus become overly preoccupied with justifying every aspect of his heliocentric theory to the Ptolemaic tradition, there is no telling when *On the Revolutions* would have been published, assuming it would have even been published at all.

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[53] 53. Feyerabend, *Against Method*, 14.

[54] 54. Dava Sobel, *A More Perfect Heaven* (New York: Walker Publishing Company, 2011), p. 63.

[55] 55. Arthur Koestler, *The Sleepwalkers: A History of Man's Changing Vision of the Universe* (Middlesex, England: Penguin Books, 1964), p. 121.

[56] 56. Koestler, *Sleepwalkers*, 126.

[57] 57. Ibid.

3.4.3.2 The Melting Point of Lead: A Unique Case Study

On the one hand, it makes perfect sense to separate the context of discovery from the context of justification. The context of discovery has to do with the circumstances surrounding the discovery of a theory while justification involves whether or not a theory is a good one. Let us apply this distinction to an example made popular by Ernest Nagel and further discussed by James Bogen and James Woodward: the melting point of lead. According to the famous distinction, the story of the discovery of the melting point of lead is wholly separate from the justification of it. Let us begin with the discovery aspect of the melting point. Bogen and Woodward point out that the discovery of the melting point of lead involves not one data point, 327 degrees C, but the mean of the scatter of individual data points.[58] The reason why a mean temperature must be used instead of referring to a single temperature measurement is that the melting point of lead will vary depending on: the lead's purity, how well the thermometer is working, how the thermometer is being read, and interactions between the initial temperature of the thermometer and the lead.[59]

Now let us move on to the justification of the melting point of lead. One would think that one could justify the melting point of lead by simply measuring the temperature at which lead begins to melt. If it starts melting at 327 degrees C then the temperature is an accurate one. If it does not begin to melt at this temperature then a new temperature must be sought. But the problem is that it is entirely possible that any of the measurements taken will not exactly coincide with 327 degrees C because the number

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[58] James Bogen and James Woodward, "Saving the Phenomena," *The Philosophical Review* 97, no. 3 (1988): 308.

[59] Bogen and Woodward, "Saving the Phenomenon," 309.

just mentioned is the result of an inference and estimation of several data points.[60] In other words, there is no way to justify the melting point of lead because no such number exists; it is an ideal much like Galileo's frictionless plane. However, if we return to the context of discovery and understand how 327 degrees C came to become the de facto melting point of lead, then we seem to have further justification for the temperature. So in this particular instance, not only is the context of justification indistinguishable from the context of discovery; on the contrary, it is entirely dependent on it.

3.5 Integration Account vs. Other Accounts of Modeling

In the previous chapter, I discussed three of the most popular accounts of modeling: (1) the correspondence account, (2) the denotation account, and (3) the models as mediators account. This section will compare each of these accounts with the integration account of modeling, providing reasons why the latter should be adopted.

3.5.1 Correspondence Account

According to the correspondence account of modeling, x is a model of y just in case x is relevantly similar to y. At the end of the day the position is based on the realist assumption that there is only one model that accurately represents the world and it is the one that is most relevantly similar to it (the world). Recall that the problem with this account is that the notion of "relevantly similar" is notoriously vague. A physicist interested in the liquid drop model of the atomic nucleus believes that fission is more relevant than the so-called magic numbers, while physicists more interested in the shell model believe that the magic numbers are more relevant. To avoid the vagueness criticism, the correspondence account must come up with a way to discern the most

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[60] Ibid., 308 – 309.

relevantly similar model between different models that employ equivalent empirical evidence, which means they need to address the problem of underdetermination.

The advantage that the integration account has over the correspondence account is that it does not have to deal with the problem of underdetermination. The integration account avoids the problem of underdetermination by avoiding representation altogether. The problem of underdetermination only arises if we assume that there is a single way that the world can be accurately represented. Because the integration account has separated the tasks of epistemology and ontology, it views empirically equivalent models as expanding our scope of knowledge even though they might be logically contradictory to one another in an ontological sense. It should be obvious by now that the integration account is consistent with more pragmatic and instrumentalist accounts of knowledge. According to such views, models should be thought of as knowledge producing products and not necessarily as accurate representations of the world.

3.5.2 Denotation Account

According to the denotation account of representation, *x* is a model of *y* just in case *x* is a sign or symbol that refers to a target *y*. The liquid drop model of the atom can be considered a model precisely because it is a symbol that refers to a specific target, in this case the atom. Recall that the advantage of the denotation account over the correspondence account is that it does not require that models be relevantly similar to their targets. As a result, the denotation account, like the integration account, is willing to consider the existence of contradictory models of the atom at one and the same time. The problem with the denotation account, specifically Goodman's articulation, is the stipulation that the target that any model refers to must actually exist. So Maxwell's ether model cannot technically be a representation of the ether (and thus a model of the ether) because the ether does not to exist. In chapter two I argued that the existence requirement was unnecessarily severe. If we take it seriously, then fictional entities, like the ether, cannot be modeled, which seems counterintuitive.

The integration account, unlike the denotation account, does not discriminate between entities known to exist and those that do not. According to the integration account the primary goal of a model is knowledge proliferation. This means that as long as a model contributes to our knowledge of the world, it has every right to be called a model even if its reference does not exist. Maxwell's mechanical model of the ether is a perfect example. Maxwell was able to use his "extraordinary assemblage of tiny spinning cells interspersed with even smaller 'idle wheel' particles"[61] to not only mathematically represent Faraday's lines of force, but also discover that light is an electromagnetic phenomenon—not bad for a model whose reference is not believed to exist.

The distinguishing feature common to the correspondence and denotation accounts, but excluded from the integration account, is the idea that models are intended to be representations of targets. This particular approach takes it for granted that the targets being represented chronologically, ontologically, and epistemologically precede the models that are constructed to represent them. The integration approach denies this priority. Instead, it maintains that when models are created through the integration of: theories, laws, background assumptions, paradigms, research programs, experiments, etc., a distinct ontological entity is ushered into existence that has a separate identity from anything else that already exists in the world. The integration account can downplay the

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[61] Basil Mahon, *The Man Who Changed Everything: The Life of James Clerk Maxwell* (West Sussex, England: John Wiley & Sons Ltd., 2003), 99.

representation of predetermined targets because it believes that targets are created during the integration process, which is just another way of saying that models oftentimes become their own targets.

3.5.3 Models as Mediators Account

According to the Models as Mediators Account of Morrison and Morgan, a model acts as a mediator between a model and a theory. The Models as Mediators Account and the Integration Account share the following characteristics in common:

- Both deny that the sole purpose of modeling is representation.
- Both deny that the sole purpose of modeling is instrumentation.
- Both deny that models are solely derived from theories.
- Both believe in the possibility of contradictory models for the same phenomenon.
- Both agree that models are comprised of elements besides theories, laws, and empirical data.

One difference between the two accounts is that the integration account emphasizes the idea of problem solving. Whatever a model is, a good one extends our knowledge by solving a problem. In the words of William James, “Any-idea that will carry us prosperously from any part of our experience to any other part, linking-things satisfactorily, working securely, simplifying, saving labour, is true for just so much.”[62] A model connects our various empirical, theoretical, metaphysical, psychological, sociological, and historical experiences together in a reliable manner that allows problems to be solved and our knowledge of the world to be extended. While Morgan and Morrison agree with the epistemic potential of models, they say very little about its role

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[62] Williams James, as quoted in, Nicholas Rescher, *Pragmatism*, (New Brunswick, NJ: Transaction Publishers, 2012), 4.

in solving scientific problems so as to distance themselves from a purely instrumentalist view of modeling.

The integration account focuses on the problem solving function of scientific models because it takes the idea of a naturalized epistemology seriously. Recall that naturalized epistemology has all but abandoned the idea of representation in favor of solving problems in the world. The integration account reflects this. Abnegating the problem solving function of scientific modeling will move scientific modeling back towards representation and all of the ontological and epistemological baggage that goes along with it (e.g., the quest for absolute truth, the idea that the latter can only be attained through logic and rationality, the distinction between the context of discovery and the context of justification, etc.)

3.6 Conclusion

According to version 3.0 of the integration of account of modeling:

> *A model is a temporary collection of integrated elements, interacting with one another, whose purpose is to help scientists solve different kinds of scientific problems.*

This account is both deflationary and naturalistic. It is deflationary in the sense that it does not provide necessary and sufficient conditions for what a model is and ought to be. It also takes a naturalistic approach, meaning it takes as its starting point how actual scientists practice science.

Nevertheless, there are some who remain skeptical regarding the practical value of scientific models. One such outspoken critic was Pierre Duhem who believed that the only conceivable purpose for creating a scientific model was pedagogical (i.e. they were intended for the edification of non-scientists). It is clear that the integration account presented here wholeheartedly disagrees with this position. The following chapter will carefully examine Duhem’s argument and provide an uncompromising critique of it so as to leave no question of the value of scientific models.

Chapter 4

Pierre Duhem's Critique of Modeling

4.1 Introduction

The instrumentalist approach to modeling is the view that models are primarily heuristic devices that expedite solutions to scientific problems. Instrumentalists also maintain that models play a significant role in the production and development of scientific theories. One of the most famous critics of scientific modeling (specifically, the instrumentalist approach) was Pierre Duhem. The latter was so distressed by the privileged role that models commandeered that he devoted chapter four of his famous *The Aim and Structure of Physical Theory* entirely to the topic. Before launching into Duhem's critique and the reasons for his vehement disdain, some background of Duhem's philosophy of science is in order.

4.2 The French and the English Mind

In the aforementioned chapter, Duhem contrasts two kinds of minds: the French mind and the English mind. The epitome of the French mind can be seen in the work of Descartes. According to Duhem, Descartes's *Discourse on Method* demonstrates the "strong but narrow mind"[1] at its best with its emphasis on abstraction, deduction, and logic. The English mind is supposed to be the opposite of the French mind; rather than being strong and narrow, it is conceived of as "weak and broad."[2] Although Duhem briefly uses Sir Francis Bacon's *Novum Organum* to demonstrate the characteristics of

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1. Pierre Duhem, *The Aim and Structure of Physical Theory*, trans. Philip P. Wiener (Princeton, NJ: Princeton University Press, 1982), 69.

2. Duhem, *Aim and Structure*, 57.

the English mind, his most extensive and detailed example is not only not a scientist, he is not even an Englishman. In fact, in an ironic twist, he happens to be the French military general and tyrant, Napoleon Bonaparte. For Duhem, Napoleon is the archetype example of the English mind for three primary reasons: (1) his predilection for concrete facts and examples, (2) his antipathy of abstractions and generalizations, and (3) his emphasis on the role of visualization (or "imagination") in thinking.

It is clear from this brief description that the two kinds of minds are intended to be polar opposites of one another. The French mind believes that knowledge begins first and foremost with "common sense" (*connaissance commune*) first principles. Regarding the origin of these common sense first principles Duhem writes, "As for the axioms, where do they come from: They are taken…from common knowledge; that is, every person of sound mind takes it that he is sure of their truth before studying the science whose foundations they are.[3]" Duhem believes that the foundational principles of all scientific theories cannot be derived from reason like the other components of any scientific theory because this would lead us into the morass of the infinite regress problem. For Duhem, the only inevitable conclusion is that the axioms must be justified in an *arational* way by what can loosely be called human judgment or human intuition. Continuing with the epistemology of the French mind, from these first principles the faculty of reason applies the rules of formal logic and mathematics to deduce more extensive knowledge of the world. Quoting Duhem, "Starting from these ideas, from these principles, the deductive method will unroll its syllogisms whose long chain of links, all tested, will firmly tie the

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[3] Pierre Duhem, as quoted in, R.N.D. Martin, *Pierre Duhem: Philosophy and History in the Work of a Believing Physicist (La Salle, IL: Open Court, 1991), 82.*

most minute consequences to the foundations of the system."[4] For the so-called French mind, scientific knowledge is largely deductive knowledge, with the exception of the intuitive first principles on which all of the preceding knowledge rests.

Nothing that was said about the analytic French mind can be applied to the mechanical English mind. In discussing Bacon's *Novum Organum* Duhem dismissingly remarks, "There is no use in looking for Bacon's method in it, for there is none." Any modern reader will find this comment ironic given that Bacon is widely regarded as the progenitor of the so-called scientific method. Duhem would not be amused by the place Bacon has assumed in the annals of science; on the contrary he would likely be repulsed by it. While discussing the English mind's lack of methodology, Duhem rhetorically asks, "Will these instructions teach us to conduct and arrange our experiments in accordance with fixed rules? Will these directions teach us the way to classify our observations? Not in the least."[5] While French theory-making depends on "fixed rules" determined by logic, English theory-making "proceed[s] not so much by a consecutive line of reasoning, as by a piling-up of examples. Instead of linking up syllogisms, they accumulate facts."[6] While the English mind is primarily concerned with stockpiling concrete facts through observation and experiment, they are very much unconcerned with the axioms or first principles that provide the foundation for French theories.

Another significant difference between the two methods, especially as it pertains to models and modeling, are their divergent thoughts on the role of the imagination (or what Duhem calls elsewhere "visualization") in the production of scientific theories. By

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[4] Duhem, *Aim and Structure*, 65.

[5] Ibid., 66

[6] Ibid., 67.

visualization, Duhem is referring to the ability to create mental models in one's mind of some scientific phenomenon. Returning to the example of Napoleon, Duhem writes, "If we read again the portrait of Napoleon…we shall recognize immediately…an extraordinary power to hold in mind an extremely complex collection of objects, provided these are sensory objects having shape and color that the imagination can visualize".[7] We are told that though this power of visualization is celebrated in the English mind, it is abhorred by the French. The English believe that scientific explanation is impossible without these mental models because explanation of a scientific phenomenon comes down to discovering the internal mechanism(s) that enable it to function.[8] For the English, these internal mechanisms can only be ascertained by the imagination alone. What Duhem finds especially deplorable, however, is the English's insistence that the imagination has as much, if not more, to do with the production and development of scientific theories than pure, unadulterated reason.

4.3 Duhem's Three Criticisms

In the end, Duhem finds models problematic because: (1) models promote the concretization of abstract phenomena, (2) models are not developed logically by moving from syllogism to syllogisms, and (3) models are ultimately superfluous.

4.3.1 Concretization of the Abstract

Regarding (1), Duhem remarks:

> The French or German physicist conceives, in the space separating two conductors, abstract lines of force having no thickness or real existence; the English physicist materializes these lines and thickens them to the dimension of a

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[7] Ibid., 58.

[8] Ibid., 72.

> tube which he will fill with vulcanized rubber. In place of a family of lines of ideal forces, conceivably only by reason, he will have a bundle of elastic strings, visible and tangible…[9]

For the French mind, the concept "lines of force" is really an unfortunate misnomer because the word "lines" suggests the existence of a concrete entity with length, thickness, and tangibility. In reality, however, force is an abstract concept with no material properties such as the ones described above. Whereas the French use the phrase "lines of force" without reading too much into it, the English believe that understanding force in terms of actual physical lines is not that far from the truth. Instead of lines, Faraday imagines elastic strings connecting two conductors that stretch upon expansion and bunch up upon contraction.[10] Recall that for the English, understanding a phenomenon requires the ability to explain the mechanism(s) underlying it. This need to literally see the internal mechanism driving electrostatics is the impetus for Faraday's mechanical model. Quoting William Thomson (i.e., Lord Kelvin), "I never satisfy myself until I can make a mechanical model of a thing. If I can make a mechanical model, I understand it. As long as I cannot make a mechanical model all the way through I cannot understand".[11] Duhem, like Heisenberg after him, believes that the requirement of a visualizable mechanism that one can imagine in one's mind is "trash" and that such forays of the imagination actually obfuscate physicists' understanding of various scientific phenomena which can only be understood mathematically through the faculty of reason alone.

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[9] Ibid., 70.

[10] Ibid., 70.

[11] William Thomson, as quoted in Ibid., 71- 72.

4.3.2 Lack of Logical Progression

This leads us to Duhem's second criticism of models, which is that scientists who are sympathetic of models and model-making do not find it epistemically problematic when different models are logically inconsistent with one another. Recall that for Duhem, scientific theories are logically deduced from first principles. This means that with the exception of the first principles themselves, all of the premises that appear in a theory are carefully derived by moving from one syllogism to the next. We saw in the previous section, however, that the creation of models involves a much different process. The *modus operandi* of a model maker is to simulate the mechanisms by which different scientific phenomena work. In certain instances, logically inconsistent models will be proffered as equal explanations for a particular phenomenon. Duhem explains the difference in attitude between the French and English minds upon encountering such a contradiction:

> To a mathematician of the school of Laplace or Ampére, it would be absurd to give two distinct theoretical explanations for the same law, and to maintain that these two explanations are equally valid. To a physicist of the school of Thomson or Maxwell, there is no contradiction in the fact that the same law can be represented by two different models.[12]

Duhem was apparently commenting on Maxwell's own remark that "it is a good thing to have two ways of looking at a subject and to admit that there *are* two ways of looking at it."[13] Maxwell made this remark in order to bolster support for Faraday's conception of "lines of force" which stood in stark contrast to the more popular understanding of force

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[12] Duhem, *Aim and Structure*, 81.

[13] James Clerk Maxwell, as quoted in Alan F. Chalmers, "The Heuristic Role of Maxwell's Mechanical Model of Electromagentic Phenomena," *Studies in History and Philosophy of Science* 17, no. 4 (1986): 417.

as “action at a distance.”[14] Maxwell’s method embraced the idea of employing as many alternative models or “physical analogies” as possible, even if some of the models conflicted with one another. Duhem, on the other hand, scoffed at the model approach for its blatant disregard of logic, in particular the principle of non-contradiction. This application of models only furthered his belief that models are “not built for the satisfying of reason but for the pleasure of the imagination.”[15]

Duhem was further perplexed that those sympathetic to modeling maintained a steadfast belief in the epistemic potential of models, despite their own recognition that models were “not to be accepted as true in nature.”[16] Quoting Thomson, “Although the molecular constitution of solids supposed in these remarks and mechanically illustrated in our model, *is not to be accepted as true in nature*, still the construction of a mechanical model of this kind is undoubtedly very instructive.”[17] And quoting Maxwell, “Go back to our spherical molecule with its central spherical shells-that is the rude mechanical illustration, remember. I think it is very far from the actual mechanism of the thing, but it will give us a mechanical model.”[18] In Duhem’s mind, making inferences about real world phenomena from fictional models is logically dubious. Such an inference is based more on “hope” rather than anything to do with logic and reason.[19] In fact, Duhem makes the rather crude joke that the French are no longer astonished by anything the English

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[14] Chalmers, “Heuristic Role,” 417.

[15] Duhem, *Aim and Structure*, 81.

[16] James Clerk Maxwell, as quoted in Ibid., 84.

[17] Willliam Thomson, as quoted in Duhem, *Aim and Structure*, 75.

[18] James Clerk Maxwell, as quoted in Duhem, *Aim and Structure*, 85.

