Making Models Work

by

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Dissertation submitted in partial fulfillment of
the requirements for the degree of Doctor
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ABSTRACT

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Abstract

Scientific models are used to investigate reality. Here "model" refers to a representation which is created by an agent for a particular inferential purpose. These purposes include but are not limited to explanation, prediction, exploration, classification, and measurement. Through modeling, scientists become capable of understanding the composition and structure of natural systems and social systems in a systematic manner constitutive of scientific research. This process of understanding is underwritten by a logical structure distinctive to scientific modeling.

Throughout this dissertation, I articulate, justify, and defend a specific account of the logical structure of scientific modeling. In order to do so, I detail economic models, which I contend are representative of general scientific modeling. Broadly, my account of scientific modeling can be decomposed into three distinct claims. First, I argue for understanding scientific modeling in terms representation. Following others, I then conceptualize representation in terms of *purpose* and *relevant similarity*. However, against this conceptualization are numerous counterarguments, which I proceed to detail and then disarm.

Second, I argue that the ideal scientific model is a useful model. Connectedly, I contend that in order for a scientific model to be useful, it must first be *idealized*. In order to demonstrate the necessity of idealizations for scientific modeling, I begin by detailing

a number of idealization strategies and demonstrate how they are integral to the use of scientific models across the natural and social sciences. But in order to demonstrate that idealized models are not only useful but are ideal, I dismantle the putative ideal of completeness which holds that the ideal model completely represents reality in all its detail and complexity. However, as I demonstrate, completeness is neither achievable nor a legitimate aspiration for working scientist.

Third, I argue that in order to use scientific models, it is often necessary for scientists to *alter* them in order to better fit particular target systems. In order to explain the alteration process, I detail the representational continuum found across the sciences which stretches from highly concrete data models to highly abstract principles. Between these extremes are theoretical models and empirical models. In order to construct such models, scientists must engage in an exploratory process by which possibilities are mapped and relative likelihoods estimated. In this way, scientists can construct highly specialized models which can allow them to better pursue specific inferential purposes. All of this results in a division of inferential labor and associated efficiency gains which, I argue, are constitutive of scientific progress.

Dedication

For Saba.

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1. Introduction

Models work, at least some of the time. While acknowledging that this is the case is relatively easy, understanding how models work is far from it. It is clear that scientists deploy models in a variety of ways and for a variety of purposes. However, even a cursory examination of scientific models reveals that there is no uniform method of modeling which can account for the diversity of scientific modeling practices. Moreover, while in the philosophical literature there is a consensus that scientific modeling is a legitimate methodological approach, there is little consensus as to how models actually work. In order to situate modeling within scientific practice, it is necessary to account for how modeling is conducted by working scientists in such a way which does justice to their practices while simultaneously explaining the underlying rationale which justifies those practices. In other words, it is necessary to understand *how models work*.

This is a broad mandate. Since models work in a variety of ways, under a variety of conditions, and for a variety of purposes, it is perhaps a fool's errand to even attempt to give a uniform account of scientific modeling. There are just too many exceptions to ever prove the rule. However, I contend that it is possible to glean the underlying *logical structure* of scientific modeling which can account for most, if not necessarily all, modeling methods. This logical structure is the key to understanding how models work. "Logic" here does not refer exclusively to symbolic logic but to the general framework of reasoning which is essential to the sciences as a whole.[1] It can be understood broadly as the *normative science of belief* by which evidence and arguments can be evaluated and used.

In this dissertation, I will investigate, articulate, and justify the logical structure underlying scientific modeling. However, this is a broad mandate. "Modeling" refers to a complex web of interrelated but distinguishable scientific methods. Implicit in an investigation into *the* logical structure underlying scientific modeling is the presupposition that there is a unified methodology which underwrites this complex web of scientific methods and practices. It is possible that there is no such uniform logical structure and that instead, there are as many logical structures as there are modeling methods used across the sciences (Veit 2019). This radical pluralism is possible, but I contend implausible. At most, modeling can be seen as a family-resemblance concept with no definite set of necessary and jointly sufficient conditions. This would make sense of the very real differences in modeling practices seen both within and between different scientific disciplines. Modeling in population genetics does differ from modeling in fluid dynamics which in turn differs from modeling in industrial organization. However, while these differences are important, the fact that these highly

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[1] Although the broad conception of logic used throughout this dissertation may appear linguistically revisionary, it should rather be understood as harking back to an earlier tradition. For Peirce, "*Logic* is the art of reasoning" and "*Reasoning* is the process by which we attain belief which we regard as the result of previous knowledge" (1895/1998, p.11). For Ramsey, "the business of logic [is] to tell us what we ought to think" and "the general conception of logic [is] as the science of rational thought" (1926/1990, p.80-82).

diverse practices are all considered instances of "modeling" is indicative that there is some underlying and unifying logical structure common to scientific modeling.

This is far from a definitive proof. Judgements can be mistaken and the fact that these diverse practices are all referred to as members of a single methodological category could be all so much flotsam. However, I contend that the perception of modeling as a unified methodology gives us good reason to search for a unifying logical structure. If it turns out to be impossible to articulate and justify such a logical structure, then so much the better for radical pluralism. But in the meantime, it seems worthwhile to at least attempt to understand how models work.

1.1 Representation and Reality

In order to articulate and justify the logical structure of scientific modeling, it is useful to first consider to what extent, if any, such an investigation is worthwhile. Perhaps for some, understanding how models work could be considered some esoteric philosophical problem divorced from practice. Scientists seem more than capable of constructing and using models without having articulated or justified any underlying logical structure, all of which seems to indicate that such an investigation is all so much hot air. However, understanding how models work is not an esoteric philosophical puzzle. It is *the* practical problem of scientific modeling. And the proceeding account is meant to not only satisfy philosophical curiosity but to speak to working scientists, even if most are not listening. However, here at the outset of the investigation, it is necessary to provide a broad conceptual map for the forthcoming account. To some extent, this may appear overly abstract and divorced from practical scientific research. But in so far as the concepts detailed in this section are elaborated and contextualized throughout the remainder of the dissertation, it will become clear the extent to which understanding how models work is a practical endeavor.

Scientific models work by *representing*. It is true that in particular instances, scientists may employ models for work which is not explicitly representational. However, underlying these modeling practices is a basic representational framework by which models become useful. Consequently, the bulk of this dissertation will be devoted to articulating and justifying the representational capacities of scientific models. Generally, representation can be understood as a four-place relationship in which a subject uses a model to represent a target system for some purpose (Giere 2004, p.743). From this account, it is possible to accommodate the diversity of representational activities found both within the sciences and outside of them. Generally, it is useful to conceive of representation as a form of substitution. Here substitution involves *delegation*. In art, portraits act as a substitute for persons. In politics, a representative legislature can act as a substitute for the electorate, who delegates political authority to their representatives.[2] And in science, models can act as substitutes for target systems.

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[2] In order to understand political representation as an act of substitution, it is necessary to situate sovereignty in the electorate. By so doing, the electorate then delegates the responsibilities entailed by

To be interchangeable, two objects must be useful for some of the same purposes. In art, portraits can act as a substitute for persons because a portrait can be interchanged with the person, given the purpose of conveying information concerning the physical appearance of the person. To delegate, one object must "take on" the responsibilities originating with the other object. In politics, a representative legislature can act as a substitute for the electorate because the electorate delegates its sovereign authority to elected representatives. And in science, models can act as a substitute for target systems because models are interchangeable with and delegates for target systems.

Before I can articulate how models work, and more specifically how models represent target systems, I will characterize what is meant by a "model". It is largely beyond the scope of this dissertation to exhaustively characterize the ontological status or even the precise composition of scientific models. As a rule, scientific models are quite heterogenous. They differ in the media through which they are instantiated and in the relationships they represent. I have adopted throughout the dissertation an expansive conception of model. It is not limited to the kinds of set-theoretic structures commonly used in symbolic logic.[3] Nor should a model be conceived of only as an

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sovereignty through the electoral process. In this way, the legislature becomes representative. The analogy between scientific and political representation is further elaborated in the proceeding analysis (Section 2.1.1).

[3] By avoiding the restrictive conception of "model" as employed in set-theory and symbolic logic, I have avoided any conflation between my account and the *Semantic View of Theories* in which theories are understood in terms of set-theoretic models.

abstract system of equations, as is commonly found across the sciences. Models can be composed of mathematical objects and models can also be composed of physical objects. Consequently, on this understanding of models, both the Cournot model (Section 2.1.3) and the physical Watson-Crick model of DNA are equally both models.

Despite this evident heterogeneity, all models are *artifacts* (Knuuttila 2011). By this I mean, models are made, they must be intentionally constructed by a working scientist. The specific procedures which constitute the construction process are often dependent on the medium through which the model is exemplified. Constructing a mathematical model often involves quite different procedures than constructing a physical model. In order to clarify this construction process, consider simple models constructed out of seemingly natural objects, say a primitive architect mapping out a simple house directly on to the intended location. Before constructing the walls of the house, he might well use a simple piece of driftwood to *stand in for* or *represent* the doorway. In this way, the naturally occurring driftwood becomes a model because it attains particular and useful representational capacities. Specifically, it allows the architect to place the walls in the correct position so as to allow him to later construct an appropriately sized door. However, for some it may be strange to think of driftwood as an artifact.

In order to construct a model, it is necessary to situate it within a broader representational framework. Driftwood thrown at random on the ground is no more representational than any other object. However, in the case of the primitive architect, the driftwood is *purposefully* placed at a particular location in order to fulfill a particular use. It is intended to represent something and in so doing, allows the architect to build the house. In this case, it is the purposeful placement of the driftwood which constitutes the construction and which makes the driftwood artificial. If the wind had carried the wood to that exact location, it would not have been a model of the doorway, unless the architect then purposefully built the house around it. Only through the purposeful actions of the architect does the driftwood become a model. Other models are far more complex than a simple piece of driftwood. And with this complexity comes a commensurately complex construction process in which disparate objects are conjoined in particular ways to achieve an object capable of fulfilling a highly specific purpose.

1.1.1 Purpose

Representation is also done for a purpose. Nothing ever has nor can represent without an articulable purpose. Oftentimes, many purposes can guide a single act of representation and at other times, no definite purpose can be uniformly identified by the participants. But underlying all acts of representation are purposes. For this reason, in order to articulate the logical structure of scientific modeling, it will be necessary throughout this dissertation to consider the varied purposes which guide modeling.

From an examination of scientific practice, it becomes evident that there are many different purposes involved in scientific modeling. This seems to follow from the heterogeneity of scientific modeling practices. However, it is possible to identify a single overarching purpose which guides all instances of scientific modeling: *inference*. All models are used for inference, in one manner or another. And here "inference" means, broadly, the process by which information is determined on the basis of other information. However, inference can itself be better understood by decomposing it into related purposes. Foremost among these is communication. Models are often used as communication tools by which one scientist conveys information to another scientist, or even the public, but communication is primarily a kind of inference.

Under the auspices of the general purpose "inference", it is useful to distinguish *five* particular kinds of inferences for which models are commonly used: *explanation*, *prediction*, *exploration*, *classification*, and *measurement*. Both explanation and prediction are commonly identified by philosophers of science as scientific purposes. Throughout this dissertation, it will be evident the varied ways in which models are used for both explanation and prediction (Sections 2.1.3, 3.1, and 4.3). However, philosophers of science rarely associate exploration, classification, and measurement with scientific modeling. Broadly, exploration can be understood as the process by which a scientist uncovers different possibilities and different evidentiary sources. Classification is the process by which a model imposes an ontology onto reality, thereby allowing the scientist to both categorize evidence and to create useable data models. And finally, measurement involves the mediated interaction between reality and scientist in such a way that the scientist is able to create relevant evidence. All of these kinds of purposes will be further elaborated and situated within the logical structure of the modeling process throughout the remainder of the dissertation.

1.1.2 Abstraction

Although all models are representational and all models are used for inference, models differ along many dimensions. One dimension of particular importance is *abstractness*. By being abstract, an object exemplifies only those properties which are important given a particular purpose. For this reason, some have argued that abstract objects must be constructed by simplifying reality (Cartwright 1989, p.187).[4] However, in order for the scientist to simplify reality, they must already possess complete knowledge of reality and then subtract from it (Section 3.3). Given that scientists are not endowed with complete knowledge, abstraction should not be understood as simplification but rather as a constructive process through which the scientist identifies those generalizable properties which are useful for them. In so doing, the scientist constructs objects which exemplify only those properties which are important to them.

Given that certain models are more abstract than others, it becomes possible to classify different types of scientific representations. By stitching together these different

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[4] Cartwright draws a distinction between *abstraction* and *idealization* (1989, p.187). In it, she characterizes idealization as a process of changing and abstraction as a process of subtraction. Following her, a sub-literature exploring and challenging this distinction has developed (Levy 2018). However, throughout this dissertation, I treat both abstraction and idealization as interchangeable because subtraction is a means for accomplishing change. It is worth noting that I do not deny that there may be some important differences between abstraction and idealization but that for my purposes, they are interchangeable (Section 3.1).

representational categories, it is possible to construct a *representational continuum* stretching from reality to the most abstract kinds of representations.[5] With this representational continuum, it becomes possible to situate the logical structure of scientific modeling within the broader logical structure of science.

In order to better understand the representational continuum, it is useful to characterize the extremes. First, consider reality. Throughout the foregoing discussion, I have regularly referred to "reality"; however, it may not be apparent what precisely is meant by the term. For me, reality is the external world. But in this context externality ought not to be understood in spatial terms but rather in terms of *resistance*. Reality is not arbitrarily controllable by mind. No individual can dictate reality without it "pushing back". This is not to say that we cannot interact with reality, far from it. Through the course of our everyday lives, we regularly interact with reality and attempt to observe and shape it in various ways. However, although we can often interact with reality, there is no guarantee that we will be successful in our attempted interventions. Reality is, in this sense, not ultimately plastic. It resists us in ways which constrain our activity. And through these constraints, it becomes possible to assess the fidelity of our representations. And precisely because of this, it is *real*.