[19] Duhem, *Aim and Structure*, 85.

modelers say and do because the latter only “wished to produce a work of imagination”[20] and “not an explanation acceptable to reason.”[21] As if violating the principle of non-contradiction were not enough, modelers were also guilty of making unwarranted inferences.

4.3.3 Superfluous Models

Duhem’s most famous criticism of models is that they are nothing more than “crutches of the imagination” that play nothing more than a heuristic role in the conception of novel scientific theories. In a word, they are superfluous. At this juncture we should not be surprised that Duhem enlists Thomson and Maxwell as cases in point. He remarks of the two of them, “neither in Lord Kelvin’s nor in Maxwell’s work has the use of mechanical models shown that fruitfulness nowadays attributed so readily to it.”[22] In particular, Duhem says of Thomson that his most laudable contributions “have been made by means of the abstract systems of thermodynamics and of classical electrodynamics.”[23] In other words, his theories regarding the electrical transfer of heat did not require models for their conception; rather, they were introduced more for the purposes of representation and elaboration.[24] As for Maxwell, Duhem believes that he did not require his model of the mechanical ether to conceive of light an electromagnetic phenomenon.[25] This specific example is defended in contemporary scholarship by Alan

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[20] Ibid., 85.

[21] Ibid..

[22] Ibid., 98.

[23] Ibid., 97.

[24] Ibid., 97 – 98.

Chalmers who not only believes that Maxwell's "model did not serve a positive heuristic function" but even went so far as to state that it was actually "an unproductive digression."[26] Maxwell's theory was that displacement currents begat magnetic fields, which could be represented by rotating ether cells brought into motion by moving particles.[27] Chalmers argues, however, that taking Maxwell's famous model seriously requires that we give up the idea that displacement currents give rise to magnetic fields because displacement requires the vortices in the model to turn one way while the model itself requires them to turn the opposite direction.[28] Even though Thomson and Maxwell are both steadfast in their belief regarding the positive contribution of models to their theories, Duhem is equally persistent (if not more so) that compared to deduction, the role of models in the discovery and justification of theories in physics is at best a "quite meager" one.[29]

4.4 Responses to Duhem

As impressive as Duhem's critiques are, each one of them can be met with a response. Duhem's first critique will be handled by accusing him of creating a straw-man out of Thomson and Maxwell's positions. Regarding criticism two, I will simply deny Duhem the privilege of his realist metaphysics, circumventing his argument altogether. Lastly, the charge of superfluousness will be answered by employing Watson and Crick's DNA model as a counterexample.

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25. Ibid., 98.

[26] Chalmers, "Heuristic Role," 428.

[27] Ibid., 425.

[28] Ibid.

[29] Duhem, *Aim and Structure*, 99.

4.4.1 Response to Concretization of the Abstract

When Duhem accuses Faraday and Maxwell of concretizing the abstract, he is essentially charging them of reducing purely theoretical, mathematical explanations into physical, mechanistic ones. But Faraday does not believe that there are actual strings connecting conductors any more than Maxwell believes that ‘vortices’ and ‘electrical particles’ under stress cause electromagnetic activity.[30] The purpose of creating both models is to gain insight into how the concept of force actually works. For the so-called English mind, a purely mathematical explanation is insufficient because such an explanation is ultimately devoid of mechanism; it does not explain to us how something works. Maxwell states that in overemphasizing mathematics “we entirely lose sight of the phenomena to be explained; and though we may trace out consequences of given laws, we never obtain more extended views of the connexions [sic.] of the subject.” [31]

Faraday and Maxwell concretize the abstract in order to develop mechanistic models that explain how different phenomena (e.g. force) work. They do not literally believe in the physical existence of the models themselves, but they do stand behind the explanations that such models provide. The models themselves may be fictions, but their contributions to our scientific understanding certainly are not.

4.4.2 Response to Lack of Logical Progression

Against Duhem’s argument that models are illogical in the sense that sometimes contradictory models are used to describe a single phenomenon, my response is to agree

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[30] Nancy J. Nersessian, “Aether/Or: The creation of scientific concepts,” *Studies in History and Philosophy of Science Part A* 15, no. 3 (1984): 191.

[31] James Clerk Maxwell, as quoted in Ibid., 187.

with Duhem that contradictory models are used in this way, but to vehemently disagree that this application renders them illogical. Duhem's position reflects a strong realist stance where models are judged according to whether or not they accurately represent their targets. According to this view, there is only one reality, one way the world actually is, and a good model is a reflection of this reality. From a realist's perspective, contradictory models cannot both be used to describe the same phenomenon because there is only one reality and hence only one accurate description of the phenomenon. When it comes to scientific modeling, I do not presuppose the same realist position that Duhem assumes. I believe that models are better thought of as problem-solving instruments rather than the means by which nature's true essence is uncovered. As such, models are better served helping scientists abandon futile research programs and pointing them in the direction of more fertile projects. According to this instrumentalist view, contradictory models can be tolerated so long as they help scientists answer the questions, "Where do we go from here?" and "What would be a good project to do next?"[32]

This happens to be the case with the various contradictory models of the atom discussed in chapter two. Recall that Margaret Morrison convincingly makes the argument that there are over thirty models of the atomic nucleus still used by scientists, and that some of the models blatantly contradict one another (e.g., the liquid drop and shell model). If we are to accept Duhem's realist thesis, there can only be one model of the atomic nucleus that reflects the way the world actually is. The instrumentalist view, however, is more concerned with producing new hypotheses and making novel predictions than it is with getting the world right. Scientists who are realists will be the

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32. Thomas Nickels, "Heuristic Appraisal : Context of Discovery or Justification?," in *Revisiting Discovery and Justification*, ed. J. Schickore and F. Steinle (Netherlands: Springer, 2006), 167.

first ones to tell you of their aspiration to reconcile various contradictory models. On the other hand, scientists with an instrumentalist perspective will continue to dabble in the messy, at times contradictory, world of model-making so long as they are contributing to a productive research program.

4.4.3 Response to Superfluous Models

Duhem's argument that models are superfluous is the most stupefying because the history of science suggests otherwise. Probably the most famous (and successful!) physical model ever built in the history of science was Watson and Crick's double helix model of DNA.[33] Like all models, Watson and Crick's DNA model went through several revisions. One such revision was made after Rosalind Franklin pointed out that the three-chain model built by the two scientists could not possibly be correct because it did not provide "appropriate places for water molecules to attach themselves."[34] It is important to note that she realized this within minutes of inspecting the three-chain physical model.[35] Nevertheless, Franklin's rejection of the three-chain model did not dissuade Watson and Crick. A year later the two returned to model building, inspired largely by the fact that their chief competitor, Linus Pauling, had not already won the Nobel Prize for solving the DNA structure riddle. This time, however, Watson suggested that they build a two-chain model instead of a three-chain one because of his hunch that "important biological objects come in pairs."[36] The end result was a double helix model made out of cardboard

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[33] 33. There will be some overlap between the DNA examples in chapters 2 and 4. However the examination in the present chapter will provide a more in-depth account than the one provided in chapter 2.

[34] 34. Ronald N. Giere, *Science Without Laws* (Chicago: The University of Chicago Press, 1999), 73.

[35] 35. Ronald N. Giere, *Understanding Scientific Reasoning* (Belmont, CA: Wadsworth, 1997), 15.

that contained “the secret of life.”[37] Watson and Crick restrained themselves from a full blown demonstration until they were able to replace the flimsy cardboard model with a more secure metal one that satisfied available x-ray data and the laws of stereochemistry.[38]

When it comes to models and their significance, the point of the DNA example cannot be clearer: the construction and manipulation of cardboard and metal models contributed to Watson and Crick’s discovery of DNA’s double helix structure. Now the question arises, how much did the models contribute? If we take Watson’s personal memoir, *The Double Helix*, seriously, the answer is that models contributed greatly. As noted above, Watson and Crick’s original three-chain model was easily discarded by Franklin when she *saw* that their model could not possibly hold the amount of water needed by actual DNA. In other words, Franklin used the physical model as an instrument to problematize the duo’s initial theory of DNA structure. There is irony in Franklin using Watson and Crick’s model this way because it was Franklin herself who believed that using “tinker-toy-like models to solve biological structures was clearly a last resort.”[39] In fact, Watson suggests that one of the reasons Franklin opposed their project from the beginning was her antipathy towards model-making in general. Watson remarks, “Obviously affecting Rosy’s transformation was her appreciation that our past hooting about model building represented a serious approach to science, not the easy

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36. James Watson, *The Double Helix: A Personal Account of the Discovery of the Structure of DNA* (New York: Touchstone, 1968), 171.

37. Watson, *The Double Helix*, 197.

38. Ibid., 200.

39. Ibid., 69.

resort of slackers who wanted to avoid the hard work necessitated by an honest scientific career."[40]

However, the use of models in the discovery of DNA is mostly appreciated when we consider that the famous spiral staircase visual was more the result of Watson tinkering with his cardboard models than laboriously working on complex equations. As Watson's account of the discovery goes, American crystallographer, Jerry Dohohue, informed him that he was probably working with the wrong tautomeric form of both guanine and thymine.[41] If Donohue was correct, then Watson's like-with-like base pair model was not going to work because the bases could not possibly bond the way that the above model required.[42] It was at this point that Watson began playing with his cardboard models of adenine, thymine, guanine, and cytosine. In Watson's own words:

> When Jerry [Donohue] came in I looked up, saw that it was not Francis, and began shifting the bases in and out of various other pairing possibilities. Suddenly I became aware that an adenine-thymine pair held together by two hydrogen bonds was identical in shape to a guanine-cytosine pair held together by at least two hydrogen bonds. All the hydrogen bonds seemed to form naturally; no fudging was required to make the two types of base pairs identical in shape.[43]

In other words, Watson came to the realization that adenine bonded with thymine and guanine with cytosine by simply moving the different bases around until they bonded in a way that required "no fudging," much the same way that someone puts a jigsaw puzzle together by moving different pieces around until they also form without "fudging." The more skeptical Crick also had his hand at "pushing the bases together in a number of

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[40] Ibid., 212.

[41] Ibid., 190.

[42] Giere, *Understanding Scientific Reasoning*, 17.

[43] Watson, 196.

different ways"[44] but inevitably came up with the same model formation as Watson. It is important to point out that the analogy between molecular bonding and jigsaw puzzles is simply not an attempt at a vivid description on my part. Rather, it was inimical to Watson and Crick's model making method, which they appropriated from Linus Pauling. In 1951, Pauling, along with Robert Corey and Herman Branson, used the modeling technique to discover the structure of protein molecules.[45] Historian Horace Judson described Pauling's models as 'open three dimensional puzzles' that looked like they had to be fitted together.[46] More importantly, Crick characterized Pauling's model as "rather like a three-dimensional jigsaw puzzle with curious pieces joined together by rotatable joints."[47]

Notice that Watson's account of the base pair discovery lacks any mention of 'deduction from abstract theories' or 'strict adherence to the rules of logic and reason' or any other phrase made famous by Duhem in his attack on models and model-making. The reason is that Watson himself all but admits that the initial base pair discovery was not the result of some grandiose logical deduction from abstract first principles; instead it was made by simply moving different parts of the model up and down until they seemed to fit with one another. Now it would be absurd to accuse Watson and Crick of not applying their reasoning abilities when they had to reconcile their model with Chagraff's rules, the x-ray diffraction data, and the laws of stereochemistry. They recognized more than anyone the need for their model to be logically consistent. At the same time, it

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[44] Ibid., 197.

[45] Samuel Schindler., "Model, Theory, and Evidence in the Discovery of the DNA Structure," *British Journal of Philosophy of Science* 59, no. 4 (2008), 630.

[46] Schindler, "Model, Theory, and Evidence," 630.

[47] Francis Crick, as quoted in Schindler, Ibid., 630 – 631.

would be just as absurd to downplay the role that the “concrete, material, visible, and tangible”[48] model played in their discovery. Duhem famously mocked the English approach to physics by remarking, “We though we were entering the tranquil and neatly ordered abode of reason, but we find ourselves in a factory.”[49] Had Watson and Crick not taken this factory approach, it is doubtful that they would have ever uncovered “the secret of life.”[50]

4.5 Duhem and Modern-day Theoretical Models

Duhem objects to models on the basis that they are tangible, physical objects that one operates using one’s hands rather than abstract, mathematical entities that one cognizes through deductive reason alone. Anyone remotely familiar with the scientific modeling literature of the past twenty-five years will find Duhem’s view ironic in the sense that mathematical modeling has become the de facto model of the field. From Cartwright’s discussion of superconductivity, to Giere’s famous linear oscillator example, to Weisberg’s Lotka-Volterra model of predation, “dynamical mathematical models represented by differential equations” are all the rage. In fact, I am tempted to state that Duhem would not have the antipathy he had towards models had he lived in this era of mathematical models instead of his own mechanical era. But this would be a mistake because mathematical models (in the form of mathematical analogies) were around during Duhem’s time and they were still not enough to dissuade him. In particular, Thomson had already argued that the distribution of heat was mathematically

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[48] Duhem, *Aim and Structure*, 70.

[49] Ibid., 71.

[50] Watson, *The Double Helix*, 197.

identical to the distribution of electricity and that the principles of each theory could be applied to one other.[51] However, Duhem believed that analogies between different physical laws could be fruitful for discoveries, "but we should not confuse them with the use of models."[52] Duhem did not consider the application of a mathematical equation to a completely different field an instance of modeling because the process did not involve "a vision of concrete collections."[53] In other words, Duhem restricted the concept of model to mechanical models (i.e. models that are physical, visible, tangible, concrete, etc.) only. This means that the very notion of a mathematical model was an oxymoron to Duhem who thought that the abstract nature of mathematical entities precluded them from being classified in the same category as models, which he thought of strictly in terms of mechanical models.

As mentioned above, contemporary scholarship has moved past the idea of equating models with mechanical models to the extent that the latter have become all but anachronistic. According to Giere, "The class of scientific models includes physical scale models and diagrammatic representations, but the models of most interest are *theoretical* models."[54] Notice that Giere does not say that theoretical (i.e. mathematical) models are of most interest *to him*; this suggests to me that he is making a general statement about philosophers of science in general and few would disagree with him. This paradigm shift in modeling is significant because it suggests that despite his fierce anti-modeling

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[51] William Thomson, as quoted in, Giora Hon and Bernard R. Goldstein, "Maxwell's Contrived Analogy: An Early Version of the Methodology of Modeling," *Studies in History and Philosophy of Modern Physics* 43, no.4 (2012): 247.

[52] Duhem, *Aim and Structure*, 97.

[53] Ibid.

[54] Giere, *Science Without Laws*, 5.

diatribes, Duhem is more relevant than ever. One only needs to consider the war of words between Schrodinger and Heisenberg regarding the viability of quantum mechanics to realize that this is the case. In 1926 Schrödinger wrote, “I knew of his [Heisenberg’s] theory, of course, but felt discouraged not to say repelled, by the methods of the transcendental algebra, which appeared very difficult to me and by the *lack of visualizability*. [my emphasis]”[55] In a letter to Schrödinger, Lorentz expressed his skepticism of quantum mechanics by remarking that “if I had to choose between your wave mechanics and the [quantum] mechanics, I would give preference to the former, owning to its *greater visualizability*. [my emphasis]”[56] Heisenberg was acutely aware of how his fellow physicists perceived the mathematical turn in atomic physics and his message for them was to get over it. Regarding the views of Schrödinger, Heisenberg said, “The more I reflect on the physical portion of Schrödinger’s theory the more disgusting I find it. What Schrödinger writes on the visualizability of his theory…I consider trash.”[57] In 1926 Heisenberg delivered a paper that many believe killed visualizability in atomic physics once and for all. In it he explained that “the electron and atom possess not any degree of physical reality as the objects of daily experience…Investigation of the type of physical reality which is proper to electrons and atoms is precisely the subject of atomic physics and thus also of ‘quantum mechanics’.”[58] Visualizability was dead; mathematics was king; and the word ‘model’ was to be the exclusive domain of abstract mathematics. This episode suggests that even in modern

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55. Arthur I. Miller, *Insights of Genius: Imagery and Creativity in Science and Art* (Cambridge, MA: The MIT Press, 2000), 56.

[56] Miller, *Insights of Genius*, 57.

57. Ibid., 58.

58. Ibid., 59.

physics Duhem’s dichotomy between the strong but narrow French mind and the ample but weak English mind are more than relevant than ever. Furthermore, if Heisenberg is correct about the non-visualizability of quantum mechanics, then Duhem sounds less like a raving, anti-model curmudgeon and more like a harbinger of the abstract, theoretical models to come.

Part II

Chapter 5

The Role of Models in Science Education

5.1 Definitions of 'Model' in the Science Education Literature

With the explosion of literature on modeling in science education, it should come as no surprise that definitions of the term 'model' are ubiquitous. Consider some of these more refined definitions:

A. Gilbert, Boulter, and Elmer - "A model in science is a representation of a phenomenon initially produced for a specific purpose...The specific purpose for which any model is originally produced in science…is as a simplification of the phenomenon to be used in enquiries to develop explanations of it. Many models are composed of entities which are concrete…Other models are composed of abstractions…A model can thus be of an idea. A model can consist of a mixture of entities which are concrete (e.g. masses) and of entities which are treated as if they are concrete (e.g. forces acting on masses). A model can be of a system…A model can be of an event…A model can be of a process…A Thought Experiment is a model of that group of processes known as a 'scientific experiment' carried out entirely within the mind as an idea, a mental model."[1]

B. Passmore and Stewart - "a scientific model is a set of ideas that describe a natural process. A scientific model (construed of *objects* and the *processes* in which they participate) so conceived can be mentally "run," given certain constraints, to explain or predict natural phenomena."[2]

C. Halloun - "A *scientific model* is…a conceptual system mapped, within the context of a specific theory, onto a specific *pattern* in the structure and/or behavior of a set of physical systems so as to reliably represent the pattern in question and serve specific functions in its regard. These function may be *exploratory* (pattern description, explanation, and prediction or post-diction), or *inventive* (pattern reification in existing physical realities or in newly devised realities). Mapping is done so the model captures the essence of the

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1. John K. Gilbert, Carolyn J. Boulter, and Roger Elmer, "Positioning Models in Science Education and in Design and Technology Education," in *Developing Models in Science Education*, ed. John K. Gilbert and Carolyn J. Boulter. (Dordrecht, Netherlands: Kluwer Academic Publishers, 2000) 11.