[5] The idea behind the representational continuum is largely derived from Giere. However, in his presentation he presents it as a vertical hierarchy, albeit one in which arrows of dependency go in both directions (2004, p.744). By conceiving of the continuum as horizontal, it is possible to more clearly indicate that there is no hierarchical ordering among the different kinds of representations.

On the other end of the representational continuum consider scientific principles. Many, although not all, scientific disciplines are organized around highly abstract principles which determine the basic ontological categories and the most fundamental relationships within a particular research program. For example, classical mechanics is largely organized around the three *Newtonian principles* and the conceptualization of absolute space. These can be considered highly abstract representations, or principles. In particular, the Newtonian principles define the ontological categories of force and mass as well as encode fundamental relationships between these new ontological categories and previously articulated ones such as position, velocity, and acceleration (Giere 2004, p.745). By so doing, the Newtonian principles exemplify the most important and generalizable properties in classical mechanics. For another example, in economic analysis the *rationality principle* holds that agents "do the appropriate act" given a particular situation, in which the particularities are determined by tastes and constraints (Popper 1967/1983, p.359). Here, the rationality principle imposes a highly abstract ontology onto economics which includes agents, tastes, and constraints. It also captures the basic relationship between these categories in such a way that it allows the scientist to understand how to represent human decision-making. In this way, the rationality principle succeeds in providing a highly abstract representational framework through which human action can be studied.

Between reality and principles there are numerous different kinds of representations which differ in terms of their abstractness. Here it is worth noting that reality is *not* a representation. It is ultimately that which is represented. However, "right next to" reality are data models which *mediate* between reality and scientific models. Specifically, data models allow for the creation of data. Data are not a part of reality. Rather, data are representations of reality. In other words, I wholly reject even the notion of "raw data" for no data could ever be raw. Each datum must be constructed under the auspices of a data model which classifies and measures reality in a particular way and allows the scientist to discard or refine certain observations (Suppes 1962; Leonelli 2019). From there, it follows that more abstract representations exist which attempt to organize data and provide explanatory and predictive frameworks through which other systems can be explored. These more abstract representations which sit in between data models and principles are commonly referred to as "models" and the bulk of the dissertation will primarily focus on how these intermediary representations work.

The logical structure of scientific modeling dictates that models are *autonomous* (Morgan and Morrison 1999). Here autonomy ought to be understood in terms of derivability and reducibility. No model is wholly derivable from either a scientific principle or from a data model. Rather, the construction of a model requires an exploratory process through which the possibilities are articulated and mapped (Section 2.1). Nor is any model reducible to a principle or to a data model. Models exit *in between* these different extremes of the representational continuum and are capable of mediating between them (Giere 2004; Morgan and Morrison 1999; Swoyer 1991). Mediation enables and constrains inference. Through mediating principles and data, models allow the scientist to impose order onto data models in a way which provides empirical content to principles and generality to data. In this way, models allow scientists to use and assess both principles and data models in a complementary manner.

1.1.3 Accuracy and Precision

Model assessment begins with use. Models are tools and as with all tools, assessment is relativized to the purposes which guide its use. A boxcutter can be used to cut boxes and to cut steaks. It is well suited to the former of these uses but is an imperfect tool, at best, for cutting steaks. Similarly with models. A model can be used to both explain and to predict but some models are far better suited for explanations than predictions, and vice versa. Consequently, in order to assess a model, it is first necessary to determine the purpose which guides it use. However, given that all models are representational, it is possible to *empirically* assess a model by its accuracy. By this I mean, it is possible to empirically assess a scientific model by the extent to which it coheres with reality. Generally, when all else is equal, accurate models are to be preferred to inaccurate models.

However, in order to articulate the logical structure of scientific modeling, it is necessary to distinguish between *accuracy* and *precision*. Accuracy concerns the coherence between representation and reality while precision relates to the fineness of measure by which that coherence is assessed (Hoover 2019, p.13). By drawing this distinction, it is possible to understand that accuracy assessments can be made only once the degree of precision has been established. Additionally, it follows that it is possible to have an imprecisely accurate model as well as a precisely inaccurate model. Neither of these is necessarily ideal; however, given the purposes of the scientist, it may be more than sufficient to adopt a relatively imprecise measure. Given this, the imprecisely accurate model would be empirically adequate, although it might not be in cases where greater precision was determined necessary. And greater precision is neither always desirable nor even always possible (Section 3.3). In certain contexts, relative imprecision may be better suited to the particular purposes of the scientist than a more precise metric. However, once the degree of precision has been set, accuracy is always preferable to inaccuracy.

In order to illustrate the distinction between accuracy and precision, consider the "target" used in archery competitions. In competition, archers are tasked with hitting the target and are assessed by the extent to which they accurately do so. Commonly, archery competitions divide up the target into distinct spatial regions, demarcated by concentric circles. The innermost circles are deemed "more valuable" in the competition, with the central "bullseye" deemed maximally valuable. In this case, the concentric circles and associated spatial regions dictate a degree of precision. All hits within the same spatial region are deemed equally valuable and from this, accuracy assessments follow. If an archer were to hit the outermost edge of the target, the shot would be assessed as inaccurate. However, this assessment follows from the spatial decomposition of the target. Suppose that in rudimentary archery contests the target was treated as an undifferentiated circular object without concentric rings or a central "bullseye" point. On this undifferentiated target, all shots which hit the target would be deemed equally valuable. Given this imprecise metric, any two shots which hit the target would be deemed equally accurate, even if one hit only the outermost edge of the target and the other hit right on the central "bullseye" point. Here, the accuracy of the shot depends *both* on where the shot hit the target and the precision of the metric imposed onto the target.

With the distinction in place, it becomes possible to better understand the logical structure of scientific modeling. Models work by representing reality. However, representation is always relativized to the particular purpose guiding the research. And although the particular purposes are highly dependent on the specific modeling exercise, it is useful to classify purposes in terms of exploration, explanation, classification, prediction, or measurement. Following from the relevant purposes, it becomes possible for the scientist to determine the appropriate degree of precision necessary for an empirical assessment of the model. However, this brief description of the logical structure of modeling is only a primer and will be expanded upon throughout the dissertation.

1.1.4 Creativity

Finally, modeling is a fundamentally *creative* enterprise. There is no algorithmic procedure by which models are constructed or used. From beginning to end, modeling involves numerous choices which require creativity and ingenuity. It is important to keep this in mind given the focus of the dissertation. While models work, they do not *just* work. Success is not the result of some accident or luck. Knowledge is not granted, it is earned. Models are artifacts which through the creativity and ingenuity of scientists come to represent target systems. Modeling is a fundamentally active and creative process, involving innumerable alterations and adjustments. Models do not just work; models must be *made to work*.