2. Cynthia Passmore and Jim Stewart, "A Modeling Approach to Teaching Evolutionary Biology in High Schools," *Journal of Research in Science teaching* 39, no. 3 (2002): 188.

pattern, and this by concentrating on specific but not all details in the physical realities exhibiting the pattern, particularly on *primary* details that are salient to the model function."[3]

D. National Research Council (NRC)

  - "In science, models are used to represent a system (or parts of a system) under study, to aid in the development of questions and explanations, to generate data that can be used to make predictions, and to communicate ideas to others."[4]

  - "Models are tentative schemes or structures that correspond to real objects, events, or classes of events, and that have explanatory power. Models help scientists and engineers understand how things work."[5]

  - "Scientists construct mental and conceptual models of phenomena. Mental models are internal, personal, idiosyncratic, incomplete, unstable, and essentially functional. They serve the purpose of being a tool for thinking with, making predictions, and making sense of experience. Conceptual models…are…explicit representations that are in some ways analogous to the phenomena they represent….Conceptual models are in some senses the external articulation of the mental models that scientists hold and are strongly interrelated with mental models."[6]

E. National Science Teachers Association (NSTA) - "A model is a representation, usually visual but sometimes mathematical, used to aid in the description or understanding of a scientific phenomenon, theory, empirical law, physical entity, organism, or part of an organism."[7]

F. American Association for the Advancement of Science (AAAS)

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[3] Ibrahim Halloun, *Modeling Theory in Science Education* (Dordrecht, Netherlands: Springer, 2006), 23.

[4] National Research Council, "Appendix F – Science and Engineering Practices in the NGSS," *NGSS Release* (April 2013): 6, accessed July 29, 2013, http://www.nextgenscience.org/sites/ngss/files/Appendix%20F%20%20Science%20and%20Engineering%20Practices%20in%20the%20NGSS%20-%20FINAL%20060513.pdf.

[5] National Research Council, as quoted in Halloun, *Modeling Theory in Science Education*, 23.

[6] National Research Council, *A Framework for K – 12 Science Education: Practices, Crosscutting Concepts, and Core Ideas* (Washington D.C.: The National Academies Press, 2012), 56.

[7] National Science Teachers Association, as quoted in Halloun, *Modeling Theory in Science Education*, 23.

- "A model of something is a simplified imitation of it that we hope can help us understand it better. A model may be a device, a plan, a drawing, an equation, a computer program, or even just a mental image. Whether models are physical, mathematical, or conceptual, their value lies in suggesting how things either do work or might work…Models may also mislead, however, suggesting characteristics that are not really shared with what is being modeled."[8]

- Physical, mathematical, and conceptual models are tools for learning about the things they are meant to resemble…The term *model* should probably be used to refer only to physical models in the early grades, but the notion of likenesses will be the central issue in using any kind of model.[9]

Carefully looking at these definitions, all but one (Passmore and Stewart), explicitly state that models are representations or imitations of some sort. Passmore's and Stewart's explanation for this exclusion is that although they believe that representation is central to model formation, it is not "the primary cognitive aim of scientists."[10] The NSTA's and AAAS's definitions are the only ones that make a direct reference to mathematical representations, while the NRC's definition is the only one that overtly states that models represent "real" objects. All of these definitions, with the exception of the NSTA's, explicitly state that models provide explanations.[11] Three of the authors, Passmore & Stewart, Halloun, and the NRC, maintain that models can be used to make predictions about phenomena. When we look at the NRC's, NSTA's, and AAA's conceptions of what a model is they are alike in that they believe that models have a pedagogical function. This is not surprising because the authors sympathetic to this definition are all

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[8] American Association for the Advancement of Science, *Science for All Americans* (New York: Oxford University Press, 1990), 168.

[9] American Association for the Advancement of Science, *Benchmarks for Science Literacy* (New York: Oxford University Press, 1993), 267.

[10] Passmore, Stewart, "Modeling Approach" 188.

[11] AAAS's conception does not explicitly use the word 'explanation,' however, the phrase "suggesting how things…work" means the same as providing an explanation.

science education organizations whose very function is inherently pedagogical. Gilbert, Boulter, and Elmer and AAAS are alone in explicitly stating that models are simplifications of phenomena. Passmore and Stewart, Gilbert, Boulter, and Elmer, and the NRC are the only authors who refer to mental models. And Halloun's definition stands alone in expressing a direct relationship between models and theories. Even though five of the six authors above agree that models represent and generate explanations, the science education community is nowhere close to agreeing upon a collection of necessary and sufficient conditions as to what makes a model a model.

5.2 Comparison with the Integration Account

In Section 3.1.3, the integration account defines a model as a temporary collection of integrated elements, whose purpose is to help scientists solve different kinds of scientific problems. Recall that the elements included in the above definition include all of the ideas, concepts, processes, physical instruments, and experiments that are involved in constructing a model. In addition to the latter must be included all of the: laws of nature, theories, research programs, tacit knowledge (including metaphysical assumptions), sources and amount of funding, interpretations of data, historical development of a field, as well as any fortuitous, non-rational contingencies that may have contributed to the construction and development of a model (i.e. luck), and *themata*.

In the present section, the shortcomings of the definitions articulated in section 5.1 will be thoroughly explored and eventually compared with the integration account. Because the preceding definitions share many of the same shortcomings, they will be treated as a whole, rather than scrutinized individually.

5.2.1 Shortcomings of Definitions in 5.1

Representation

Gilbert, Boulter, and Elmer begin their definition by stating that a model is a "representation of a phenomenon."[12] Halloun argues that models "reliably represent" the patterns of structures and behaviors of physical systems.[13] Like Halloun, the NRC also believes that models are representations of a system or part of a system. The NSTA's definition is similar to Gilbert, Boulter, and Elmore's in that they also maintain that a model is a representation of a phenomenon, further adding that models can be visual as well as mathematical.[14] The NRC does not explicitly use the word 'representation,' however their definition certainly implies it by defining models as schemes or structures that correspond to real objects and events.[15] As I mentioned towards the end of 5.1, five out of the six definitions either explicitly or implicitly refer to models as representations of a phenomenon, system, or object. The only authors that exclude any kind of allusion to representation are Passmore and Stewart.

The problem of representation was thoroughly explored in chapter 2 of this project. However, given science education's fixation on models as representations, I believe that it is more than appropriate to revisit some of those arguments here. The problem with defining models as a representation is that such a definition assumes that models have some kind of designated target of which they are a representation, such as: the atom, DNA, the ether, etc. The inadequacy of this targeting approach is two-fold: (1)

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[12] Gilbert, Boulter, and Elmer, "Positioning Models in Science Education," 11.

[13] Halloun, *Modeling Theory in Science Education*, 23.

[14] National Science Teachers Association, as quoted in, Halloun, *Modeling Theory in Science Education*, 23.

[15] National Research Council, "Appendix F," 6.

not all models are representations of real phenomena or systems, and (2) in many instances it is unclear what the target of a model is actually like. Regarding (1), James Clerk Maxwell is famous (or infamous) for creating models that have no parallel in the real world. For instance, in order to think mathematically about Faraday's lines of force, Maxwell re-imagined electromagnetic phenomena as an incompressible fluid flowing in both directions at varying velocities through a tube.[16] Now Maxwell was not naïve enough to believe that electromagnetism actually consisted of an incompressible fluid and tubes. What he did believe, however, was that the experimental physics of his time provided him with very little that would help him understand electromagnetism, let alone formulate it mathematically. So what he did instead was create "his own artificial physics to suit the purpose he set himself."[17] Maxwell's "tubes of incompressible fluid" are wholly contrived in the sense that they are "not intended to illustrate anything in nature" and certainly do "not represent a physical system."[18] But this does not at all diminish the significance of Maxwell's model because the aforementioned contrivance of fluid and tubes laid the groundwork for his famous Laws of Electromagnetism, which make computers, the Internet, television, radio, and radar all possible.

Another reason why science educators need to overcome their commitment to representation is that it is not uncommon for there to be several empirically adequate models of a single phenomenon at the same time. In chapter 2 I discussed the fact that there are over 30 working models of the atom currently used by physicists for their

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16. Giora Hon and Bernard R. Goldstein, "Maxwell's Contrived Analogy: An Early Version of the Methodology of Modeling," *Studies in History and Philosophy of Modern Physics* 43, no.4 (2012): 243.

17. Hon and Goldstein, "Maxwell's Contrived Analogy," 244.

18. Ibid., 237.

specific purposes. Each physicist uses a particular model of the atom because it either fits their interpretation of the data, fits their predictions, fits their preconceived notions (e.g. theories), or a combination of some or all of these reasons. The point is that it is unproductive to discuss models in terms of a relationship to a target because scientists themselves have moved past such talk and are mostly concerned with how models help them solve scientific problems. Thomas Nickels goes a step further and adds that, "Inconsistency, that worst of logical sins, is today widely recognized as a frequent characteristic of scientific theorizing…Many scientific theories have been plagued by inconsistencies and near-inconsistencies or 'conceptual blowups', yet several such theories have been extremely fruitful."[19] While some scientists still concern themselves with whether or not their model is the one true representation of a phenomenon, others move forward to the next problem to be solved, the next fruitful research program, and even the next research grant. The integration account is adamant that scientific modeling must move on to a post-representation phase where a model's value is tied to its "problem solving capacity"[20] instead of how isomorphic it is to a given target. According to Nickels, "Scientists are not content…to become spectators of the universe, with their models and theories being representational object of epistemic admiration…Rather, they want and need tools that they can use to get on with their work."[21]

Simplicity

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19. Thomas Nickels, "'Twixt Method and Madness,'" in *The Process of Science: Contemporary Philosophical Approaches to Understanding Scientific Practice*, ed. Nancy J. Nersessian (Dordrecht, Netherlands: Martinus Nijhoff Publishers, 1987), 60 – 61.

20. Thomas Nickles, "Heuristic Appraisal: Context of Discovery or Justification?." in *Revisiting Discovery and Justification*, ed. Jutta Schickore and Friedrich Steinle (Dordrecht, Netherlands: Springer, 2006), 61.

21. Nickels, "Heuristic Appraisal," 164.

Gilbert, Boulter, and Elmer continue with their definition, maintaining that, “The specific purpose for which any model is originally produced in science…is as a simplification of the phenomenon to be used in enquiries to develop explanations of it.”[22] The worrisome phrase in this portion of the definition is “simplification,” but not for the reason one would expect. Admittedly, it is tempting to argue that models are not simplifications by simply pointing to any mathematical model used by a physicist or a statistical model used by an economist and asking the rhetorical question, “Are those models simple?”. However, this would be a case of equivocation, mistaking the terms “simplification” and “simple.” To say that a model is a simplification of a phenomenon is to say that it is a simple representation of the phenomenon in question. That is, it is a more accessible version of the phenomenon itself. On the other hand, when a model is described as simple, *the model itself* is being described as basic and accessible, which has nothing to do with the relationship between model and phenomenon. For example, the Millenium Run model used by cosmologists to simulate the early universe consists of a number of complex mathematical and statistical models hardly anyone would classify as simple. But at the same time, because it is a simulation of how cosmologists think the early universe was created, it is nevertheless a simplification of the universe moments after the Big Bang.

The real issue with simplification is that it allows the Duhemian narrative regarding the dispensability of models to continue. If models are viewed as simplified versions of a phenomenon, then once that phenomenon is fully comprehended the model has outlived its usefulness. But as I argue in chapter four, this is far from the case.

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[22] Gilbert, Boulter, and Elmer, “Positioning Models,” 11.

According to this particular train of thought, Watson and Crick could have very easily discovered the structure of DNA without constructing their cardboard and metal plate models. Logically speaking, the latter scenario is definitely a possibility; however, the fact of the matter is that Watson and Crick would not have discovered the structure of DNA when they did, had they not busied themselves with model-making. In fact, Watson makes a point to stress the importance of model making in the DNA discovery. In his personal memoir, he explains,

> The a-helix had not been found by only staring at X-ray pictures…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 no 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.[23]

To argue that Watson and Crick's discovery could have been made without models is not only idle speculation, it is also an affront to Watson and Crick themselves, whose creativity and imagination led them to unprecedented ideas that would forever change humankind's understanding of the natural world.

According to integration account, scientists do not *resort* to models out of intellectual desperation; rather they construct them because models have a proven track record when it comes to solving scientific problems. The integration account believes that the model itself, replete with all of the elements (e.g. theories, laws, materials, funding, etc.) that go into its construction, is just as endemic to the scientific process as the problem it is helping to solve. In fact, I would even go so far as to say that groundbreaking discoveries discussed in the preceding paragraph occurred precisely because all of the elements in each respective model happened to be integrated in just the

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[23] James Watson, *The Double Helix* (New York: Simon & Schuster, 1996), 50.

right way, at just the right time, with just the right people doing the integrating. In other words, the model works precisely because the model's elements interact in such a way that builds-in justification. This not only speaks to the contingency and fragility of discovery; it also supports my contention that Duhem is unequivocally mistaken when it comes to the dispensability of models.

Explanation

Four out of the six definitions in 5.1 explicitly state that models possess an explanatory function. Passmore and Steward describe scientific models as objects or processes that can be mentally run in order to explain or predict natural phenomena.[24] Halloun believes that models have an exploratory function that includes explanation. [25] According to the NGSS, models are used to aid in the development of explanations. [26] And the NRC defines models as tentative schemes or structures that have explanatory power. [27]

The focus of the science education community on a model's explanatory power is significant because it clearly demonstrates their position that a model's value is epistemic rather than instrumental (i.e. problem solving). When science educators remark that scientific models explain, what they are really saying is that scientific models provide *true* explanations of how entities and processes in the world actually work. It might be the case that science educators are weakening their requirements regarding representation and similarity (i.e. models do not have to completely represent their targets and be similar

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[24] Passmore, Stewart, "Modeling Approach," 188.

[25] Halloun, *Modeling Theory in Science Education*, 23.

[26] National Research Council, "Appendix F" 6.

[27] National Research Council, as quoted in, Halloun, *Modeling Theory in Science Education*, 23.

to them in all aspects), but we cannot imagine this when it comes to explanations. In science education, it is all but assumed that scientific explanations are *true* explanations, with the only caveat being that a day will come when model *x*'s explanation of phenomenon/process *a* might be surpassed by model *y*'s *truer* explanation of the said phenomenon/process and so on. The science education community makes epistemological matters their concern because that is what they believe the scientific community is concerned with. But this belief is ultimately misguided. In "Kuhn's Philosophy of Scientific Practice," Joseph Rouse maintains that "Scientists' primary concern is not whether present beliefs are likely to be true, but instead whether available models of inquiry can effectively guide further research."[28] According to Rouse, phlogiston theory was not replaced because it was empirically falsified; it was abandoned because of its failure to guide further research into the new gases discovered in pneumatic chemistry.[29]

The science education community has to come to grips with the view that scientists are less worried about the explanatory power of their models and more concerned with the quality and quantity of the research projects that their models generate. Contrary to popular belief, model builders are not driven and motivated by the question, "Is my model true?" Quite the reverse, scientists are more future-oriented than we give them credit for. In place of epistemic questions like the one above, scientists are

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[28] Joseph Rouse, "Kuhn's Philosophy of Scientific Practice," in *Thomas Kuhn*, ed. Thomas Nickels (Cambridge: Cambridge University Press, 2003), 115.

[29] Rouse, "Kuhn's Philosophy," 115.

more concerned with more pragmatic questions such as, “Where do we go from here?,” “What would be a good project to do next?,” and “What is the next big thing?”[30]

Again, the science education community holds the epistemic view because they believe it reflects the view of the science community at large. But as I have attempted to argue in this section, the scientific community itself does not view models epistemologically. If the science education community wants what it does in the classroom to reflect what science practitioners do in the real world, they need to first free themselves from 20th century philosophy of science’s (specifically, logical positivism’s) fascination with justification.

5.3 Students and Teachers’ View of Models

This section will take a look at the views of various science teachers and students as it pertains to scientific models and compare their accounts with the science education literature. By the end of the section the reader will see an obvious connection between the literature and the teachers and students’ views; namely, all of the above views are inconsistent with the integration account and current scientific practice.

5.3.1 Science Teachers and Students’ Model Epistemology Levels

One of the most telling studies concerning students’ epistemology of models (what they are, their use, under what conditions they can be changed, etc.) was performed by Grosslight et al.[31] The study surveyed three distinct groups (seventh graders, eleventh

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[30] Nickels, “Heuristic Appraisal,” 167.

[31] Lorraine Grosslight, Christopher Unger, and Eileen Jay, “Understanding Models and their Use in Science,” *Journal of Research in Science Education* 28, no. 9 (1991): 799 – 822.

grade honor students, and experts[32]) to see if there was a noticeable difference between the ways that they all thought about and conceived of models. The survey questions were classified into five categories: (1) kinds of models (e.g. What comes to your mind when you hear the word 'model'?), (2) purposes of models (e.g. What are models used for?), (3) designing and creating models (e.g. How close does a model have to be to the thing itself?), (4) changing a model (e.g. Would a scientist ever change a model?), and (5) multiple models (e.g. Can a scientist have more than one model for the same thing?).

Using the data from the surveys, the authors were able to identify three general levels of thinking about models, each level exhibiting a more sophisticated epistemology than the previous one. A level one understanding implies that the subjects surveyed believe that models are simple copies of reality. That is, "Models are thought to be useful because they can provide copies of actual objects or actions."[33] Those who possess a level two understanding are more epistemologically sophisticated than their level one counterparts in that they no longer believe that models provide complete representations of reality. Instead, they realize that models are "repackaged" by modelers in order to simplify or highlight specific aspects of the things being modeled.[34] However, those at a level two understanding still focus on how models represent reality and do not consider how models can be used to generate novel ideas and theories, an essential characteristic of level three understanding according to Grosslight et al. Unlike those at a level one and

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[32] The experts consisted of a museum director, a high-school physics teacher, an engineering and education professor, and a researcher specializing in thinking and representation.

[33] Grosslight, Unger, and Jay, "Understanding Models," 817.

[34] Ibid.

two understanding, those at level three have move passed the idea that models are supposed to act as mirrors of reality.

Grosslight et al.'s study found that a majority (67%) of the 7th graders surveyed demonstrated a level one understanding.[35] The 11th grade honors students fared much better with only 23% of them scoring at level one.[36] The rest of them either received a pure level two score (36%), or a mix of level one and two scores (36%).[37] None of the 11th grade honor students, however, scored at either a pure level three or mixed level two and three.[38] As for the experts, all of them received scores consistent with a level three understanding.[39] For Grosslight et al. the main conclusion of the study is clear: middle and high school students' epistemology of models is rather unsophisticated when compared to experts in the field. The latter understand that models can be used to drive further research, while the former have retained the naïve view that models are only useful insofar as they transmit information about the world as it really is. This understanding of models is dangerous in that it encourages students to think of models as "a package of facts…that needs to be memorized" rather than as dynamic tools of inquiry that aid both our interpretation and construction of reality.[40]

Since Grosslight et al.'s study, researchers have been interested in the idea of epistemological levels of understanding of models and applying these levels to both

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35. Ibid., 818.

36. Ibid.

37. Ibid.

38. Ibid.

39. Ibid.

40. Ibid., 820.

students and teachers alike. Van Driel and Verloop looked at secondary teachers' understanding of model epistemology using an open-item questionnaire and discovered that, for the most part, teachers possessed a level one (naïve) understanding of models.[41] In 2001, Harrison interviewed 10 experienced high school teachers with the following results: two scored at pure level 1, two scored at mixed level 1 and 2, four scored at mixed level 2 and 3, and two scored at pure level 3.[42] Although Harrison's study is more optimistic than J. Van Driel and Verlop's in that only 20% of the participants scored at pure level 1, it is nevertheless discouraging that only 20% received the desired score of pure level 3 (sophisticated).[43] Both of these studies should make us skeptical of Grosslight et al.'s study as it pertains to the subcategory of experts. In their study, the physics teacher scored at pure level three, but as the previous studies point out, such a score is the exception rather than the rule.

When we compare the results of these three studies to the integration account, it is clear that the pervasive view amongst secondary school teachers and students are: (1) that models are supposed to represent their targets, and (2) that as models continually approach their targets, they become more and more true. In other words, both view models epistemologically rather than instrumentally. What matters to secondary teachers and students alike is a model's ability to represent the truth, not its potential to solve problems and guide further research.

5.3.2 Rutherford's Case Study on Models of Light

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[41] Rosaria Justi and John Gilbert, "Teachers' Views on the Nature of Models," *International Journal of Science Education* 25, no. 11 (2003): 1371.

[42] Justi and Gilbert, "Teachers' Views," 1371.

[43] Ibid.

An intriguing case study was performed by Margaret Rutherford on models of light.[44] Rutherford chose the phenomenon of light because like the interior of the atom there is no single consensus model that applies to it; rather there are three distinct models referred to by scientists: particle, wave, and dual particle-wave. The particle-wave model is certainly the most interesting because its construction is essentially an admission by physicists that in certain instances light behaves as a particle, while in others it behaves as a wave. One of the most interesting aspects of Rutherford's study was the data she gathered on models of light from an analysis of twelve textbooks (biology, chemistry, and physics) and six teacher interviews.[45] Her examination led her to the surprising conclusion that "The single most used model in teaching light is neither the wave nor the particle model. It is the original diagrammatic model where a straight line with an arrow on one end is said to represent a 'ray' of light."[46] I say surprising because the most frequent textbook model used to represent light was neither of the three used by practicing scientists. Although models are understood to be simplifications of their targets, the simplifications must not be so drastic that they obscure the most important properties of the phenomenon or process being represented. It is not uncommon for models to miss their respective targets, but this case is entirely different. It is as if the textbook authors knew which models they had to choose from, yet consciously created an entirely different model altogether. This view is supported by one interviewed teacher

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[44] Margaret Rutherford, "Models in the Explanation of Physics: The Case of Light," in Gilbert and Boulter, *Developing Models,* 253 – 269.

[45] Ibid., 258.

[46] Ibid.

who recommended that the wave model of light should only be introduced to students seventeen and older.[47]

The implication of this particular case study is of great consequence because it demonstrates a clear demarcation between scientific models on the one hand and pedagogical models on the other. Scientific models are the models used by the scientific community to help its members solve problems and guide future research, while pedagogical models are the models used by the educational community to teach students about the current beliefs of the scientific community. It cannot be stressed enough that these two conceptions of models are at cross purposes—the former instrumental, the latter epistemological. The ray model described above is a pedagogical model with no equivalent in real-world science. But this by itself does not render it useless, because as I argue in chapter two, a model (even a pedagogical model) can be devoid of a target so long as it fulfills its problem-solving function. In this particular instance, the problem that requires resolution is pedagogical rather than scientific. The teacher in question is struggling to get her students to understand the nature and behavior of light, not attempting to solve a new problem in the field. Completion of her pedagogical task requires the use of models, but ones that are markedly different than scientists are certainly accustomed to. The rest of this chapter will explore the nature and necessity of this scientific/pedagogical model distinction.

5.3.3 Content Knowledge vs. Pedagogical Content Knowledge: An Analogy

To better understand the relationship between scientific models and pedagogical models, one should consider Lee Shulman's distinction between content knowledge and

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[47] Ibid., 268.

pedagogical content knowledge. Content knowledge consists of all the technical knowledge of a particular subject. In science, scientific content knowledge consists of all the particular theories, laws, and equations that scientists employ on a daily basis. Pedagogical content knowledge is different. Pedagogical content knowledge “represents the blending of content and pedagogy into an understanding of how particular topics, problems, or issues are organized, represented, and adapted to the diverse interests and abilities of learners, and presented for instruction.”[48] It is the knowledge that educators possess that enables them to “transform” erudite academic content into something more manageable for their students.[49] In science, scientific pedagogical content knowledge consists of the body of teaching knowledge that teachers use to make the learning of theories, laws, and equations more approachable. In this regard, pedagogical content knowledge is actually more of a skill or ability than a body of knowledge.

The relationship between scientific models and pedagogical models follows very much the same logic. Scientific models are the models employed by scientists to solve scientific problems and guide future research. Like content knowledge, scientific models are quite technical and beyond the comprehension of the uninitiated layperson. Pedagogical models, on the other hand, are the simplified models that teachers use to explain concepts such as: Maxwell’s equations, the nature of light, and the structure of the atom. Like pedagogical content knowledge, pedagogical models also involve a transformative process whereby depth, rigor, and complexity are sacrificed for the sake of basic understanding.

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[48] 48. Lee Shulman, “Knowledge and Teaching: Foundations of the New Reform,” *Harvard Educational Review*, 57, no.1 (1987): 8.

[49] 49. Lee Shulman, “Those Who Understand: Knowledge Growth in Teaching,” *Educational Researcher* 15, no. 2 (1986)” 6.

Those familiar with constuctivism in science education realize that the transformation from scientific models to pedagogical models is only an intermediate stage in a larger and much more complex network of students' scientific knowledge construction.[50] The transformation process begins with the scientist's own conception of her scientific model and her external representation of it in some kind of physical form (e.g., peer-reviewed publication, conference presentation, etc.). This external representation comes to be the scientific model per se. It is the model that subsequent textbook authors look upon when writing their textbooks. However, even the seemingly innocuous expression of a scientific model in a textbook already involves some kind of knowledge transformation because the textbook author must not only interpret the expressed scientific model, she must also represent it in a manner appropriate to the students who will be employing the textbook. Here is where we first see teachers enter the picture. They are supposed to act as mediators between the models that students see in their textbooks and the students themselves. In order to mediate, however, the teacher must have a firm understanding of the model used in the textbook. To this end, the teacher will reference several sources (e.g., the teacher's edition of the textbook, various websites, material collected at professional development training, and other media) to develop her own understanding of the model, which may or may not directly correlate to the textbook author's conception. The next stage is where pedagogical content knowledge is created. After the teacher has done her due diligence and is finally satisfied with her understanding of how a particular model is presented in a textbook, she will then transform her understanding into a lesson plan conducive for all of her students, including

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[50] Keith S. Taber , "Towards a Curricular Model of the Nature of Science," *Science & Education* 17, no. 2 – 3 (2008):185.

Special Education students and English Language Learners (ELLs). Even though the textbook authors have already simplified their models for a student audience, veteran teachers know that these modifications are not enough and will require even further simplification. But knowledge transformation, or knowledge construction if you like, does not end there. Students do not passively receive models, no matter how simplified their textbooks and teachers make them. A student's background assumptions, as well as their prior knowledge, will both inevitably affect how they come to understand a model. In the end, a student's conception of a particular scientific model will be an amalgamation of: the scientist's expression of her model to the public, the textbook author's interpretation of the scientific model, the textbook author's simplification of the scientific model, the teacher's interpretation of the model as it appears in the textbook, the teacher's further simplification of the model in the textbook, a student's previous academic knowledge, in addition to any knowledge that the student brings with her from her cultural background.

The utter complexity of this system of knowledge transformation and acquisition is why I have been reluctant to support the almost ubiquitous view that models are representations. When we consider the process above, the only times we deal directly with scientific models are when the scientist is working out the scientific model in her head and when she makes the model available to the public, every other manifestation of it is a pedagogical model that is modified and simplified for the sake of creating pedagogical content knowledge. A physicist's conception of the atom is not the same as a textbook author's, a teacher's, or a student's. This is not to say that the physicist's conception is superior or ‘closer to the truth,’ but at the same time the two conceptions are so different that each might as well be “responding to a different world.”[51]

5.3.4 Hybrid Models

One unique kind of pedagogical model that Justi and Gilbert discuss is called a hybrid model. A hybrid model is “constituted of elements of different historical models treated as if they constituted a coherent whole.”[52] In other words, a hybrid model is simply a combination of several different pedagogical models put together to create a single model. Justi came across the concept of hybrid models when she was exploring how different science textbooks in Brazil represented the atom. To her surprise, one particular textbook depicted the atom as having both an electron orbit and an electron cloud in the same model. According to Justi, the problem with this dual representation is that students come to believe that “there is an absolute and unchanging conception of the atom” and “if a given model is changed by another scientist, the scientist who had proposed that model had made experimental mistakes.”[53] In a second article on hybrid models, but this time on models of chemical kinetics, Justi and Gilbert specify their criticism of hybrid models by stating that they are *not* suggesting that teachers and textbooks “should present a linear progression of historical models of a given subject.”[54] Rather, they should “make the backgrounds of their expressed models clear. They should

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51. Thomas S. Kuhh, *The Structure of Scientific Revolutions*, 3rd ed. (Chicago: The University of Chicago Press, 1996), 111.

52. Rosaria Justi and John Gilbert, “A Cause of Ahistorical Science Teaching: Use of Hybrid Models,” in *Constructing Worlds through Science Education: The Selected Works of John K. Gilbert*, ed. John K. Gilbert (New York: Routledge, 2005), 55.

53. Rosaria Justi and John Gilbert “History and Philosophy of Science Through Models: Some Challenges in the Case of ‘the Atom’,” *International Journal of Science Education* 22, no. 9 (2000): 1005.

54. Justi and Gilbert, “A Cause of Ahistorical Science Teaching,” 55.

state the context in which they are valid.” [55] Meaning, Just and Gilbert do not believe that textbooks should adhere to the principle of verisimilitude when presenting different models of the atom, whereby each successive model is considered to be “more true” or “closer to reality” than its predecessor. Instead, they want textbooks to carefully articulate the transition from one model to the next, explaining why certain properties get replaced while others are retained. Just and Gilbert are optimistic that the inclusion of such textbook explanations will encourage teachers to discuss the nuanced process of how a model emerges from empirical data, including the role that interpretation plays in the process.[56]

While I sympathize with Justi and Gilbert’s desire to inform students about how scientists develop scientific models, the hybrid models under examination are pedagogical models in school textbooks, which means their primary purpose is to teach scientific concepts. The hybrid model of the atom that Justi and Gilbert discussed was intended to teach physics students about the composition of the atom, specifically the behavior of the electron(s) within it. Given the ambiguous nature of the atomic hybrid model discussed above, one would not be surprised if students walked away with more questions than answers (Do electrons behave like orbits, a cloud, or both?). That being said, I maintain that Justi and Gilbert’s condemnation of the hybrid model is premature. I say this because we do not know the intentions of the textbook authors or how the classroom teachers integrated the models into their lesson plans. If the authors intended to portray the true nature of the atom, then their pedagogical model was a poor one indeed. However, what if their intentions were more humble? It is entirely possible that they

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[55] Ibid.

[56] Justi and Gilbert, “History and Philosophy of Science Through Models,” 1005.

would be content if their readers walked away knowing the difference between an electron orbit and an electron orbital (i.e. electron cloud) and that the electron behaves both ways. If students are able to use the hybrid model to retain this particular content knowledge it must be considered a pedagogical success regardless of how poorly it represents its target and how invaluable it is for practicing scientists. This compromise is an inevitable part of the Faustian bargain educators make when they transform scientific models into pedagogical models: as models become more student-friendly, they become more unrecognizable to the very scientists who developed them in the first place.

Cesar Delgado is correct that one of the most daunting problems facing teachers who employ models on an everyday basis is navigating the tension between conceptual and metaconceptual goals.[57] Conceptual goals have to do with content knowledge, while metaconcpetual goals concern broader issues in the history, philosophy, and sociology of science. In particular, Delgado believes that pedagogical models are susceptible to several metaconceptual problems, chief among them being "epistemological overreach."[58] Epistemological overreach is committed when a model implies much more certainty and knowledge than is actually warranted.[59] For example, a thin-line curve of Boyle's Law suggests an exact mathematical relationship between pressure and volume when in reality the actual data points collected by ecologists are messy, scattered, and stochastically varied.[60]

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[57] Cesar Delgado, "Navigating Tensions Between Conceptual and Metaconceptual Goals in the Use of Models," *Journal of Science Education and Technology* 24, no. 2 – 3 (2015): 132.

[58] Delgado, "Navigating Tensions," 135.

[59] Ibid., 139.

[60] Ibid., 139, 143.

At this point the science teacher has a quandary. If she presents the graph of Boyle's Law as it is usually presented in science textbooks, she will be guilty of epistemological overreach. On the other hand, if she informs her students that the thin-line curve of Boyle's Law is only a graph under ideal (i.e. fictional) circumstances and does not exist anywhere in the real world, she may risk confusing her students (or even worse, alienating them). AAAS suggest that only high school students and above should be introduced to the abstract models presented in computer simulations and mathematical models because of their more advanced cognitive ability, but as Grosslight et al's study has suggested, 11th grade honor students are for the most part still naïve realists who have an unsophisticated understanding of the relationship between models and reality.[61] And even if students at this age begin to show signs of metapconceptual awareness, the effects on the retention of basic content knowledge could be deleterious.

5.3.5 Stages of Model Complexity

One commonplace assertion regarding how models should be taught in school is that the level of model complexity should increase as a student progresses from elementary school all the way to high school (and for many, college). Complexity in this sense simply means abstraction. Students at the elementary level should be introduced to tangible, physical models such as model cars and trains, at the middle school level they should become acquainted with more abstract models such computer simulations of birds' flocking patterns, and at the high school level and above they should familiarize themselves with purely abstract, mathematical models such as Maxwell's equations.[62]

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[61] Grosslight, Unger, and Jay, "Understanding Models," 819.

Now I do not disagree with either the psychological foundation of the recommendations or with the recommendations themselves. My problem is with the implicit assumption that as a student's model knowledge becomes more and more abstract; her model knowledge will bear a closer resemblance to the scientist's model knowledge. In other words, there comes a point where a student transcends the use of pedagogical models and begins to employ scientific models, the models that scientists themselves use. This is certainly erroneous. High school students using the law of gravitation to calculate the force between two bodies look like they are doing what physicists are doing, but they are not because the law of gravitation that high school students use is a *ceteris pariubus* law that "can explain in only very simple, or ideal, circumstances. It can account for why the force is as it is when just gravity is at work; but it is of no help for cases in which both gravity and electricity matter. Once the *ceteris paribus* modifier has been attached, the law of gravity is irrelevant to the more complex and interesting situations."[63] Research scientists have to deal with these "complex and interesting situations" on an everyday basis, but high school students (and even most undergraduates) do not. The quicker we come to the realization that pedagogical models are created for educational purposes and scientific models for research purposes, the less time we will waste trying to transform classrooms into miniature research laboratories.

The recent mantra in science education has been that students will learn science by doing science. Which leads me to the obvious question, "Is school science even science?" My reply is that students who are doing school science are doing school

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[62] American Association for the Advancement of Science, "Benchmarks for Science Literacy," 268 – 270.

[63] Nancy Cartwright, *How the Laws of Physics Lie* (New York: Clarendon Press, 1986), p, 58.

science, but not cutting-edge, research science. As Keith S. Taber points out (citing Richard Feynman), "even the most respected physicist may find it a difficult task to explain a physicist's notion of energy to the layman".[64] In my estimation, the science education community needs to make peace with the scientific model/pedagogical model distinction once and for all. Only then can it construct curriculums that are ontologically, epistemically, and "intellectually honest."[65]

5.4 Conclusion

This chapter explored teachers' and students' ontological and epistemic attitudes towards models and modeling. Unfortunately, the literature suggests that both their ontological and epistemic views are lamentably naïve in that they believe that models are supposed to be mirrors of nature and provide true explanations of physical reality. (The reader will recall that chapters 2 – 4 of this study have gone a long way in demonstrating the shortcomings of both of these assertions.) Later in the chapter, I diagnosed the root of the problem: a failure to distinguish between scientific models and pedagogical models, or the models scientists use and the models teachers use. So long as teachers and students see themselves as professional scientists-in-training, doing what real scientists do, they are going to adhere to their simple-minded ontological and epistemic attitudes.

The following chapter is a natural continuation of the present one. While this chapter discussed teachers' and students' attitudes towards modeling, the proceeding chapter explores three model-based lessons that have already been implemented in secondary science and engineering classrooms. By exploring the lessons themselves, we

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[64] Taber , "Towards a Curricular Model," 190.

[65] Ibid., 204.

can discern if teachers' thinking about models has become more sophisticated, or if they are simply perpetuating the same naïve ontological and epistemic sentiments.

Chapter 6

Three Case Studies on Modeling

6.1 Introduction

The following chapter discusses three examples of modeling lessons that have been applied in the classroom. Case study 1 comes from mathematics, case study 2 from engineering, and case study 3 comes from biology. My discussion of each case study includes: (1) a detailed description of the lesson, (2) an in depth analysis where I provide critique, praise, or both, (3) and a brief conclusion that suggests possible ways to improve the modeling lesson under review. At the end of the chapter I return to the issue of mathematical models and explain why our approach to them should be one of caution rather than wholehearted commitment.

6.2 Mathematical Modeling Case Study

6.2.1 What is Mathematical Modeling?

According to the American Association for the Advancement of Science (AAAS), "The basic idea of mathematical modeling is to find a mathematical relationship that behaves in the same way the system of interest does."[1] The system in question can be anything from an abstract system in mathematics, physics, and economics to a physical system in biology, geology, and engineering.[2] Consider Boyle's Law, $p \alpha 1/v$, where p is the pressure of the gas, α means "proportional to," and v is the volume of the gas. According to Boyle's Law, there is a direct relationship between pressure and volume

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[1] American Association for the Advancement of Science, *Science for All Americans* (New York: Oxford University Press, 1990), 171.

[2] American Association for the Advancement of Science, *Science for All Americans*, 171.

such that as the pressure of a gas increases, the volume of its container decreases. This means that scientists do not need to continuously increase the pressure of a gas and measure its volume to know that the volume is going to decrease because the mathematical model (Boyle's Law) already provides us with this information. Mathematical models play an essential role in scientific investigation in that they allow scientists to make approximations regarding how a system might or might not behave, which is especially useful in those sciences where empirical data is notoriously lacking (e.g. cosmology).

6.2.2 Description

In "Using the Scientific Method to Engage Mathematical Modeling: An Investigation of pi," Lester Archer and Karen Ng develop an experimental activity ambitiously attempting to engage 6th and 7th graders in mathematical modeling via the 'scientific method,' The suggested activity is intended to get students to realize the close relationship that exists between science and mathematics. On the one hand, most (if not all) scientific explanations can be reduced to mathematical explanations. On the other hand, mathematical discoveries might be aided by duplicating the processes and methods used in many scientific discoveries. The article under consideration only concerns itself with the second half of the science-mathematics relationship; namely, how science can be of benefit to mathematics.