2. How Models Work

Models work by representing. In general, representation can be understood as a form of substitution through delegation. Legislatures can be substituted for electorates. Portraits can be substituted for persons. And scientific models can be substituted for target systems. Common to all representations, whether they be political, artistic, or scientific, is that representations are used to achieve a particular purpose. The representative legislature can be used to convey legitimacy. The representative portrait can be used to convey appearance. And the scientific model can be used to convey information concerning a target system. Throughout, representations are used in accordance with the purposes which guide their creators and users. Political representation is guided by the need for stable and accountable governance. Artistic representation is guided by the goal to reflect reality. And scientific representation is guided by the inferential goals to understand and control reality.[1]

In order to articulate the logical structure of scientific modeling, it is useful to first consider the kinds of purposes which guide it. These purposes include *explanation*, *prediction*, *exploration*, *classification*, and *measurement* (Section 1.1.1). Together, these

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[1] Philosophical accounts of representation sometimes differentiate scientific representation from other kinds of representation such as mental, artistic, and political (Callender and Cohen 2005). Throughout this dissertation, I regularly use analogies with non-scientific representation in the context of art or politics in order to better illustrate precisely how models are used to represent target systems. In so doing, I am actively undermining any definitive distinction between scientific representation and other kinds. However, in so far as representation is largely determined by purpose and science can be characterized by distinctive purposes, it is reasonable to argue that some representations are distinctively scientific, even if there is nothing distinctive about scientific representation.

purposes are directed towards enabling the scientist to draw inferences regarding the constitution of reality. Explanation allows the scientist to determine those pathways by which reality can be controlled and manipulated (Woodward 2003).[2] Prediction allows the scientist to project from observations to unobserved portions of reality. Exploration is the overall process by which the scientist uncovers, or in some circumstances creates, those possibilities which can then figure into explanations and predictions as well as the construction of additional models. Classification involves the imposition of ontological categories onto reality in such a way that the scientist can then observe reality in a useful manner. And finally, measurement allows the scientist to generate accurate observations which can then be used for explanations, predictions, and even explorations. Together, these purposes enable the scientist to understand and control reality by allowing them to generate and assess inferences from models to reality.

Despite the importance of purposes, they alone do not determine the logical structure of scientific modeling. Representation, although largely determined by purpose is ultimately dependent upon the actual constitution of reality. A model can represent reality, or more precisely a particular target system, if and only if it is *relevantly similar* to that target system. Here relevant similarity is determined both by the purposes of the scientist as well as by the constitution of reality. Any target system will

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[2] Throughout the dissertation, I regularly reference causal explanations. However, I am not committed to causal explanations constituting the only legitimate form of scientific explanation. Far from it. For example, *unificatory explanations* are often employed across the sciences and are genuinely explanatory (Kitcher 1981).

necessarily possess innumerable properties, as will any model. Given that similarity is merely the sharing of properties in common between two objects, the model and target system will invariably share many properties in common. But the purposes which guide the scientist will necessarily restrict the set of properties in such a way as to allow for a useful *similarity assessment* in which only relevant properties are included. From this similarity assessment, the scientist can then use the model to represent the target system and infer information concerning reality. It is in this way that models work.

However, there are potential challenges for this account of the logical structure of scientific modeling. In this chapter, I detail two challenges and demonstrate not only why they are potentially problematic but also precisely why they are defeasible. First, I consider the challenge which holds that similarity cannot be the basis for the logical structure of scientific representation on the grounds that similarity assessments are either nonsensical or contravene scientific naturalism (Suárez 2003).[3] If successful, this challenge would require that representation either be characterized by some other kind of relationship than similarity or else left unexplained. Neither of these alternatives are necessary however, as I demonstrate it is possible to maintain *both* a naturalistic attitude toward scientific modeling and hold that similarity is the basis for the logical structure of scientific modeling.

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[3] Suárez defines scientific naturalism as anti-intentionalism (2003, p.229). For him, in order for an inferential pattern to be genuinely natural, it must not incorporate any intentional considerations. This is a wrongheaded conception of scientific naturalism (Section 2.2.2).

Second, I consider the challenge which holds that similarity cannot be the basis for the logical structure of scientific modeling on the grounds that models are not decomposable (Rice 2019). The challenge asserts that models must be treated as indivisible wholes which precludes the kinds of similarity assessments which I contend are central to understanding scientific modeling. However, this challenge is likewise defeasible by distinguishing between *identification* and *isolation*. Granting that models may be indivisible; I demonstrate that model properties are still identifiable by the scientist. In this way, it remains possible for the scientist to not only determine which properties are relevant based on their guiding purposes but also whether those properties are instantiated in reality. By so doing, it remains possible to accept the potential indivisibility of scientific models and still understand the logical structure of scientific models as ultimately based on relevant similarity.

2.1 Representation

The logical structure of scientific modeling is based on the representational capacities of models. Under particular conditions and when guided by articulable purposes, models enable the scientist to infer information concerning the constitution of reality. The basis for these inferences is representation. Broadly, representation is the process by which the model comes to be a useful substitute for reality. Although formalism can often obscure rather than reveal, it is useful to introduce an abstract formulation of representation:

S uses *M* to represent *T* for purpose *P*

In this formulation, *S* denotes the subject who uses the model, *M* denotes the model, *T* denotes the target system, and *P* denotes the guiding purpose (Giere 2004, p,743). From this formulation, it is possible to identify the major determinants of representation.

Straightforwardly, representation involves the relationship between *model* and *target system*. Models are artificial representations which can be used for scientific inferences according to particular purposes (Section 1.1). When used, models can yield information concerning the constitution of reality, but strictly speaking models do not represent reality. Or at least, models do not represent reality in its totality. Rather, models represent target systems which can be understood as particular temporal and spatial regions (Elliot-Graves 2020). In this way, the target system enables the scientist to focus on particularly relevant constitutive properties of reality in a manner which is conducive to scientific research. However, the scientist is rarely capable of fully conceptualizing the target system, at least initially. Rather, it is through the construction and use of scientific models that the scientist is able to *explore* possibilities and actualities and, in this way, map out the relevant target system.

In addition, representation is determined by both subject and purpose. In this circumstance, the subject refers to the scientist *using* the model. In many instances, a model is constructed by one scientist and then used by another, often in a different location or time. And although the initial constructor certainly has an influence over the particular constitution of the model, it is the subject which determines the particularities of the representation. Specifically, the subject determines the relevant community in which the model is used. Different communities impute different meanings onto identical signs and for this reason, it becomes necessary to identify the subject and associated relevant community in order to understand the representational content of a model. However, the subject is also important because subjects determine which purpose will guide the representational exercise. And purposes determine which properties of the model and the target system will be relevant for the representation relationship.

Given these determinants, the logical structure of scientific modeling can be articulated in terms of *relevant similarity*. In brief, a model represents a target system if and only if the model is relevantly similar to the target system. Here similarity can be broadly understood as the sharing of properties. The subject determines the known set of properties and the purpose determines which of those properties are relevant. From this, the scientist is then able to empirically assess the accuracy of the representation by the extent to which relevant properties are shared between model and target. However, in order to better understand precisely how models work, it will be useful to consider the logical structure in greater detail.