The mathematical modeling activity selected by Lester and Ng focuses on the relationship between the circumference and diameter of various, everyday circular objects. The envisioned activity is simple enough. First, a group of students will measure the diameter of a penny using a ruler. Next, they will record their observation on a data table. They will repeat these steps two more times, ending up with a total of three measurements for the penny's diameter. Next, the students will take the three measurements and calculate the penny's average diameter and record it on the data table. After measuring the diameter of the penny, the students will move on to measuring its circumference. In order to do this, the students will either use a tape measure or a piece of string. The students will do this a total of three times and record their results on the data table. After all three circumference measurements have been taken, they will calculate the average circumference of the penny and record it on the data table. Once the students have calculated both the penny's average diameter and circumference, they will be asked to find the ratio between the two. The calculated ratio will then be recorded onto the data table. This process will be repeated for several more circular objects, including a: hula hoop, plate, CD, and cookie.

The authors suggest that this activity can be performed in at least two different ways. First, students can be explicitly informed at the outset of the activity that the relationship between a circular object's diameter and circumference should remain constant. In other words, for each circular object measured, the diameter-circumference ratio should be one and the same. By providing students with this information (what the authors call the hypothesis), it becomes the students' task to either support it or disprove it with their recorded data; hence, the authors' belief that the application of the 'scientific method' supports students' mathematical modeling abilities.

For the most part, the alternative version of the activity is one and the same, with the exception that students will not be privy to the diameter-circumference hypothesis. The hope is that they will discover the consistency of the ratio all by themselves, without any prompting. The variation of the first activity is supposed to encourage mathematical discovery through mathematical modeling. The point is not so much to support or disprove a proposed hypothesis, as it is to discover mathematical relationships between variables all on one's own.

6.2.3 Analysis

While it is admirable that Archer and Ng's attempt to integrate science and mathematics in a classroom activity, at the same time it is difficult to avoid the elephant in the room: their commitment to the anachronistic 'scientific method'. Although the authors acknowledge that the 'scientific method' is not at all comprehensive when it comes to answering questions and understanding about the world, they nevertheless maintain that it "is a good approximation to use."[3] But for whom is it a good approximation? According to the traditional 'scientific method,' a scientist makes some initial observations, develops a question from those observations, proposes a hypothesis that answers the question, formulates an experiment to test the hypothesis, performs the experiment, records data from the experiment, compares the data with the proposed hypothesis, and either concludes that the data supports the hypothesis or that it does not support it. If it is the case that the data does not support the hypothesis, then a new hypothesis is proposed and the cycle begins anew. However, one glaring problem with the traditional 'scientific method' is that it heavily favors the empirical sciences at the expense of the theoretical sciences. According to the 'scientific method,' if a theory and

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3. L.A.C Archer and K.E. Ng, "Using the Scientific Method to Engage Mathematical Modeling: An Investigation of Pi," *Journal of Education in Science, Environment and Health* 2, no. 1 (2016): 53.

an experiment disagree, the experiment overrides the theory.[4] But this is not how a theorist (e.g. cosmologist) thinks. The theorist believes in the theory despite contrary experimental evidence so long as it is a fruitful area of research capable of producing more and more theories. Quoting eminent physicist Sir Arthur Eddington, "it is also a good rule not to put overmuch confidence in observational results that are put forward until they have been confirmed by theory."[5] The point is that each thinks that the other is misled because they each have their own idiosyncratic view of what science is and how science should be done. In the end, there is no one 'scientific method' that all the sciences use; instead "there are many different sorts of science"[6] that rely on all different sorts of methods.

Furthermore, the idea of a single 'scientific method' promulgates the idea that all scientific discoveries are rational, orderly, and systematic. But this is far from the case. Take, for instance, what Nima Arkani-Hamed, a physicist at the Institute for Advanced Study in Princeton, had to say about the future of supersymmetry, the extension of the Standard Model of particle physics that predicts a partner particle for each particle in the Standard Model.[7] While delivering a talk at the Kavli Institute for Theoretical Physics at the University of California, Santa Barbara, Arkani-Hamed asked rhetorically to the audience, "What if supersymmetry is not found at the LHC (Large Hadron Collider)…then we will make new supersymmetry models that put the superpartners just

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[4] Henry H. Bauer, *Scientific Literacy and the Myth of the Scientific Method* (Urbana: University of Illinois Press, 1992), 21.

[5] Bauer, *Scientific Literacy*, 23.

[6] Ibid, 28.

[7] "Supersymmetry," CERN, published January 20, 2012, accessed April 2, 2017, https://home.cern/about/physics/supersymmetry.

beyond the reach of the experiments."[8] Consider for a moment this statement by Dr. Arkani-Hamed. He is implying that lack of experimental evidence for supersymmetry does not necessarily spell doom for supersymmetry because all that needs to be done is to formulate a model of supersymmetry that is incapable of being disproved by any of the experiments currently being performed at the LHC. Needless to say, Arkani-Hamed's methodology does not match the 'scientific method' advocated by Archer, Ng, and every primary and secondary school science textbook written before the year 2000. But wait, the article gets even more interesting. Arkani-Hamed continues, "But wouldn't we be changing our story? That's okay; theorists don't need to be consistent—only their theories do."[9] At the very heart of the 'scientific method' is the notion that theories must be supported by empirical or experimental evidence. This anecdote from particle physics suggests that for some scientists, inter-theoretic consistency is more desirable than theory-evidence consistency. In the words of Henry Bauer, "Scientific research is a medley of all sorts of attempts to gain new knowledge…by cutting corners; by doing "quick-and-dirty" experiments, not just carefully systematic ones; by following hunches or "just playing around."[10] It is unclear what the future holds for supersymmetry, but one thing is certain: those who support it will remain undeterred despite lack of empirical evidence.

Next, the authors' dedication to the 'scientific method' stifles the creativity of the students to the point that one can question whether or not they are actually participating

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[8] Joseph Lykken and Maria Spiropulu, "Supersymmetry and the Crisis in Physics," *Scientific American*, May 2014, 38.

[9] Lykken and Spiropulu, "Supersymmetry," 38.

[10] Bauer, *Scientific Literacy*," 46.

in a modeling activity in the first place. According to the Standard of Mathematical Practice 4 (MP4): Model with Mathematics, mathematical modeling includes both "identifying variables" and "formulating models that describe relationships between those variables."[11] In the activity presented, students neither identify their own variables, nor formulate their own mathematical models of those variables. From the outset, students are told that the variables they will be working with are diameter and circumference, and that their primary objective is to find the relationship between the two. Regarding the former, it is incomprehensible why the students are not allowed to discover the variables on their own. Why not give the students a ruler, string, and tape measure and provide them the freedom to identify the midpoint, radius, diameter, circumference, and area all by themselves? Why tell them what to measure and how to measure it in the first place? The activity's focus is clearly on having students *measure* predetermined variables rather than identifying them on their own; unfortunately, the simple act of measuring is not tantamount to modeling.

As for the activity's role in helping students formulate mathematical models between variables, the activity is similarly ineffective for the same reasons stated above. Authentic mathematical modeling involves "playing" with variables until a meaningful relationship is discovered. By telling students that there is a relationship between the diameter and circumference, and by further informing them that the relationship can be found by finding the ratio between the two, the aforementioned activity deprives students of the joys of mathematical exploration. There is no harm in allowing students to find out for themselves that there is a relationship between: radius, diameter, pi, circumference, and area. Even if the only relationship a student recognizes is that for all circles $2r = d$,

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[11] Archer and Ng, "Using the Scientific Method," 52.

she is still discovering a mathematical model on her own with minimal prompting from her teacher. I contend that this is the kind of mathematical modeling that teachers should encourage in their classroom, the kind that engenders creativity, exploration, and imagination. Mary Hesse says the following about scientific discovery but it can also be applied to mathematical discovery: "there can be no set of rules given for the procedure of scientific discovery…it is a product of creative imagination, of a mind which absorbs the experimental data until it sees them fall into a pattern."[12] By combining their activity with the 'scientific method,' Archer and Ng transform what could be a genuine modeling activity into a banal exercise of measuring and dividing.

According to the integrated account of modeling, a model is a temporary collection of integrated elements whose purpose is to contribute to human knowledge by helping scientists solve different kinds of scientific problems. Although some of the elements in the integrated account include the traditional components of the 'scientific method,' the integrated account maintains that scientific modeling, and the scientific endeavor in general, is much more robust and dynamic than simply proposing a hypothesis, testing it, and reconciling the hypothesis with experimental data. If anything, the integrated account scoffs at the idea that there is one universal method that all scientists employ. The fact that: theory-ladenness of knowledge, metaphysical assumptions, access to funding, interpretation, intuition (or "playing a hunch"), analogical reasoning, the historical development of a scientific field, and luck all play a crucial role in scientific discovery and justification strongly suggests that scientists themselves do not even follow the infamous method attributed to them.

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[12] Mary Hesse, as quoted in, Daniela M. Bailer-Jones, *Scientific Models in Philosophy of Science* (Pittsburgh: University of Pittsburgh Press, 2009), 101.

The ‘scientific method’ is intended to be a pedagogical model that teachers can use to explain to their students what science is, how it works, and how to do it. The problem with transforming any scientific model into a pedagogical model, however, is that certain attributes of the former are bound to be left out of the latter. In the case of the ‘scientific method’ model, the vicissitudes of scientific discovery and justification are sacrificed for the sake of simplicity and student understanding. However, if a pedagogical model no longer resembles the scientific model it was intended to represent in the first place, then we have to consider whether or not the Faustian bargain was worth it.

6.2.4 Conclusion

If the problem with this activity is that it is too restrictive, the solution is to allow students the freedom to think on their own. When it comes to measuring the circumference of a circle, the teacher should not prompt the students how to do it; instead, the teacher should make explicit all of the measuring instruments available to them and allow them to make the choice of what instruments they want to use and how they are going to use them. The same suggestion applies when having students calculate the ratio between the circumference of a circle and the diameter—it needs to be student-centric. This means that students should not even be told that there is a relationship between the two in the first place; they should be allowed to apply their critical thinking skills and discover the relationship entirely on their own. Students are going to take several wrong turns and arrive at many dead ends, but this is part of the modeling process. Even Kepler, Galileo, Newton, Maxwell, and Einstein did not get their mathematical models right the first time around.

I applaud Archer and Ng for introducing students to mathematical modeling and the kinesthetic approach they have adopted. However, I would like to see the objectives and methods of their modeling activities radically altered so that students are *building* mathematical models rather than simply calculating them.

6.3 Engineering Case Study

6.3.1 Description

In “High School Student Modeling in the Engineering Design Process,” Nathan Mentzer, Tanner Huffman, and Hilde Thayer are interested in how and in what ways high school students are modeling when presented with an engineering design challenge.[13] In particular, the authors challenged students to design a neighborhood playground (75 ft. x 75 ft.) in a mid-size city on a donated city block. Some of the parameters of the playground were: it had to be safe, it had to remain outside all year long, it could not cost too much, and it had to comply with the Americans with Disabilities Act, as well as any zoning laws.[14] Furthermore, all playground equipment had to be made from scratch from materials available at any local hardware and/or lumber store. Participants had exactly three hours to complete their model(s). As for materials, students were only allowed to use basic office supplies such as: paper, pencils, pens, rulers, etc. during the challenge.

After the students completed the design challenge, the authors coded the student models five different ways. The models were classified as: (1) employing mathematical explanations, (2) employing mathematical descriptions, (3) graphical, (4) physical, or (5)

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[13] 13. Nathan Mentzer, Tanner Huffman, and Hilde Thayer, “High School Student Modeling in the Engineering Design Process,” *International Journal of Technology and Design Education* 24, no. 3 (2014), 294.

[14] 14. Mentzer, Huffman, and Thayer, “High School Student Modeling,” 296 – 297.

other.[15] By mathematical explanations, the authors simply mean mathematical models. If students used any kind of mathematical relationship, such as the Pythagorean Theorem, they were coded as practicing mathematical modeling. Mathematical description, on the other hand, implies that students used numbers in their models. For example, if students merely wrote down the dimension of the playground on a piece of paper, they were coded as providing a mathematical description. The difference between a mathematical explanation and a mathematical description is that a mathematical explanation expects students to use mathematical equations and functions, whereas a mathematical description requires that they use numbers in any way. Models were coded as graphical if they used anything written such as: drawings, lists, or notes. If any of the models used any kind of physical object, such as a ruler, to represent a piece of playground equipment, it was coded as physical. Finally, models coded as "other" did not employ any of the modeling strategies mentioned above; rather, the students verbally discussed their models without any attempt of mathematical, graphical, or physical representation.

The results of the design challenge severely disappointed Mentzer, Huffman, and Thayer. Whereas the authors expected many of the participants to employ graphical descriptions, they were shocked by the minimal amount of time students devoted to mathematical explanations (i.e. mathematical modeling).[16] This result was especially disheartening given the amount of mathematical modeling involved in engineering. Every building, bridge, plane, car, space shuttle etc. is the product of a combination of complex mathematical modeling, computer simulations, and real world testing. The fact that high school engineering students do not know ho w endemic mathematical modeling is to

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[15] Ibid., 299 – 300.

[16] Ibid., 301.

engineering makes the authors seriously question if aspiring engineers truly understand what engineers actually do. The biggest challenge for these students is making the transition from just creating graphical and physical models to also creating abstract, mathematical models. The authors believe that in order for high school students to recognize the importance of mathematical modeling to engineering, they need to be consistently exposed to some real world applications of mathematical modeling throughout their K-12 education.

6.3.2 Analysis

The engineering case study presented by Mentzer, Huffman, and Thayer was interesting in the sense that the authors gave the participants an open-ended problem and left them to their own devices to arrive at a solution, thus encouraging the students to use their own creativity and imagination throughout the entire modeling process. Unfortunately, this is only partially the case. Although the students were encouraged to create their own models for the design challenge, there was one type of model that the authors admittedly discouraged: physical models. According to the authors, "*Students did not have an opportunity to construct physical models or prototypes*"[17] because "*prototyping materials were not included*."[18] In my view, this is a major omission that considerably weakens the reliability of the case study. The intentional attempt to suppress physical modeling begs an important question; namely, why? Why did the authors seemingly encourage mathematical and graphical modeling, but not physical modeling? According to the text quoted above, it is because "prototyping materials were not

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[17] Ibid., 297.

[18] Ibid., 300.

included.” But surely this is not an acceptable answer. Students could have easily been supplied with any combination of the following: Legos, blocks, play dough, construction paper, scissors, and glue. So it is not so much that the authors of the study could not supply the students with materials to construct prototypes, what is becoming more apparent is that they did not want to provide them with the materials in the first place. Which begs the question again, why? One possible answer might have to do with time constraints. Maybe the authors believed that students would use all of their time building physical models and not leave any time to explore other types of model constructions. This is plausible, but if Mentzer, Huffman, and Thayer were only worried about time constraints, then they would have said as much when describing the limitations of their study. I maintain that neither limited availability of resources, nor time constraints were their primary concern. Instead, I believe that the authors suspected that if they allowed students to build physical models, they (the students) would become so preoccupied with constructing prototypes to the point that mathematical models would all but be neglected. Remember that even though the students were presented with an open-ended design challenge, the real purpose of the study was to see how many students incorporated mathematical functions and equations in their model constructions. Hence the authors’ dilemma: if they discouraged students from building prototypes, then their design challenge would not be completely open; on the other hand, if they allowed students to build prototypes, they would most likely gather very little, if any, data concerning mathematical modeling. In the end, the authors discouraged the construction of physical models because the prospect of having an insufficient sample size was far too discomfiting.

Some might argue that I am making a mountain out of a molehill, but I truly believe that the design challenge's failure to include prototypes slanders physical modeling and its contributions to the acquisition, development, and cultivation of both knowledge and technology. Consider the problem of flight. In particular, consider the primary difficulty that faced all early flying machines: control. Once an aircraft took flight, how was the pilot going to steer it? The answer came to Wilbur Wright when he reflected on the only entities capable of sustained flight at the time: birds. Wright suspected that a bird "adjusted the tips of its wings…turning itself into an animated windmill."[19] He received an early confirmation of his hypothesis when he created a kite out of a cardboard box and designed it to behave like a bird's wing.[20] "The construction of a small model to validate the general idea of wing-warping was the next step, followed by the building of a full-scale model".[21] The construction of these successively complex kite models were the first successful prototypes of controlled, unmanned flight.

I included this brief example from the history of aviation to reinforce the limitless possibilities of physical models. In this particular example, Wright's physical models were so productive because they allowed him to participate in what Adam Toon calls "games of make-believe."[22] According to Toon, physical modelers not only imagine the properties of the thing being modeled, they also imagine themselves physically manipulating that which is being represented. Thus, when Wilbur Wright was twisting

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[19] Susan G. Sterrett, *Wittgenstein Flies a Kite: A Story of Models of Wings and Models of the World* (New York: Pi Press, 2006), 59.

[20] Sterrett, *Wittgenstein Flies a Kite*, 60.

[21] Ibid.

[22] Adam Toon, "Playing With Molecules," *Studies in the History and Philosophy of Science Part A* 42, no. 4 (2011): 587.

and bending the inner-tube box to create his earliest prototype, he was actually imagining himself holding a bird's wing and fashioning it to resemble what a bird's wing looked like during mid-flight. What makes this kind of "imagined experiment" unique to physical modeling is its exploitation and dependence on our sense of touch.[23] Graphical models, mathematical models, and even computer simulations do not allow us to use our sense of touch in this productive way.[24] This means that no matter how complex our technology gets, there seems to always be a place for models made out of cardboard boxes, popsicle sticks, and foam spheres.

By speaking on the behalf of physical models, I am in no way denigrating the significance of mathematical models, especially when it comes to engineering. What is frustrating about the design challenge in particular is that the authors did not even consider the possibility that physical modeling could encourage mathematical modeling. While discussing their results, Mentzer, Huffman, and Thayer discovered that "graphical modeling often preceded mathematical modeling."[25] The same could be said of physical modeling. Consider Andrea, the study's exemplar when it came to the application of mathematical modeling. When asked what she thought were the most important factors to consider during the design process, one of her responses was "prototyping."[26] This is especially poignant coming from Andrea, the design challenge standout. One can only imagine what kind of mathematical models she would have developed if she was given the opportunity to construct physical models alongside her graphical ones. Whether we

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[23] Toon, "Playing With Molecules," 587.

[24] Ibid., 588.

[25] Mentzer, Huffman, and Thayer, "High School Student Modeling ," 301.

[26] Ibid., 304.

are discussing James Clerk Maxwell, Wilbur Wright, or James Watson, physical models are never too far removed from their mathematical counterparts. In most cases the two go hand-in-hand; unfortunately the architects of the engineering design challenge failed to capitalize on this all too obvious association.

6.3.3 Conclusion

The integrated account of modeling subscribes to the notion that different kinds of models are useful for different kinds of purposes. More often than not, scientists use a combination of graphical, physical, and mathematical models to develop and justify their theories. Even though Mentzer, Huffman, and Thayer were primarily concerned with how high school students developed mathematical models, their study should not have been carried out at the expense of physical models. Any reasonable engineering design challenge should allow students the freedom to explore the kinds of models they want to construct. Now it will likely be the case that most of the student models will end up being deficient, but this is where teachers have the opportunity to introduce pedagogical models to supplement any incomplete learning.

6.4 Biology Case Study

6.4.1 Description

In “Systems Modelling and the Development of Coherent Understanding of Cell Biology,” Roald P. Verhoeff, Arend Jan Wasario, and Kerst Th. Boersma developed a teaching and learning strategy (from now on T-L strategy) in cell biology primarily based on modeling. The T-L strategy created by Verhoeff et al. subjected upper secondary students to four distinct modeling stages, with each stage acting as an epistemic scaffold for the previous one. First, students were expected to learn how an independent cell functioned, then they studied how living (i.e. plant and animal) cells functioned with one another, and finally they examined the role that cells played at the organ and organism level. Although students had difficulty jumping back and forth between the cell, organ, and organism levels (stage 4), the T-L strategy nevertheless increased their understanding of cell biology, in particular, the different parts of the cell and how each part interacts with one another to produce a functioning whole.

As noted above, the foundation of Verhoeff et al.'s study was the development of a four stage cell biology modeling strategy. What distinguished the authors' approach from other modeling activities was their emphasis on differentiated and hierarchical modeling. In particular, the authors suggested the following. First, different kinds of models (e.g. 2D, 3D, and computer simulation) should be used at each of the different stages of the activity; and second, the models being created and studied should increase in complexity parallel to the complexity of the biological system being studied. In other words, if the system being studied was simple, then the model created for that particular system should likewise be simple.

At this point I will briefly outline each of the modeling stages presented by Verhoeff et al.'s systems modeling T-L strategy.

Stage 1: Developing a Model of Free-living Cells

During the initial stage of the systems modeling T-L strategy, students observed free living Paramecia cells under a microscope in order to develop a model for unicellular and multicellular organisms.[27] In particular, students learned how cells carry out basic life processes such as: feeding, breathing, growing, regeneration, excretion, and self-protection.[28] To aid in their understanding of the cells and how they function, the students were also introduced to the work of Antoine van Leeuwenhock, who made analogies between how cells function and how familiar organisms function.[29] The students demonstrated their acquired knowledge of free living cells by creating rudimentary 2D drawings of some of the aforementioned functions.

Stage 2: Developing a General 2D Model of Cells

In stage 2, students moved on from studying free-living cells to cells that were part of an organism (e.g. plant and animal cells). Students began by observing plant and animal cells using a light microscope, but quickly discovered that they could hardly discern any of the cells' different organelles. As a result, they transitioned to using electron microscopes instead, which provided higher magnification powers. The students were able to recognize larger organelles such as mitochondria and chloroplast, but had difficulty when it came to identifying organelles that were not clear and round. To move passed this difficulty, the teacher introduced them to a "textbook" (my phrase) model of the cell, in which all of the cell's organelles were clearly depicted and labeled. By comparing the "textbook" model with their initial electron microscope observations, the students were able to develop a more coherent understanding of the cell.

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27. Roald P. Verhoeff, Arend Jan Wasario, and Kerst Th. Boersma, "Systems Modelling and the Development of Coherent Understanding of Cell Biology," *International Journal of Science Education* 30, no. 4 (March 2008): 552.

28. Verhoeff, Wasario, and Boersma, "Systems Modelling," 552.

29. Ibid.

It should be pointed out that stage 2 did not require students to produce a 2D model of a plant or animal cell like they did in stage 1. The authors do not specify why a student 2D model was produced in stage 1, but not stage 2, but I suspect that the answer has to do with context. Because stage 1 students did not have difficulty understanding free-living cells, it was appropriate for them to produce a model of it to demonstrate their understanding. This was not the case when the students transitioned to studying plant and animal cells. Unlike free-living cells, the organelles in these cells were much more difficult to identify regardless of the kind of microscope they were using. As a result, the teacher must have believed it more appropriate to use a "textbook" 2D model to *support* his students understanding, rather than have them produce one for themselves.

Stage 3: Developing a 3D Model of a Plant Cell

Using the knowledge they acquired during stages 1 and 2, the students were subsequently given the task to create a 3D model of a plant cell. To accomplish this task, the students were separated into pairs of two, with each pair given the responsibility to create a 3D model of a particular plant organelle. Once each separate model organelle was created, each pair would come together and combine their individual organelles to create a single 3D model of a plant cell.

To construct their models, each pair used a variety of resources at their disposal including: their textbook, other biology books, and the Internet. Rather than simply adopting the first model they came across, the students went "back and forth between the different cell representations"[30] and eventually had to agree on which model(s) they were going to eventually use.

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[30] Ibid., 555.

Although it is conceivable that each individual pair could have worked in isolation from one another until the final model was created, this is in fact the opposite of what actually transpired. Rather than separating themselves from one another, the pairs worked more closely than ever, helping one other understand how their particular organelle affected the functions of other organelles within the system. One might go so far as to say that the students were apparently mimicking the behavior of real life plant organelles by how closely they were working together. For according to the study's authors, "Sometimes this cooperation became a kind of role-play in which students identified themselves with their organelles."[31]

Step 4: Modeling Representation at the Organism, Organ, and Cellular Level to a General Systems Model

The final stage of the study ended up being the most challenging for the students. Whereas stages 1 – 3 introduced students to free-living cells, plant cells, and animal cells, stage 4 required them to consider the larger role that cells played in various organs and organisms. To accomplish this difficult task, students were allowed to participate in an interactive computer simulation that guided them through the process of human digestion beginning at the cellular level all the way up to the organism level.[32] The final assessment for the simulation required the participants to not only describe cellular activity at the cell, organ, and organism levels, but also the interaction between all of the levels. Unsurprisingly, the final part of the assessment is where the students demonstrated the most difficulty.

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31. Ibid., 556.

32. Ibid., 557.

To further explore their idea of a “hieracrchical open-system model,” the authors challenged the students to apply the previous model to an all together different human function, breast-feeding. Once again, the students were rather comfortable investigating each level on its own (i.e. cellular, organ, and organism), but encountered difficulty when required to explain the role of individual organelles at the organ and organism level. At this point, teacher intervention was required to help students understand that what was occurring at the organism level was likely a function of organelle activity at the cellular level. With their teacher’s guidance, the students were eventually able to conceive a hierarchical model of breast-feeding at the cellular, organ, and organism levels. To demonstrate their knowledge, the students created a complex 2D chart that exhibited the many nuances of this complex relationship.

6.4.2 Analysis

The proceeding analysis of Verhoeff et al.’s “Systems Modelling and the Development of Coherent Understanding of Cell Biology” is going to take place in the context of the National Center on Universal Design for Learning’s Universal Design for Learning (UDL) framework. UDL is a teaching and learning framework that “helps address learner variability by suggesting flexible goals, methods, materials, and assessments”.[33] The UDL framework is a direct response to inflexible, “one-size-fits-all” curricula that assumes that all children should be taught and assessed the same way. Instead, the UDL framework focuses on individual variability whereby each student is assumed to represent, express, and engage knowledge differently.

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[33] “The Concept of UDL,” *National Center on Universal Design for Learning*, accessed June 9, 2017, http://www.udlcenter.org/aboutudl/whatisudl/conceptofudl.

At its core, UDL is founded on three basic principles: (1) Provide Multiple Means of Representation, (2) Provide Multiple Means of Action and Expression, and (3) Provide Multiple Means of Engagement.[34] Principle 1 states that each learner represents the same information differently. While many secondary school teachers and university professors heavily rely on textbooks and lectures, they neglect the possibility that visual and hands-on learning might be more conducive to some of their students' learning needs. Principle 2 calls for individualized, rather than ubiquitous, assessment. Because students represent information differently, they should be given every opportunity to express their learning in a way that is conducive to their particular understanding. Principle 3 asserts that each student has a different motivation to learn. Differences in "neurology, culture, personal relevance, subjectivity, and background knowledge"[35] all have an influence on whether or not a particular student will be engaged on any given day or during a particular lesson.

Principle 1 of UDL and Systems Modeling

Once we analyze Verhoeff et al.'s teaching and learning framework in the context of UDL, it becomes immediately clear that the two agree on the same basic principles. According to principle 1 of UDL, different students represent knowledge in different ways. This principle suggests that teachers present knowledge to their students in different ways because it is uncertain which kind of representation is going to maximize student learning. When we analyze the four modeling stages in Verhoeff et al.'s framework, this is precisely what the teacher did. During stage 1 of the modeling

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[34] "The Three Principles of UDL," *National Center on Universal Design for Learning*, accessed June 9, 2017, http://www.udlcenter.org/aboutudl/whatisudl/3principles.

[35] "The Three Principles of UDL," National Center on Universal Design for Learning, accessed June 9, 2017, http://www.udlcenter.org/aboutudl/whatisudl/3principles,

framework, he had students look at cells of Paramecia under a light microscope and supplemented their learning by making analogies between how cells and familiar organisms function. In stage 2, the teacher differentiated his means of representation by not only having students look at plant and animal cells instead of Paramcia; he also had them view the cells using an electron microscope instead of a light microscope. During a discussion with the class he made the reason for his decision explicitly clear, "You stick to your model until it turns out to be otherwise, which seems to be the case now. But then you could still say: well maybe the model is right after all, but I just need better equipment."[36] The teacher understood that the representations of plant and animal cells provided by the light microscope were much too convoluted; as a result he variegated his teaching and discovered a more tenable means of knowledge representation. Nevertheless, the teacher's curriculum adjustments did not end there. He immediately realized that the images produced by the electron microscope, by itself, were not enough to help students identify the different organelles contained in the plant and animal cells. Consequently, he provided workbooks for students depicting clear plant cell models in order to help them recognize the organelles they could not initially find. Stage 2 of Verhoeff et al.'s teaching and learning modeling framework certainly embodied UDL's emphasis on "the importance of providing multiple, flexible methods of presentation when teaching."[37] By providing multiple examples, the teacher clearly attempted to address each student's pattern recognition network in a manner satisfactory for that

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[36] Verhoeff, Wasario, and Boersma, "Systems Modelling," 554.

[37] Tracey Hall, Nicole Stangman, and Anne Meyer, "Differentiated Instruction and Implications for UDL Implementation: Effective Classroom Practices Report" (2004): 9, accessed June 17, 2017, https://www.google.com/url?sa=t&rct=j&q=&esrc=s&source=web&cd=2&ved=0ahUKEwiz5PGGotjUAhWEOCYKHRZHBHsQFggtMAE&url=http%3A%2F%2Fwww.cast.org%2Fudlcourse%2FDifferInstruct.doc&usg=AFQjCNFFuyocisCdswR7LamCG-bx1CkCnw&cad=rja.

particular learner.[38] If the teacher acquiesced to the notion that "one-representation-fits-all," all of the students' learning would have been greatly compromised.

Stage 3 of Verhoeff et al.'s framework required students to collaborate with one another to build a large 3D model of a cell. In order to construct this model, students were encouraged to use various cell models from their textbook, biology books, and the Internet.[39] Some groups even went as far as accessing "dynamic computer models"[40] to further understand the organelle they were constructing. Stage 3 was similar to stage 2 in that the students did not rely on a single source of knowledge for their understanding. Rather than relying solely on their textbook, they were prompted to look at additional resources that presented the model of a cell somewhat differently. Stage 3 supports another recommended practice in UDL methodology: provide multiple media formats.[41] What may have been initially unclear in the student's textbook was clarified by using another book or a model presented on the Internet. For most visual learners, a normal 2D picture model of a cell is sufficient for basic understanding. For others, however, a more dynamic means of representation, such as an animated computer model, is more likely to entrench and establish learning. The diversity of resources provided by both Verhoeff et al. and UDL ensure that alternative representations can be accessed so as to maximize information and learning.[42]

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38. Hall, Stangman, and Meyer, "Differentiated Instruction," 9.

39. Verhoeff, Wasario, and Boersma, "Systems Modelling," 555.

40. Ibid.

41. Hall, Stangman, and Meyer, "Differentiated Instruction," 9.

42. Ibid., 7.

Stage 4 of the modeling phase required students to complete an interactive computer program on human digestion. The program allowed students to explore human digestion at the cellular, organ, and organism levels by various interactive activities such as dragging and dropping organs to the correct place in the human body and drawing arrows between cells to signify exchange of matter/information.[43] Unlike looking at models of cells in textbooks or on the Internet, the computer program promoted students' active learning through practice and experience. As limited as dragging-and-dropping and pointing-and-clicking activities might be, it was far more "interesting, engaging, and accessible"[44] than the skill-and-drill pedagogy prevalent in so many classrooms today. For Hall et al., digital materials are critical to student success because unlike speech, printed text, and printed images they are inherently flexible, meaning they can be modified depending on the needs of the student.[45]

Principle 2 of UDL and Systems Modeling

Principle 2 of UDL states that students should be provided with multiple, flexible methods of action and expression. In other words, teachers should allow students to express their learning in different ways. When we examine Verhoeff et al.'s teaching and learning framework, it is clear that the latter follows guidelines similar to principle 2 of UDL. This is exemplified by the fact that each stage of the framework calls on a different form of assessment. Stage 1 required students to create basic 2D drawing of cells multiplying, taking in food, and excreting waste matter. Stage 2 did not call for students

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[43] Verhoeff, Wasario, and Boersma, "Systems Modelling," 555.

[44] Hall, Stangman, and Meye*r*, "Differentiated Instruction," p. 4.

[45] Ibid., 7.

to complete any kind of formal assessment; instead the focus seemed to be on supplementing and supporting their knowledge. Recall that this is the stage where students made the transition from Paramecia to plant and animal cells, as well as light microscopes to electron microscopes. Because students had difficulty identifying the cell's various organelles, the teacher focused primarily on how to employ various types of media to bolster student learning. Now it would be a mistake to infer that the teacher abandoned assessment of plant and animal cells altogether; rather he merely postponed assessment to stages 3 and 4 where his students would be more adequately prepared.

In terms of student-produced artifacts, stage 3 was certainly the most compelling. Recall that this was the stage where students separated into smaller groups and each group created their own 3D model of an organelle that would later be combined with the other organelles to form a single, complete model of a cell. What distinguished this stage's activities from the others was its employment of kinesthetic (i.e. bodily) learning. Students were doing much more than drawing rough 2D sketches and dragging and dropping icons from one side of the computer screen to the other; they were cutting, gluing, coloring, and pasting. This kind of hands-on learning using manipulatives certainly caters to students who get easily bored with lessons that focus primarily on visual and auditory learning. The hope is that these kinds of engaging activities translate into Piagetian "sensorimotor learning," in which physical activity transforms into representative mental symbols."[46] The connection between stage 3's kinesthetic learning activity and principle 2 of UDL is an apparent one. According to UDL, teachers should

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46. Steven A. Wolfman, Rebecca A. Bates, "Kinesthetic Learning in the Classroom" *Journal of Computing Sciences in Colleges* 21, no. 1 (2005): 204.

provide their students with “flexible opportunities for demonstrating [their] skill.”[47] By varying their “requirements and expectations for learning and expressing knowledge,” [48] Verhoeff et al.’s teaching and learning framework demonstrates their full-fledged endorsement of UDL. In the end, both are entirely aware that “versatility is crucial in the classroom if learning is to occur.”[49]

The final stage of Verhoeff et al.’s teaching and learning framework had students use all of the knowledge they acquired from the computer program of human digestion to “draw a hierarchical systems model of the organism by combining the models at the cellular, organ, and organism level.”[50] The teacher used the activity as a dual assessment, evaluating the students’ understanding of the content of the computer program as well as their overall knowledge of the cell. According to UDL, “*supported practice* should be used to ensure success and eventual independence. Supported practice enables students to split up a complex skill into manageable components and fully master these components.”[51] When we look back at stages 1-3 of Verhoeff et al.’s framework, it is clear that each stage was arranged hierarchically so that the knowledge obtained in a later stage was built upon the previous one. At first, the teacher did not give students too much material at once. Similar to the teaching method guidelines put forth by UDL, he “chunked” learning into tractable lessons (i.e. stages) for students to master. Once each

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47. Hall, Stangman, and Meyer, “Differentiated Instruction,” 8.

48. Ibid., 10.

49. Judith Reiff, *Learning Styles: What Research Says to the Teacher* (Washington D.C.: National Education Association, 1992) quoted in Richard Gage, “Excuse Me, You’re Cramping My Style: Kinesthetics for the Classroom,” *The English Journal* 84, no. 8 (December 1995): 53.

50. Verhoeff, Wasario, and Boersma, “Systems Modelling,” 558.

51. Hall, Stangman, and Meyer, “Differentiated Instruction,” 10.

stage was mastered, the students used their learning as a scaffold for the next stage that contained more complex information. As students ascended through the hierarchy, the teacher increasingly relinquished his pedagogical authority and the students became more active and responsible learners.

6.4.3 Conclusion

Verhoeff et al.'s teaching and learning framework provides an example of a modeling curriculum at its apex. The primary strength of the curriculum is its willingness to have students construct a variety of models in the classroom. Unlike Mentzer, Huffman, and Thayer's engineering design lesson, the former did not discourage students from creating models of a certain type. Instead, students learned about the structure and function of cells via: light microscopes, images from electron microscopes, images in textbooks, images from the Internet, animated computer simulations, sophisticated computer programs, simple sketches, complicated hierarchical sketches, as well as physical, 3D models. Verhoeff et al.;'s commitment to model diversity reflects their understanding that different students represent knowledge differently at different stages in the learning process. While some models might work for one group of students, they might not work for others. More classroom activities need to follow Verhoeff et al.;'s lead and recognize that when it comes to modeling, heterogeneity is the key.

6.5 Beyond Mathematical Models

The case studies discussed in sections 6.2 and 6.3 of this chapter focus primarily on mathematical modeling. Archer and Ng's curriculum explores the relationship between mathematical models and the scientific method, while Mentzer et al.'s study suggests that high school engineering students favor creating visual 2D models of an engineering project rather than more sophisticated mathematical models. To a certain extent I sympathize with the authors' clarion call for more mathematical modeling because I agree that "some basic understanding of the nature of mathematics is requisite for scientific literacy."[52] However, I vehemently disagree with any suggestion, either implicit or explicit, that suggests sacrificing visual, physical, and analogical models for the sake of purely abstract, mathematical models. This hidden curriculum agenda usually takes the following form: In elementary school (grades K – 5), students should be exposed to rudimentary visual and physical models ranging from graphs to orreries. When they are in middle school (grades 6 – 8), they should be introduced to basic mathematical modeling, some of which can be done by hand, calculator, or computer. And by the time they reach high school (9 – 12), the students' focus should be primarily on complex mathematical modeling that can only be performed on and by computers.[53] The assumption this kind of modeling curriculum makes is that by the time students graduate from high school they no longer need to rely on obsolete visual and physical models because all the work can be performed more accurately using mathematical models. This dogma that mathematical models are somehow more desirable than their non-mathematical counterparts needs to be challenged. The discovery of the structure of DNA provides a case in point. The contribution of mathematical modeling to the discovery has received scant attention. However, quantum mechanics was "used as a check and a guide in the selection of probable molecular structures, and in particular to

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52. American Association for the Advancement of Science, *Science for All Americans*, 15.