2.1.1 Stability and Convention

Representation requires *constitutive stability*. Without a stable constitution, the content of a model would be constantly changing, rendering the model useless. And for this reason, it would cease to be representative. Here representation is being directly associated with usefulness. Useless models cannot represent. A model which could not be used to understand or control reality would cease to be representative because it could never be used by a working scientist. Similarly, in the political realm the legislature can represent the electorate. However, in order to do so, it must be useful. Usefulness for a legislature includes the construction, debate, and passage of legislation. But an unstable legislature whose membership was constantly being overhauled would be unable to construct, debate, or pass legislation. It would be a useless and hence unrepresentative legislature. In order to ensure the stability and usefulness of a legislature, elections are spaced so that the constitutive membership of the legislature can remain *stable enough* that it can fulfill its purposes. The same holds for models.

Models can represent a target system only when they are constitutively stable enough to be used. Given that models are composed of interpretable signs, the constitution of a model is determined by the meaning of those signs. In other words, for models constitutive stability requires semantic stability. However, meanings can change. Consider the word "awesome" or the word "mayhem". Both of these words have undergone major semantic changes over the centuries.[4] The same semantic drift holds for models. Many models have been developed within one scientific discipline and then been repurposed for another.[5] In this transition, the meaning of the model is changed. But once the model is re-enmeshed in the new scientific community its meaning becomes stable enough for it to be useful. Here constitutive stability can be attributed to the *conventions* which govern the scientific community.[6] Just as the placement of silverware is conventional so is the meaning of particular terms. For example, in economics "*p*" denotes price while in chemistry "*p*" can denote pressure. Neither denotation is inherently more apt, but both are stable within their respective scientific communities due to the regulative power of scientific conventions.

However, constitutive stability is not enough for a model to prove useful. Stable meaning can allow a model to be used by members of a community in the service of scientific purposes such as explanation, prediction, and so on. And yet, unless the model

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[4] In colloquial English, the word "awesome" typically means something excellent, fantastic, or great. However, its original meaning, which is still occasionally used was "arousing or inspiring awe" (OED). Similarly, the word "mayhem" typically now means rowdy confusion, chaos, or disorder while the original meaning was more specifically related to physical harm done so as to prevent self-defense (OED).

[5] In the history of science, numerous models have been repurposed. For example, the Fresnel developed an optical theory based around a model of an elastic solid ether. The model was then repurposed by Maxwell in his model of the electromagnetic field (Worrall 1989, p.117).

[6] Lewis developed an account of conventions based around formal game theoretic models. In his analysis, he defined conventions in the following way:

> Convention turns out to be a general sense of common interest; which sense all the members of the society express to one another, and which induces them to regulate their conduct by certain rules [Lewis 1969, p.3].

Here Lewis details many important elements of conventions. Foremost among these is the centrality of purpose or common interest. Additionally, he directly connects conventions with behavior or conduct and in this way understands conventions as an essentially *useful* activity.

represents the target system, the model will not prove useful. Consider again the legislature. Even if sufficiently stable so as to be capable of constructing, debating, and passing legislation, the legislature could still prove useless if unrepresentative.[7] Suppose that the composition of the legislature was not based off of representative elections but was instead composed exclusively of a hereditary aristocracy which excluded much of the polities populace. Under these conditions, the legislature would not represent the populace and would plausibly not possess the legitimacy needed to ensure a stable and accountable government. Here constitutive stability is insufficient. What is needed is representation which presupposes constitutive stability but goes beyond it. The same, once again, holds for models.

2.2.2 Similarity and Purpose

Representation requires similarity. In order for an object to be a useful substitute for another object, the two objects must be similar to one another. The legislature must resemble the electorate. The portrait must resemble the person. And the model must resemble the target system. Without some similarities between the model and target system, it is unclear in what way the model could be used to infer information

[7] Behind the assertion that political representation is required for a legislature to be useful are the claims that sovereignty derives from the electorate and that representation is required for legitimacy. Given this, a legislature would need to represent the electorate in order to be considered as legitimate. Moreover, legitimacy is being understood as a necessary condition for effective governance. Both the contention that sovereignty derives from the electorate and that political representation is necessary for legitimacy are controversial theses from political philosophy. I will not defend either of these theses here. Rather, they are being presumed in order to make the analogy between political representation and scientific representation work.

concerning the constitution of the target system. However, it is unclear precisely what is meant by "similarity".

Broadly, similarity refers to the mere sharing of properties. From this it follows that a red apple and a red barn are similar as both possess the property of redness. Likewise, it follows that an agile gazelle and an agile panther are similar as both possess the property of agility. And yet, from this broad understanding of similarity certain issues invariably arise. All objects possess innumerable properties. An apple is not only red, it is roughly round, it is carbon-based, it is nutritious, and so on. From this it follows that any two objects will necessarily share *at least* one property in common. The result is that all objects are similar to each other. However, by making all objects similar to each other, similarity becomes useless. On the other hand, just as all objects share at least one property in common, no two distinct objects share *all* properties in common (Goodman 1972, p.443). Consequently, if similarity were to be reconceptualized as the sharing of all properties in common, then no two objects would be similar. The result would, once again, be that similarity becomes useless. To avoid both of these extremes, it is necessary to understand similarity not as the mere sharing of properties, but as the sharing of *relevant* properties (Goodman 1972, p.444).

It is here that purpose becomes most important. Relevancy is understandable only relative to a purpose. Purpose allows the scientist to discriminate between those properties which are important to them and those properties which can be *safely ignored*.

The purpose behind a representation determines which properties are relevant for the similarity assessment and by extension allow models to be useful. For example, consider again a legislature meant to represent the electorate. Both the legislature and the electorate possess innumerable properties such as *average age*, *ethnic composition*, *average educational-level*, *gender distribution*, *ideological distribution*, and so on. Invariably, any legislature will be similar to the electorate in that it will share some property in common. Likewise, it is invariable that the legislature will not share some property in common with the electorate, for example the property of *population size*, at least for a modern representative government. Given this, in order for the legislature to represent the electorate, it must be similar to the electorate along some dimension deemed relevant.[8] And importantly, what is considered relevant can change across time and place. In

[8] For political representation, it is important to distinguish between similarity and delegation. In order for a legislature to be considered a representative body, it must be similar to the electorate in some relevant property. However, this does not mean that the legislature ought to be a microcosm of the electorate or that the best legislature would be constructed by a random sampling of the electorate. Random samplings would be useful only if *all* properties (except population size) were deemed relevant for governance. But this is not the case. Many, if not most, population properties are regularly considered irrelevant for governance. Because of this, elected representatives need only be relevant along a small number of relevant properties. However, it is equally important to recall that representation involves a form of delegation (Section 1.1). Electorates delegate political decision making to legislative representatives. In this, the representatives take on responsibilities which could perhaps be exercised directly by the electorate, in the form of a direct democracy, but which are better conducted by a small number of representatives. In this way, political representation mirrors scientific modeling. It could perhaps be possible in some situations to directly intervene or observe reality (analogous to the electorate), but it is often far more useful to intervene of observe a model (analogous to the legislature). Once again, in order for the analogy between political representation and scientific representation to work, I have had to make a number of controversial claims concerning political sovereignty and legitimacy which I will not defend here. Rather, they are meant only to help make the analogy work.

certain circumstances ideological similarity may be deemed most important while in others, ethnic similarity might be deemed most important, and so on.[9]

Stable relevancy is enforced by conventions. In order for models to be useful, the set of relevant properties must remain stable long enough for the model to be useful. Just as the set of politically relevant properties must remain stable long enough for the legislature to be useful. In art, conventions serve a similar function. For those with the expertise, it is possible to distinguish paintings emanating from different artistic communities. For example, the Chinese painter Chiang Yee travelled to Europe in the 1930's and 1940's. A member of the Chinese artistic community, he painted British landscapes which were readily identifiable as East Asian in origin (Gombrich 1960, p.84). The conventions of Chinese art determined which properties of the landscape were relevant for him to capture in his paintings. However, despite the ways in which the conventions shaped his work, Chiang Yee still created *realistic* paintings (Gombrich 1960, p.84-85). By this I mean, his paintings still had properties in common with the British landscape. In this, the realisticness of the painting was determined by the constitution of reality but seen through particular conventions. And the same holds for

[9] In Lebanon, the legislature is organized on confessional lines in which representation is determined by the religious affiliations of the populace. Originally, this confessional organization was introduced as a temporary measure in order to reduce sectarian violence but it has been in effect for decades. Although Lebanon would not exemplify effective representative government, its confessional organization indicates that it is at least possible for religious identity to be constitutionally relevant for political representation.

models. Although conventions are important, representation is ultimately dependent on the constitution of reality as well as the ways in which the model is used.

In order to understand the logical structure of scientific modeling, it is useful to consider the ways in which models were used to draw scientific inferences. Use is always intimately linked to purpose. And from this, it follows that the assessment of a scientific model is dependent on the purpose behind the representation. In art, this is often explicit. Many artistic creations are made to create an emotive response in the viewer, but some are built with explicitly inferential goals. For example, in early modern Europe, it became a relatively common practice among the aristocracy to commission portraits of potential spouses. The purpose behind such portraits was to convey information concerning the physical appearance of the spouse without the need for in-person meetings. Although commissioning a portrait in order to ascertain information concerning the appearance of another person may seem extravagant, it was considered a reasonable investment given the importance of physical beauty in potential spouses and the high costs of long-distance travel during the period. Given this, such portraits were constructed with an explicitly inferential purpose and could consequently be assessed relative to that purpose.

Oftentimes portraits were determined to be successful given the purposes behind their construction. However, there are notable exceptions which can prove revealing. For example, during the tumultuous reign of the renaissance King Henry VIII of

England, his chief counselor Thomas Cromwell commissioned the famed artist Hans Holbein the Younger to paint a portrait of the German Protestant noblewoman Anne of Cleves. Cromwell was likely motivated by confessional and diplomatic concerns arising from the schism between the Roman and English churches. He sought to establish an evangelical alliance between the English throne and German Protestant princes and in 1539 C.E. seemed on the cusp of success (Moyle 2022). The marriage alliance between King Henry VIII of England and Duke Wilhelm of Cleves would have greatly enhanced the prospects for a working evangelical alliance. In light of this, the commissioned Holbein painting can be understood as designed to *entice* the King by portraying the physical beauty of Anne of Cleves. After viewing the completed portrait, King Henry VIII agreed to a betrothal and Anne of Cleves departed the continent for England. It appeared that Cromwell would succeed.

However, Cromwell failed and eventually ended up paying for his failure with his life. Upon arriving in England, Anne of Cleves met privately with King Henry VIII. Motivated by medieval romantic traditions, the king was underwhelmed by his prospective wife (Moyle 2022). Notably, he considered her physical appearance to *differ substantially* from that represented in the Holbein portrait (Moyle 2022). The disconnect between representation and reality contributed to the fall of Cromwell and with him, the collapse of the nascent evangelical alliance (MacCulloch 2003, p.196). Here, Holbein cohered with the reigning artistic conventions but failed to convey the appearance of

Anne of Cleves because of the objective constitution of reality. Anne of Cleves just did not resemble the portrait, at least in the ways which mattered to King Henry VIII. The relevant *dis*similarity between portrait and person undermined the usefulness of the portrait for King Henry VIII given his purpose of inferring information concerning the physical appearance of a woman he had never before seen in person. Together, purpose, conventions, and reality combined to render the Holbein portrait *useless*. And from the perspective of Cromwell, it was even worse than useless, it was deadly.

Returning to the logical structure of scientific modeling, both similarity, conventions, and purpose combine to allow models to represent target systems. Models are used to draw inferences, can be useful or useless, and are largely determined by the purposes of those who construct and use them. However, in order to better understand precisely how models represent target systems through relevant similarities, it is useful to consider an exemplary scientific model in considerable depth.

2.1.3 Cournot Model

Economic analysis is organized around the *rationality principle*. It holds that agents behave appropriately given particular tastes and constraints (Section 1.1.2). Using this principle, economists have been able to explore various different market interactions and provide compelling explanations and useful predictions. Prominently, economists have long attempted to represent competitive interaction of firms in the marketplace. In order to do so, it has proven useful for economists to construct models. Here, the guiding purpose behind the models has been largely *classificatory* (Section 1.1.1). By creating representations, economists create categories into which different marketplace interactions can be classified. These categories include, but are not limited to, monopoly, duopoly, and perfect competition.

The French mathematician and economist Augustin Cournot first developed these kinds of classificatory models.[10] His purpose was to provide precise definitions for the different categories of marketplace interactions through the construction of mathematical models (1838/1897, p.79). Beginning with a simple narrative in which a single proprietor dominated a natural spring, Cournot developed a mathematical model of monopoly (1838/1897, p.56). From there, he expanded the narrative to include two identical natural springs occupying similar positions each of which is controlled by a rival firm (1838/1897, p.79).[11] Cournot then developed a highly influential mathematical model which represented the interactions of these two firms. Following a modern conceptualization of the Cournot model, the natural spring narrative has been formulated in terms of *homogenous goods* which are perfectly substitutable.

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[10] Cournot himself did not use the term "model". In fact, to describe him as a modeler or his creations as models is an anachronism. However, given the widespread adoption of the term "model" across economics and the sciences during the twentieth century, it is useful to employ the anachronism in this context.

[11] Morgan has analyzed the important exploratory and explanatory roles of narratives in scientific modeling (2012; 2017). Although I have not explicitly referenced narratives in my own account of scientific modeling, it is possible to incorporate narratives into it. Specifically, narratives can be understood as clarificatory devices by which scientists come to better understand their own purposes. Narratives can also be used as communicative and pedagogical tools by which others learn how to use models.