53. American Association for the Advancement of Science, *Benchmarks for Science Literacy* (New York: Oxford University Press, 1993), 268 – 270.

accept or reject the bond distances and angles derived from X-ray diffraction pictures."[54] Nevertheless, the mathematical models produced by quantum mechanics do not diminish the contribution of other models used in the discovery including: Franklin's X-ray diffraction images, Watson's sketches to Delbrϋck, Pauling's folded paper model of a helical polypeptide chain that resulted in the discovery of the α and γ helices,[55] and last but certainly not least, Watson and Crick's physical model of the double helix constructed out of cardboard and then metal. The fact is that all of these models worked in concert with one another to produce Watson and Crick's groundbreaking discovery. The elimination of one or several of these models would have severely impeded Watson and Crick's progress. My point being, different models serve different purposes. At times, mathematical models are more appropriate than physical models, but the reverse is also the case. Students, teachers, and scientists need to be reminded that useful models come in a variety of forms; each should be given the freedom to explore which models are appropriate for a particular problem and which are not. My fear is that society's preoccupation with STEM education (and mathematical modeling in particular) is making it more and more difficult for visual, physical, and analogical models to be taken seriously as genuine problem-solving alternatives.

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[54] Robert Olby, *The Path to the Double Helix* (Seattle: University of Washington Press, 1974), p. 268.

[55] Olby, *The Path to the Double Helix*, 280.

Chapter 7

Models Revisited

7.1 Scientific Models, Pedagogical Models, and Student Models

In chapter five I discussed some of the differences between scientific models, pedagogical models, and student models. Scientific models are the various models scientists use everyday to solve scientific problems. These models are typically mathematical in form, but they can also be: visual (2D), physical (3D), computer generated simulations, or mental (e.g., Einstein's *Gedanken* experiments). Pedagogical models are the models that teachers, textbook authors, and curriculum designers create to make learning accessible for their students. Some examples include: pictures of the Bohr (i.e. solar system) atom, diagrams of the water cycle, Styrofoam-and-stick molecules, orrerries, and plant cells made out of papier-mâché. Student models are the students' mental models of pedagogical models. More specifically, they are the internal conceptualizations that students form when they encounter the pedagogical models provided by their teachers. For instance, a student's mental model of the Bohr atom consists of all her thoughts and beliefs about the Bohr atom spawning from her encounter with the pedagogical model (e.g. What does it look like?, How do the electrons within it behave?, How far are the electrons from the nucleus?, etc.).

In the same chapter I argued that pedagogical models could not be representations of scientific models because each has their own distinct epistemic purpose. Whereas scientific models are used by scientists to solve problems, pedagogical models are teacher created models designed specifically to help students learn the curriculum. It would be a mistake to argue that scientific and pedagogical models deal with the same target system because what is conducive for one might be inconvenient (even unnecessary) for the other. When Maxwell conceived of his model of the ether, he used it to explore the relationship between electricity and magnetism, which culminated in his famous Maxwell's Equations. Teachers and curriculum designers, on the other hand, do not create pedagogical models with the intent of supporting or disconfirming current theories or developing novel ones. Rather, their whole purpose is educational, to support students as they learn theories in normal science. For Maxwell, the ether model was invaluable; for physics teachers it has become nothing more than an expendable historical anecdote that merits scant attention. This sounds harsh, but a physics teacher's primary concern is to create pedagogical models that maximize student comprehension and application of Maxwell's equations, and not to re-create the anachronistic models that Maxwell himself used.

Some might argue that I am exaggerating the differences between scientific models and pedagogical models, because surely there are times when the two are similar, if not the same. We can imagine, for instance, a high school engineering teacher having her class recreate the Army Corps of Engineers (ACE) Bay Area model to scale. Like the original model, the pedagogical model would also show the catastrophic results of Reber's plan to dam up the San Francisco Bay Area. In this particular case, wouldn't the scientific model (the original ACE model) and the pedagogical model (the student model of the original model) resemble one another? I agree that the two models would share a visible likeness, but I am not willing to concede anything more. First, we must remember that the ACE model was only an approximation of what might have happened if Reber's plan had been implemented. This means that its designers had to make decisions which model elements had to be included and which were disposable. The same applies to the pedagogical model of the ACE model. In creating their model, the teachers and students have to decide which model elements are essential, and which are expendable. My point is that whether we are talking about the ACE model or the pedagogical model of the ACE model, human choice is always going to be a factor in model making.[1] In chapter two I discussed this issue in depth, arguing that because our choices are guided by our values, it is unsurprising that modelers allow extra-logical factors to affect the construction of their models. This consideration is exacerbated by the fact that the pedagogical model in question is a model of a model, which makes it considerably more difficult for it to maintain its integrity.

Second, even though a scientific model and a pedagogical model may share a similar appearance, it does not follow that they also share the same purpose. The ACE model was built in order to test a novel hypothesis; while a pedagogical model of the ACE model does nothing more than confirm a previously accepted hypothesis. The difference is subtle, but of great consequence. A scientist who is testing a new theory for the first time may have a hypothesis of what is going to happen once a model is run, but does not know for sure. The scientist is under a professional obligation to run a model several times to provide further support for her findings, whether or not those results support or refute her hypothesis. Teachers do not share the scientist's approach when creating and running a pedagogical model. The teacher already knows in advance how the simulation should run, what the results should be. If the pedagogical model does not run as expected, no attempt is made to fix either the model or the theory; instead the

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1. For a more thorough investigation of the role that choice plays in model construction, see chapter 2 of this study.

teacher explains the discrepancies away by appealing to errors in the model's reconstruction and/or user error. For a scientist, a positive model run could indicate a paradigm shifting theory; for a teacher, a positive model run is just another affirmation of normal science.

Even with these remarks, the critic might still be undeterred. In fact, she might ask me to consider the following argument. In order to solve the structure of DNA once and for all, Watson and Crick first needed to teach themselves what the structure looked like. In other words, before arriving at the double helix model, Watson and Crick had to first create several useful pedagogical models that would propel them towards their historic discovery. For instance, they had to teach themselves that the structure was a helix and that adenine bonded with thymine and guanine with cytosine. If this description is correct, then the distinction between scientific models and pedagogical models collapses because scientific models either are pedagogical models themselves or else they rely so heavily on the prior construction of several pedagogical models that it is difficult to tell where one ends and the other begins.

I do not deny that scientific models require the prior construction of several previous models before reaching its final form. Model construction is a frustrating process where modelers should anticipate the construction of multiple models, many of which end up being dead ends. (In the case of Edison this number is literally in the thousands!) But at the same time, these so-called dead ends are actually quite productive when they steer modelers away from unproductive research programs. Consider Watson and Crick's early three-chain model of DNA that failed to take into account how much water DNA actually contained. Had Rosalind Franklin not immediately pointed out their miscalculation, Watson and Crick would have stubbornly insisted on their incorrect model. Yet, I deny that this example and others like it support the view that pedagogical models are indispensable when it comes to the formulation and construction of scientific models because I dispute the notion that these early, abandoned models are even pedagogical models in the first place. Pedagogical models are models created by educators and professional scientists (when they are tasked with presenting their work to non-specialists) with the specific purpose of transmitting specific information about the scientific models in question. Pedagogical models are only created *after* a scientific model has reached its completion, never before. This is because the scientific model-building process and the pedagogical model-building processes are wholly separate endeavors with distinctive functions. The former is in the business of knowledge construction and production, while the latter is concerned with how best to disseminate and transmit this knowledge.

Let us return to the early Watson and Crick model of DNA that literally did not hold enough water. This model was neither a scientific model, nor a pedagogical model. It was not a scientific model because it was incomplete and erroneous; and it was not a pedagogical model because Watson and Crick did not use it to present their final version of the structure of DNA. Rather, the best way to understand the model was as a working model whose purpose was to bridge the gap to their final, scientific model. Of course, at the moment, Watson and Crick did not realize that this model would only be an intermediary; they were hopeful it was going to be the final model they would have to create. But such is the nature of scientific, pedagogical, and working models, their manifestations only become clear after the fact.

At this point, the critic might respond that even though Watson and Crick's early models were not pedagogical models in the operationalist sense I have formulated, they nonetheless served a pedagogical function in the broadest sense of the term by teaching Watson and Crick about the nature of DNA and how much water it needed to be able to contain. This, however, is blatant equivocation. From the start I have been adamant that a pedagogical model fulfills the function of informing the non-scientifically trained public about the most up to date findings in the scientific community. Watson and Crick's early models may have contributed to their final, scientific model, and thus played a pedagogical function in the layman's sense of the term, but that is not how I have been applying the concept throughout this project. Such a loose conceptualization would have us interpret every model as a pedagogical model, which runs the risk of rendering a once substantive concept into one bereft of any meaningful value.

7.2 Problems with Student Models

Student models are the students' mental models of the pedagogical models. When a student listens to a teacher's lesson, reads a chapter in a textbook, or completes an online assignment, it is assumed that the student's mental model of her task resembles the activity assigned to her. But this is the exception rather than the rule. In "Some Observations on Mental Models," Donald Norman describes student models as: incomplete, severely limited, unstable, confused, parsimonious, and unscientific,[2] which is just a roundabout way of saying that they are oftentimes inaccurate. Student models are incomplete in the sense that they lack essential information included in scientific and

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[2] Donald Norman, "Some Observations on Mental Models," in *Mental Models*, ed. Dedre Gentner and Albert L. Stevens (Hillsdale, NJ: Lawrence Erlbaum Associates, 1983), 8.

pedagogical models.[3] For instance, a student's model of force lacks the complexity and depth of her teacher's conception, and only bears a superficial resemblance to a professional scientist's model. To say that students' models are severely limited means that students lack the ability to run their mental models adequately.[4] It is one thing for a student to know that the seasons are caused by the 23° tilt of the celestial equator to the ecliptic, wholly another for a student to be able to run the simulation in her head. Students who are unable to make accurate predictions and/or accommodate their models to novel situations do not understand how their models work. Student models are unstable because students oftentimes forget the details of what they are learning about, especially if those details are not reviewed on a consistent basis.[5] Every teacher (and professor) knows that if they do not review material throughout the semester, most, if not all of it, will mysteriously disappear. Another common occurrence is that students confuse different models of the same phenomenon, sometimes through no fault of their own.[6] Consider the case of the atom. By the time students graduate from high school they are going to encounter several models of the atom, including the solar system model and the cloud model. Unfortunately, these models give us different, sometimes contradictory, information about the behavior of electrons in an atom. The solar system model tells us that the electron orbits the nucleus like a planet orbits the sun, while the cloud model maintains that the electron behaves more like a cloud in that it is everywhere at once. Recall Rosaria Justi's discovery (chapter five) of a textbook that combined these two

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[3] Norman, "Some Observations on Mental Models," 8.

[4] Ibid.

[5] Ibid.

[6] Ibid.

disparate models together to form a single "hybrid model" of the atom.[7] (No wonder students are confused!) Finally, the idea of models being parsimonious and unscientific goes hand in hand. Given the choice between a complex model to run and a simple one, some students will choose the latter simply because it requires less mental effort.[8] This means that given the choice between the solar system model and the cloud model of the atom, some students are going to adopt the solar system model for reasons that concern parsimony rather than the truth. From the fact that students allow non-rational factors to influence their model selection, it should come as no surprise that student models are also unscientific, or to use Norman's phrase, "superstitious."[9] It is unfortunate that when it comes to selecting and applying models, students are prone to follow the path of least mental resistance.

7.3 Student Models and Vygotsky's Zone of Proximal Development

If we accept the notion that student models are: incomplete, severely limited, unstable, confused, parsimonious, and unscientific, it becomes the teacher's task to alter her students' mental models so that they are less incomplete, limited, unstable, etc. In other words, teachers ideally want their student to enter what Vygotsky called the *zone of proximal development* (from now on ZPD). According to Vygotsky, ZPD is "the distance between the actual developmental level as determined by independent problem solving and the level of potential development as determined through problem solving under

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[7] Rosaria Justi, "History and Philosophy of Science Through Models: Some Challenges in the Case of 'the Atom'," *International Journal of Science Education* 22, no. 9 (2000): 1002.

[8] Norman, "Some Observations on Mental Models," 8

[9] Ibid.

adult guidance or in collaboration with more capable peers."[10] In layman's terms, it is the gap in between what the student already knows and what she doesn't know or what the student has already learned and what she can potentially learn. For teachers, the most significant aspect of ZPD is Vygotsky's appeal to "adult guidance" and "collaboration with more capable peers." The main point is that students cannot learn what they do not already know on their own, they require the assistance of teachers and other learned students to get them there. This suggests that in order for students to develop better mental models of scientific phenomena, they cannot be without their teacher's guidance. A few examples will illustrate my point. First, let us consider model confusion. On the one hand teachers need to correct their students if they embrace contradictory models of the same phenomenon (e.g. geocentrism and heliocentrism). On the other hand, physics is notorious for its contradictory models. In addition to the 30 concurrent models of the atom, light is also a contradictory phenomenon, at times behaving like a wave and at other times like a particle. Think back to the teacher in Rutherford's study (chapter five) who expressed the idea that students should not examine the nature of light until they were at least 17. Students have difficulty understanding contradictory models just as much as textbook authors have difficulty creating pedagogical models of them. For this reason, a teacher's counsel is more crucial than ever.

Second, let us consider what it means for students' mental models to be parsimonious. According to Norman, a student's mental model is parsimonious if she accepts the explanation that requires the least amount of intellectual effort. The most banal, and unfortunately common, form of student parsimony is when students simply

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[10] Seth Chaiklin, "The Zone of Proximal Development in Vygotsky's Analysis of Learning and Instruction," in *Vygotsky's Educational Theory in Cultural Context*, ed. Alex Kozulin (Cambridge: Cambridge University Press, 2003), 40.

resort to guessing because they do not want to put in the mental effort to solve a problem, or remember the correct answer. This kind of mental model parsimony is inexcusable and unacceptable. However, there is another kind of parsimony that is more sophisticated and warrants our attention. In some philosophy circles, parsimony is considered a “theoretical virtue.”[11] In the 14th century, religious philosopher William of Occam developed the Law of Parsimony, otherwise known as Occam’s Razor. According to the Law of Parsimony or Occam’s Razor, “Entities are not be to be multiplied beyond necessity.”[12] This means that other things being equal, the simplest explanation is usually the correct one. From the fourteenth century on, prominent natural philosophers (or scientists) used Occam’s Razor to support their position. Galileo used Occam’s Razor to defend the Copernican model of the solar system against the Ptolemaic one arguing that, “Nature does not multiply things unnecessarily; that she makes use of the easiest and simplest means for producing her effects; that she does nothing in vain, and the like”.[13] Similarly, the eighteenth century chemist Lavoisier appealed to Occam’s Razor to admonish the existence of phlogiston by remarking, “If all of chemistry can be explained in a satisfactory manner without the help of phlogiston, that is enough to render it infinitely likely that the principle does not exist, that it is a hypothetical substance, a gratuitous supposition. It is, after all, a principle of logic not to multiply entities unnecessarily.”[14] The Galileo and Lavoiser examples suggest that there may be some value to parsimonious models. The nebulous and

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11. Alan Baker, "Simplicity", ed. Edward N. Zalta (2016), in the The Stanford Encyclopedia of Philosophy, accessed August 1, 2017, https://plato.stanford.edu/archives/win2016/entries/simplicity/.

12. Louis P. Pojman, *Classics of Philosophy*, 2nd ed. (Oxford: Oxford University Press, 2003), 486.

13. Baker, “Simplicity.”

14. Ibid.

confusing nature of parsimonious models makes it more imperative than ever for teachers to be able to explain to their students in what instances parsimony is a virtue and when it is just a convenient excuse.

The teacher's obligation is to get her students to their ZPD. The issues of contradiction and parsimony suggest that when it comes to mental models, the content of the ZPD is imprecise at best and must be handled on a case by case basis. Students are besieged with innumerable mental models of various phenomena all the time, their only chance of making sense of these phenomena is through the stewardship of a caring, patient, and learned teacher.

7.4 Expert Knowledge vs. Novice Knowledge

According to Susan Ambrose, Michael Bridges, and Michelle DiPietro et al., authors of *How Learning Works*, experts such as practicing scientists use their background knowledge and experience to "create and maintain, often unconsciously, a complex network that connects the important facts, concepts, procedures, and other elements" within a domain.[15] These networks are often deep, meaningful, and based on abstract principles.[16] Non-experts (e.g. students), on the other hand, only have a handful of superficial ways in which they can organize their knowledge. A study cited by Ambrose, Bridges, and DiPietro et al. describes how practicing physicists organized different physics problems according to which 'laws of nature' informed each problem, while physics students organized the same problems into basic categories such as ramp

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15. Susan A. Ambrose, Michael W. Bridges, Michele DiPietro, Marsha C. Lovett, and Marie K. Norman, *How Learning Works: Seven Research-Based Principles for Smart Teaching* (San Francisco: Jossey-Bass, 2010), 43.

16. Ambrose, Bridges, DiPietro, Lovett, and Norman, *How Learning Works*, 43.

problem or pulley problem.[17] Once again, the notion of *purpose* cannot be overlooked. The physicist's mind naturally identifies complex patterns and relationships because that is how physicists have traditionally solved problems in their field for hundreds of years. Students, on the other hand, are primarily concerned with expending the least amount of cognitive effort necessary to answer questions correctly, which usually entails sacrificing rigorous thinking for the sake of expediency and convenience.

The authors of *How Learning Works* continue their comparison of expert and novice organizational structures by suggesting that even though novices (e.g. students) are far from attaining an expert level of expertise (e.g. scientists' knowledge), "there are instructional approaches that can help students organize their knowledge meaningfully around deep, rather than superficial, features of the domain."[18] In other words, there are ways to improve novice thinking so that it resembles expert thinking. Or to put it bluntly, there are ways to make students think like scientists. And this is where I disagree with Ambrose, Bridges, and DiPietro et al. and the scores of others in science education who believe that low student interest in science and low science assessment scores can be fixed by treating students like amateur scientists. In his research on the topic, Derek Hodson remarks that "Research fails to yield clear and consistent conclusions about the success of these [laboratory science] courses in sharpening children's understanding of the nature of science and increasing their abilities to employ the processes of science."[19] Even worse, these science classes focusing on problem solving and critical thinking have

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[17] Ibid., 54.

[18] Ibid., 58.

[19] Derek Hodson, "Towards a Philosophically More Valid Science Curriculum" *Science Education* 72, no. 1 (1988): 19.

had the deleterious effect of conflating scientific and non-scientific student models. Quoting Hodson, “If there is any transfer [of models] it would seem, if anything, to be in the opposite direction, with children applying their everyday thinking skills to laboratory problems.”[20] The point of turning the science classroom into a mini laboratory is to give students a space to act and think like amateur scientists, but this transformation cannot occur if students are applying their non-scientific models to solve scientific problems.