Here it is useful to clarify precisely what is meant by homogeneity and perfect substitutability. Given two goods, it is possible for consumers to substitute them for one another. Automobiles and bicycles are substitutes as both can be used for transportation. However, for most purposes, automobiles and bicycles are imperfect substitutes. If I were to try and travel across the continental United States, an automobile would be a far more effective vehicle, yielding a faster and likely more comfortable trip. Similarly, if I were commuting in a congested metropolitan area, a bicycle would likely be a far faster option as I could more easily avoid traffic. However, for certain consumers some goods can genuinely be perfectly substitutable in that they could both be used equally effectively for all the same purposes. For example, both Coca-Cola and Pepsi-Cola are interchangeable beverages. For those like myself who lack a discerning palate, these two beverages are perfectly substitutable. And yet for those more attuned to the subtle differences in flavor between Coca-Cola and Pepsi-Cola, the two would be, at best, imperfect substitutes. From this it follows that homogeneity is not an absolute property but is one which changes with the perspective of the relevant user.

For the Cournot model, homogeneity is important because it simplifies the consumer decision. Given two perfectly substitutable goods, the consumer cannot, by stipulation, differentiate the goods on any basis except for price. And following from the rationality principle, the consumer will tend to prefer the less expensive perfectly substitutable good. It follows from this that there will exist a *uniform market price*, denoted by p. Behind this is a relatively simple dynamic. Any firm which charged more than its rivals would lose market share and associated profits. To prevent this, all firms would settle on a single uniform price which was simultaneously profitable and too low to undercut. On the other hand, if the goods were not homogenous, then firms could differentiate their products and charge higher prices. This dynamic explains the efforts of firms to differentiate their products through branding and marketing campaigns.

From the uniform market price, it follows that there exists a *demand schedule* which represents the capacity of consumers to purchase goods conditional on a particular price. Following from the rationality principle, it is a general result that as prices decrease the quantity demanded increases. However, in order to determine the overall status of the industry, it is necessary to determine not only the demand schedule but also the *supply schedule*. From the rationality principle, it becomes possible to construct constrained optimization problems for each firm in which it attempts to maximize profits given two constraints. First, firms are constrained by the total production of all other firms in the industry. Second, firms are constrained by the costs associated with production. In this way, the constrained optimization problem for the firm is a partial function of the constrained optimization solution for all other firms in the industry. Or in other words, all of the firms depend upon each other and any change in output by one firm will affect the productive calculus of all other firms in the industry.

Homogeneity of goods results in a straightforward constrained optimization problem in which each firm *i* attempts to maximize its own profits given constraints. These constraints include (i) internal costs and (ii) production of other firms.[12] Formally, it is possible to represent firm production:

$$q_i = \frac{(a-c)}{(n+1)} \qquad \text{for } i = 1, 2, 3, \ldots, n \tag{2.1}$$

Here q_i denotes the production for firm *i*. Additionally, *c* denotes the internal costs and *n* denotes the total number of firms in the industry. Finally, *a* denotes the quantity demanded as the market price approaches zero. In order to determine the industry market price and industry output, it is useful to aggregate across all firms. To simplify the aggregation calculations, it is useful to assume that all firms face identical internal costs, although this can be relaxed. Given this, it is possible to formally represent industry production:

$$Q = nq_i = \left(\frac{n}{n+1}\right)(a - c) \tag{2.2}$$

Here *Q* denotes industry production. And from this, it is relatively straightforward to calculate the market price as a function of industry output:

$$p = a - \left(\frac{n}{n+1}\right)(a - c)$$

[12] The proceeding exposition of the Cournot model is largely derived from class notes by Professor Curtis Taylor. Many thanks to Alexander Marsh for forwarding these notes as well as for many illuminating discussions concerning this model and economic methodology.

$$= c - \left(\frac{a-c}{n+1}\right) \qquad (2.3)$$

Here p denotes the market price. Underlying this representation is the insight that market price is equal to maximum quantity demanded as the price approaches zero and the quantity actually supplied. From these two equations, it is possible to determine both industry output and market price which amounts to the industry equilibrium.

With the Cournot model characterized, it is possible to use it. Although there are many possible uses for it, one important use is classificatory. By altering the total number of firms in the industry, it is possible to represent different kinds of marketplace interactions. This amounts to a *categorization*. First, by reducing the number of firms to one, the Cournot model becomes a representation of monopoly:

$$Q = \left(\frac{1}{2}\right)(a - c) \qquad (2.2^{M})$$

$$p = c - \left(\frac{a-c}{2}\right) \qquad (2.3^{M})$$

Second, by adding an additional firm, the Cournot model becomes a representation of duopoly:

$$Q = \left(\frac{2}{3}\right)(a - c) \qquad (2.2^{D})$$

$$p = c - \left(\frac{a-c}{3}\right) \qquad (2.3^{D})$$

And finally, as the number of firms in the industry approaches infinity in the limit, the Cournot model becomes a representation of perfect competition:

$$Q = \left(\frac{\infty}{\infty + 1}\right)(a - c)$$

$$= (a - c) \tag{2.2PC}$$

$$p = c - \left(\frac{a - c}{\infty}\right)$$

$$= c \tag{2.3PC}$$

The modularity of the Cournot model allows the economist to classify different marketplace interactions. The categories of “monopoly”, “duopoly”, and “perfect competition” are derivable and definable through the model. From this classificatory use follows an important explanatory use. Despite the seeming differences between monopoly, duopoly, and perfect competition, by using the Cournot model it is possible to determine an underlying common structure. In this way, the Cournot model provides a unificatory explanation (Kitcher 1981).

Additionally, it is possible to use the Cournot model to predict. Given both the maximum market size *a* as well as the internal costs *c*, it is possible to determine both the industry output and market price for monopoly, duopoly, and perfect competition:

Table 1: Cournot Classification of Competition

	Monopoly	Duopoly	Perfect Competition
Parameters	1 80 20	2 80 20	∞ 80 20
Variables	30 50	40 40	60 20

In Table 1, it is possible to represent the three different kinds of market interactions by changing only the number of firms. The other parameters remain constant. From even this simple algebraic exercise, an important predictive result emerges. The number of firms is directly related to the quantity supplied and inversely related to the market price. Consequently, monopolies supply less but for a greater relative to either a duopoly or a perfectly competitive industry. Using this, the economist can generate imprecise but powerful counterfactuals regarding how the industry equilibrium will react to changes in the number of firms.

However, the use of the Cournot model is limited. In order for the model to represent a target system, the target industry must have homogenous goods. Here homogeneity can be considered a *relevant property* and in order for the model to be useful, it must be relevantly similar to the target industry in regard to the homogeneity of the goods produced and sold in the industry. Without homogeneity, it would not be reasonable to postulate a uniform market price. This would contravene the basic structure of the Cournot model, rendering it inapt to represent the target industry. In this way, analyzing the internal composition of the model and using it for different scientific purposes, it becomes possible to determine which properties of the model are relevant. It is possible to construct an economic model which does not require homogenous goods, but that would be a different model that the one presented here. In order to use the Cournot model, it must be relevantly similar to the target industry and, in this case, the homogeneity of goods can justifiably be considered a relevant property.