Henry Bauer also objects to teaching expert-level science thinking to students because he believes that the science classroom turned laboratory cannot possibly be a site where genuine science education takes place. According to Bauer, the experiments that students learn about and perform in science class are only examples of “successful science,” when in reality “history teaches that the science being done at any given time will largely be discarded, even in the short space of a few years, as unsuccessful.”[21] In an 1890 interview in *Harper's Monthly Magazine*, Thomas Edison is quoted as saying, “I speak without exaggeration when I say that I have constructed three thousand different theories in connection with the electric light, each one of them reasonable and apparently to be true. Yet only in two cases did my experiments prove the truth of my theory.”[22] When students learn about Thomas Edison and his invention of the light bulb, they are only going to be exposed to the theories and experiments that worked and not the 2998 theories that he discarded. Bauer’s point is that theory and experimental failure are as much a part of science as any successful theory and experiment. In fact, any honest

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[20] Hodson, “Towards a Philosophically More Valid Science Curriculum,” 20.

[21] Henry H. Bauer, *Scientific Literacy and the Myth of the Scientific Method* (Urbana: University of Illinois Press, 1992), 11.

[22] Thomas Edison, “Thomas A. Edison Paper,” Rutgers School of Arts and Sciences, accessed on July 17, 2017, http://edison.rutgers.edu/newsletter9.html.

scientist and historian of science will readily admit that science has seen far more failures than successes. By only exposing students to science's achievements, students will get the misguided notion that the scientific method, the principal organizational structure of classroom science experiments around the world, ensures success when history has demonstrated otherwise. When Edison's friend and associate Walter S. Mallory was asked about Edison's experiments on the alkaline battery, Mallory vividly recalled Edison remarking, *'Results! Why, man, I have gotten lots of results! I know several thousand things that won't work!'*[23] Bauer is adamant that science students must no longer be indoctrinated with the myth of the scientific method; rather it should be presented to them as an unattainable ideal that does not mirror what goes on in actual science laboratories.[24]

7.5 Against Science Students and Nonscientists as "Little Scientists"

Not everyone agrees with my position that scientists, teachers, and students all employ different kinds of models. In particular, William Brewer maintains "that scientists have the same basic cognitive architecture as nonscientists, with some possible differences due to selection and special training."[25] By "cognitive architecture" Brewer is referring to how nonscientists (i.e., anyone who is not a professional scientist) mentally represent different kinds of scientific concepts including, but not limited to, explanations, theories, and models. Brewer's argument is that, for the most part, nonscientists are just like scientists in that they have the same kinds of mental representations; the differences

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[23] Edison, "Thomas A. Edison Paper."

[24] Bauer, *Scientific Literacy*, 39.

[25] William F. Brewer, "Models in Science and Mental Models in Scientists and Nonscientists," *Mind & Society* 2, no. 4 (2001): 34.

being more of degree and sophistication rather than differences in kind. For example, children, like professional scientists, propose theories about how the natural world works, it is just that most of those theories are what we would call "native theories," but they are theories nonetheless. One of the examples that Brewer cites is his own study on the day/night cycle.[26] In response to the question, 'Where is the sun at night?,' one student responded, 'Well, it goes under the earth to China. See, while we have day China has night and while China has night, we have day.'[27] Now most us probably cringed after reading the student's explanation with thoughts about what is occurring in America's science classrooms, but Brewer's point is that no matter how mistaken the student's explanation is, it is still a causal/mechanical explanation about how the world works. Even though the student's explanation did not include any mention of the earth's rotation on its axis or the revolution of the moon around the earth, it nevertheless provides a causal explanation (albeit a very poor one) of why the sun and the moon both appear to rise and set at seemingly opposing times.

I do not deny that the previous student's 'goes under the earth to China' explanation counts as an explanation, what I do take umbrage with is the idea that just because children (and all nonscientists for that matter) provide causal/mechanistic explanations, they all of a sudden count as "little scientists." Consider the following example: The planets move because God is pushing them. This example satisfies many of the so-called epistemic virtues that philosophers of science are quite fond of: it is causal/mechanistic, universal in scope, simple, and can be mentally modeled. At the same

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[26] See Stella Vosniadou and William F. Brewer, "Mental Models of the Day/Night Cycle," *Cognitive Science* 18 (1994): 123 – 183.

[27] William F. Brewer, Clark. A. Chinn, and Ala Samarapungavan, "Explanations in Scientists and Children," *Minds & Machines* 8 (1998): 127.

time, no scientist or philosopher of science would classify such an explanation as scientific, even if we are working with the loosest possible conception of scientific available. This is because working scientists highly value testability and mathematical formalism as epistemic virtues as well, which the above explanation clearly does not satisfy. It is all the more curious that Brewer defends the "little scientists" moniker, given that he clearly recognizes that testability and mathematical formalism are values children do not possess and scientists do.

> 1. [O]ur overall analysis suggests that children's explanations for the physical world show the same essential structure as those used by scientists, *except that scientists include a requirement that explanations be potentially testable* [my emphasis].[28]
>
> 2. [I]t appears to us that children use most of the common forms of explanatory frameworks used by scientists, *except for formal/mathematical accounts* [my emphasis].[29]

Brewer would remark that I should be the last person defending the merits of testability and mathematical formalism given what I have said in this project about cosmologists putting theories out of the reach of empirical testability, and my defense of visual and physical models against the proliferation of mathematical models in both the natural and social sciences. And Brewer would be correct. However, all this discussion demonstrates is that there is a lack of agreement when it comes to defining what counts as a scientific model and what does not. (So in the end, Brewer has actually strengthened my position that there are no necessary and sufficient conditions when it comes to defining a scientific model!) My point is that even if children and nonscientists happen to share some of the same cognitive architecture as professional scientists, I am nevertheless

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[28] Brewer, Clark, and Samarapungavan, "Explanations in Scientists and Children," 128.

[29] Ibid., 133.

hesitant to call them "little scientists" because of the varying purposes that professional scientists and non-scientists have. When professional scientists propose a model, theory, explanation, or mathematical formulation of how phenomena in the natural world work, they do so with the purpose of solving specific problems in their specialized fields, of which they do not know the answer in advance. Nonscientists, especially those in a science classroom setting, on the other hand, do not employ their cognitive architecture in the same manner. Any explanations students proffer can be mitigated by their teachers who already have the correct explanation at their disposal, while professional scientists, because they are chartering into unknown territory, are devoid of such luxury.

For example, astronomers before Kepler, and even Kepler himself, assumed that all planetary orbits were circular. It was only after he applied exacting mathematical analysis to Tycho Brahe's meticulous observational data that he was able to derive his three laws of planetary motion, including the second law that states that all planetary orbits must be elliptical. Kepler's laws contain all the virtues scientists demand out of an exemplary scientific model or theory. They are: simple, universal, explanatory, predictive, can be confirmed or denied (i.e., testable), fruitful, and mathematically rigorous. The only significant virtue his laws were missing was a causal mechanism (gravity), which he was on the precipice of discovering before Newton himself, but eventually abandoned because he could not "work out the mechanics" to his satisfaction.[30]

When science students in elementary and middle school are asked about the shape of a planet's orbit, their answers are either immediately confirmed or denied by their

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[30] Arthur Koestler, *The Sleepwalkers: A History of Man's Changing Vision of the Universe* (Middlesex, England: Penguin Books, 1964), p. 343 – 344.

teachers and Kepler's laws are never spoken of again. And therein lies the difference between scientists and nonscientists. For scientists, their cognitive architecture is sacrosanct; their hierarchy of epistemic values is what they ultimately use to convince themselves and their colleagues that a scientific issue has been resolved. Nonscientists may initially use their cognitive architecture to propose a model or a theory about the world, but in the end they do not use it to adopt a given model or theory. Sadly, students base their model and theory choice entirely on a single factor alone, whether or not their teacher tells them they are right or wrong.

7.6 Models as a *Societas*

Scientific models, pedagogical models, and student models should all be treated as distinct phenomena in and of themselves because each is equipped with their own: purpose, logic, context, and idiosyncrasies that make them all particularly unique; nor should we think of them as imitations of one another because any similarities that they share are superficial at best, nothing more than a structuralist attempt to bring order to epistemic, cognitive, and naturalistic chaos. As Vladimir Tasić remarks, if "there is no such thing as a grounding, central principle…we invent it, assume it, make it up for our own convenience."[31] In the end, each kind of model plays by the rules of its own individual language-game, which means that once models get into the hands of scientists, teachers, and students alike, they take on a life of their own.

In *Philosophy and the Mirror of Nature*, Richard Rorty uses Michael Oakeshott's distinction between a *universitas* and a *societas*.[32] A universitas consists of a group of

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[31] 31. Vladimir Tasić, *Mathematics and the Roots of Postmodern Thought* (Oxford: Oxford University Press, 2001), 143.

people united by mutual interest to achieve a common end.[33] A *societas*, on the other hand, is formed by people bonded by civility and respect, rather than a common goal or common ground.[34] I believe that this is a useful distinction when ruminating about scientific, pedagogical, and student models. The usual way to approach models is to think of them as a *universitas*, connected by some kind of common ground or essence. But this study has shown this way of thinking to be outdated. A much more productive way to think about models is to regard them as a *societas*, bonded by mutual respect for one another instead of an overarching epistemological structure. By thinking of models as a *societas*, we are absolved from the obligation of treating them as "mirrors of nature" and free to praise or blame them depending on how they contribute to our students, education, and society in general.

The suggestion that we approach the study of models as a *societas* does not imply that I am advancing a relativistic view of models. Because I have gone to great lengths in this study to distinguish scientific, pedagogical, and student models from one another, it would be a mistake to compare them to one another. However, it is quite appropriate, some would even say obligatory, to compare like models with one another (i.e., scientific vs. scientific, pedagogical vs. pedagogical, and student vs. student). In this regard, all models are not equal. Copernicus's heliocentric model is a better model of our solar system than Ptolemy's geocentric model. The cloud model of the electron provides a better description of an electron's behavior than the Bohr model. The question that

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32. Richard Rorty, *Philosophy and the Mirror of Nature: Twentieth Anniversary Edition* (Princeton: Princeton University Press, 2009), 318.

33. Rorty, *Philosophy and the Mirror of Nature*, 318.

34. Ibid.

naturally arises is, how do we determine which models are better than others? What criteria do we use?

When it comes to scientific models, I advocate adopting models that either solve or can help solve current scientific problems. The heliocentric model of the solar system may have been more economic, and hence elegant, than the geocentric model, but that is not why it ultimately supplanted the geocentric model as the pervading model of our solar system. In the end, the heliocentric model just solved astronomical problems better. Quoting Thomas Kuhn, "In its most developed form the system of compounded circles was an astounding achievement. *But it never quite worked*." The geocentric model, replete with its epicycles or "compounded circles," was useful for navigational and theological purposes, but in the end it could not "discern or deduce the principal thing—namely the shape of the Universe and the unchangeable symmetry of its parts."[35] The heliocentric view, with the help of Johannes Kepler, was able to do just that. Assuming the heliocentric model, Kepler was able to determine: that the shape of all the planetary orbits are ellipses, that planets must cover the same area in the same amount of time, and that there is a mathematical relationship between a planet's period and its distance from the sun. Kepler's mathematical Copernicanism was nearly impossible for any mathematically minded astronomer to refute, and by the end of the 18th century the geocentric model of the universe was all but dead.[36]

The choice between pedagogical models comes down to which one maximizes student learning. Put differently, given two distinct models, the better pedagogical model

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[35] Nicholas Copernicus, *De Revolutionibus*, in Thomas S. Kuhn, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought* (Cambridge, MA: Harvard University Press, 1985), 139.

[36] Kuhn, *Copernican Revolution*, 227.

is the one that helps students understand the material presented in the curriculum better. The signposts that usually designate the desired level of achievement include students passing their science classes, as well as any statewide assessments. At the same time, anyone who has stepped into a classroom as a teacher knows that there is more to learning than just grades on a report card or test score numbers. The latter demonstrate a student's ability to get the correct answer, but as teachers, especially science teachers, we should want more than that. Paul Thagart implores that students should learn a particular scientific theory, "not just because it is in the textbook or class lectures, but because it is a powerful explanatory theory. Students need to learn not only what to believe, but how to believe for the right scientific reasons."[37] According to Thagart, a good place to start is having discussions with students regarding the nature of scientific evidence, explanations, hypothesis, and theories.[38] The committee for new K – 12 science education standards agrees with Thagart, stating that "The goal for students is to construct logically coherent explanations of phenomena that incorporate their current understanding of science…, and are consistent with the available evidence."[39] When a student answers a question correctly, she should be able to explain why it is the correct answer and not some other theory or explanation. For example, it is not enough for a student to know that the Earth goes around the sun and not the other way around; she should also be able to explain the advantages of the heliocentric model over its geocentric counterpart.

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[37] Paul Thagart, *The Cognitive Science of Science* (Cambridge, MA: The Massachusetts Institute of Technology Press, 2012), 225.

[38] Thagart, *The Cognitive Science of Science*, 225.

[39] National Research Council, *A Framework for K – 12 Science Education* (Washington D.C.: The National Academies Press, 2012), 52.

I can think of at least once instance, however, where a teacher might avoid a pedagogical model with the most explanatory power. In chapter six I discussed the idea of differentiation, the notion that different students require individualized teaching approaches and assignments that cater to their individualized ways of learning. In a differentiated teaching environment, we can sympathize with a teacher selecting a pedagogical model that is more conducive to her student's individualized learning style, rather than one with stronger explanatory power. Teachers know their students the best, and what works for one student might not work for another. When explaining electricity, teachers usually resort to one of two analogies: either electricity is like water-flow or electricity is like a moving crowd.[40] Both analogies provide clear explanations for current, voltage, and resistance, but the moving-crowd analogy is deficient in that there is no clear explanation of batteries in the analogy.[41] Clearly the water-flow analogy is the stronger analogy for the reason just stated; however, we can certainly imagine teachers invoking the moving crowd analogy either as a supplement to the water-flow analogy or to give students an entirely different approach to think about electricity. Student models are precarious, you never know which models students are going latch on to and which ones they are going to completely disregard. While some students might immediately gravitate towards the water-flow analogy, others might make more sense out of the moving crowd analogy. When it comes to differentiation, teachers should use their discretion in selecting the pedagogical models that will maximize their students' learning. For most students this means being introduced to models with the most explanatory

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[40] Dedre Gentner and Donald R. Gentner, "Flowing or Teeming Crowds: Mental Models of Electricity," in Gentner and Stevens, *Mental Models*, 108 – 111.

[41] Gentner and Gentner, "Flowing or Teeming Crowds," 108 – 111.

power. For others, it entails sacrificing a modicum of explanatory power for models that simply make more sense to them.

The reason teachers have had a frustrating time evaluating their students' mental models are that they are notoriously difficult to determine. Donald Norman, cited earlier in this chapter, gives us many reasons why this is so. First, verbally asking students for an answer and/or explanation might be informative, but ultimately incomplete.[42] Second, and more importantly, teachers encounter what is referred to as the "demand structure" problem.[43] Norman explain the problem thusly,

> If you ask people why or how they have done something, they are apt to feel compelled to give a reason, even if they did not have one prior to your question. They are apt to tell you what they believe you want to hear…Having generated a reason for you, they may then believe it themselves, even though it was generated on the spot to answer your question.[44]

Imagine the following scenario. A teacher asks a student how she thinks an electron behaves. In her mind, she begins running a mental simulation of an electron traveling around the nucleus like a planet, but then she remembers that her teacher told her that the solar system model was an outdated model. She quickly remembers that her teacher said something about a cloud. She herself does not know what this means, she just knows that it is the correct answer. Consequently, she tells her teacher that the electron behaves like a cloud and her teacher congratulates her for her keen intellect. Even though the student in question got the correct answer and has established a verbal connection between the words "electron" and "cloud," she has absolutely no conception of what it means for an electron to behave as a cloud. In her mind, she is still attached to

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[42] Norman, "Some Observations on Mental Models," 11n.

[43] Ibid.

[44] Ibid.

the solar system model because that is the model she is able to mentally run and make the most sense of, even though she recognizes that come test day, it is technically the inferior model.

The “demand structure” problem highlights the importance of differentiation, especially when it comes to student created artifacts; the implication being that teachers need to give their students access to as many different forms of media as possible to demonstrate their knowledge. What might be difficult for a student to express verbally might be easier for her to express physically, using either a visual 2D model or a mechanical 3D model. This does not mean that students will not still try to give their teachers the desired answer, but it certainly creates multiple opportunities for teachers to get a glimpse of what their students genuinely think. Only after a teacher has a clear idea of the kinds of mental models her students possess, can she judge these models according to their: accuracy with observed phenomenon, explanatory power, coherence, evidential/data support, and predictive power. Just because a teacher encourages students to produce several different models of the same phenomenon, it does not follow that all of the models created are of equal value.

7.7 Concluding Remarks: Models in the STS and NOS Curriculum

In choosing pedagogical models, teachers must consider choosing the ones with the most explanatory power. Unfortunately, in many science classrooms, issues regarding the explanatory power of models are viewed as beyond the purview of traditional scientific content and as a result are not adequately considered. This is not only a shame, but completely and utterly disagreeable. If scientific literacy genuinely refers to a student’s understanding of the “concepts, principles, theories, and processes of science, and one's awareness of the complex relationship between science, technology, and society,"[45] then STS (Science and Technology Studies) and NOS (Nature of Science) issues should not be perceived as just "additional didactic strateg[ies]"[46] that compliment STEM education; they ought to thought of as endemic to it. Science teachers and students should familiarize themselves with the differences between science *by* inquiry and science *as* inquiry,[47] the two sides of the scientific literacy coin. Teachers and students alike need to realize that knowledge of one does not imply knowledge of the other, yet both are requisite for genuine scientific literacy.

Part of the reason why STS and NOS issues have not been adequately addressed in primary and secondary classrooms has to do with mandatory statewide assessments. My quarrel is not so much that students are required to take such assessments (that is a different issue altogether); rather, I object to their content. As long as these assessments focus strictly on traditional scientific content knowledge and not STS or NOS issues, teachers and administrators have a convenient excuse to disregard the latter, even if it flies in the face of the most up to date AAAS (American Association for the Advancement of Science) and NRC (National Research Council) recommendations.
For decades now, modeling has been a hot topic in science education. What the field needs now are more and more researchers and practitioners working together to design

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[45] Fouad Abd-El-Khalick, Randy L. Bell, and Norman G. Lederman, "The Nature of Science and Instructional Practice: Making the Unnatural Natural," *Science education* 82, no. 4 (1998): 417-418.

[46] Dietmar Höttecke and Cibelle Celestino Silva, "Why Implementing History and Philosophy in School Science Education is a Challenge: An Analysis of Obstacles," *Science & Education* 20, no. 3-4 (2011): 299.

[47] Derek Hodson, "Toward a Philosophically More Valid Science Curriculum," *Science Education* 72, no. 1 (1988): 22.

innovative, teacher-friendly, and student-friendly STS and NOS curriculums that emphasize the central role that models play in the scientific process.

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