2.2 Challenges

Representation is dependent upon both the purposes which guide the scientist and the constitution of reality. Purposes determine which properties of the model and the target system are relevant and from there it follows that reality ultimately determines whether those relevant properties are instantiated in both the model and target system. In this way, models can be used for inferences such as explanation, prediction, exploration, classification, and measurement.

Articulating the logical structure of scientific modeling in terms of representation constituted by relevant similarity is a not an uncommon position in the relevant literature. Various philosophers and scientists have constructed accounts which are largely along the same lines to the one which I have developed throughout this account

(Weisberg 2013; Strevens 2008). It is however worth noting the underlying relationship between my favored account and the one developed by Mary Hesse. She articulated the logical structure of modeling in terms of *analogical reasoning* (1963, p.64). For her, there were three kinds of analogies which could be used to draw inferences. Positive analogies were predicated on previously known similarities between model and target system. Negative analogies were predicated on previously known dissimilarities between model and target system. And finally, neutral analogies were predicated on unknown potential similarities between model and target system. Hesse contended that it was through *exploring* neutral analogies that scientists were able to learn about the constitution of reality. Although I have opted for the language of relevant similarity rather than analogical reasoning, analogies are ultimately based upon relevant similarities and relevant similarities are used in analogical reasoning (Hesse 1963, p.75) Both accounts are two sides of the same coin.

However, there are arguments which purport to undermine the viability of the account I have articulated. These challenges are designed to demonstrate that the logical structure of scientific modeling cannot be understood in terms of relevant similarities between model and target system. Given this, in order to better justify my preferred account, I will detail two of these challenges and then demonstrate in which ways they can be defused.

2.2.1 Naturalism

First, the *naturalistic challenge* purports to demonstrate that understanding representation in terms of relevant similarity is either nonsensical or contravenes the need for scientific naturalism. It has been articulated by Mauricio Suárez who sought to undermine the viability of similarity accounts in order to justify his own deflationary position (2004). In brief, his preferred account attempts to understand scientific representation not in terms of any substantive relationship between model and target system but in terms of the inferential process for which models are used. For Suárez, models are used for inferential purposes but that is all there is to say. He denies the possibility of explaining or understanding the inferential process. It is simply a brute fact which must be accepted (2004, p.770). Throughout, his position is explicitly pessimistic. It is predicated on the impossibility of articulating and justifying an account of scientific representation which can explain the inferential process. To that end, Suárez develops a negative argument which purports to demonstrate the impossibility of understanding the logical structure of scientific modeling in terms of relevant similarity (2003).

Throughout, his strategy is straightforward. He argues that similarity is a problematic basis for scientific representation. He then proposes a solution to the problem but argues that to invoke his proposed solution would require abandoning scientific naturalism. It is here useful to introduce more precisely what Suárez means by scientific naturalism:

> [The] weak form of naturalism… merely claims that science can study representation; and [the] stronger form of naturalism, which I employ… claims that the relation of representation does not involve in any essential way agent's intentions and value judgments, but appeals only to the facts [Suárez 2003, p.229].

Given this, Suárez is able to contend that any conceptualization of similarity which appeals to purpose contravenes scientific naturalism. However, as I have argued throughout this chapter, representation is in part constituted by purpose (Section 2.1). To divorce representation from purpose is nonsensical, but by setting the goal posts in this way, Suárez is able to construct five arguments which allegedly undermine understanding representation in terms of similarity (2003).

First is the *variety argument* (2003 p.231). Suárez argues that similarity is not a sufficiently versatile basis to account for the diversity of scientific representations. For the sake of both brevity and charity, it is useful to consider only the example which Suárez considers most demonstrative. In it, he presents a mathematical model from quantum mechanics and argues that it is not similar to any physical system in any relevant way (2003, p.232). Here, the specifics of the model are unimportant. For Suárez, the argument is predicated on the *mathematical* medium of the model and his contention that mathematics is wholly dissimilar from non-mathematical target systems. Ignoring for the moment that many scientific models are not expressed in mathematical media; it is untrue that mathematical objects *in models* are always not relevantly similar to target systems.

Consider the Cournot model. It is expressed in a mathematical medium of algebraic equations. For Suárez it would be considered wholly dissimilar to target industries. And yet, there are numerous plausible similarities between the mathematical model and a target industry. For example, in the model, the variable n denotes the total number of firms in the target industry. Suppose that n is set equal to two. And then suppose that the Cournot model is used to represent the international aeronautics industry which is dominated by two firms: Boeing and Airbus. Here, the Cournot model is relevantly similar to the aeronautical industry. Mathematics *qua* mathematics may not be similar to non-mathematical target systems, but mathematical objects *qua* model parts can be similar to target systems.

Second is the *logical argument* (2003 p.232). Here Suárez argues that similarity and representation possess different logical properties, which precludes them being identical. For him, similarity is a reflexive and symmetric relationship. A red apple is similar to itself. And a red apple is similar to a red barn and a red barn is similar to a red apple. However, Suárez then claims that representation is neither reflexive nor symmetric. For him, a red apple cannot represent itself. And nor can a model. Granting the non-reflexivity and non-symmetry of representation, it does not follow that similarity cannot be used to understand representation.[13] It is true that given the difference in logical properties between similarity and representation that the two cannot be identical, but it is still possible for similarity to be partially constitutive of representation. It is purpose which can endow representation with its non-reflexivity and non-symmetry. Models are just not often used to represent themselves and models and target systems are rarely used to represent models. This is not due to the logical properties of similarity but rather the purposes which guide scientific modeling.

Third is the *misrepresentation argument* (2003, p.233). For Suárez there are two kinds of misrepresentations which undermine the dependency between representation and similarity: mistargeting and inaccuracy. Mistargeting occurs when a model is used to represent the "wrong" target system. For Suárez, the possibility of mistargeting invites the possibility that a model could be similar to a target system but not be intended to represent that target system. From here, he concludes that similarity cannot be all that there is to representation. In this he is, once again, wholly correct. To illustrate his argument, consider again artistic representation. Imagine two identical twins: Anna and Beatrice. Given the similarities between the twins, it is possible that a portrait of Anna would be similar to Beatrice. However, because the artist did not *intend* to represent Beatrice, any attempt to represent Beatrice with the portrait would be an

[13] Plausibly, representation could be understood as both reflexive and symmetric. Reflexive representations may well be trivial but that does not undermine its reflexivity. Similarly, it is possible that representation is symmetric. Consider the case of fashion models. These individuals serve both as the target of representation as well as a representation for an abstract category.

instance of mistargeting. It is the purpose of the portrait which is important. Similarity without purpose could allow for mistargeting, but once purpose has been incorporated into representation, mistargeting can either be avoided or defused.

Inaccuracy occurs when a model differs from a target system. Suárez concedes that similarity is compatible with inaccuracies, unlike isomorphic accounts of representation. However, mere compatibility is not strong enough. Understanding representation in terms of similarity entails that there will always be some dissimilarities (Section 3.3) But it does not follow that these dissimilarities will be relevant. Nor does it follow that, given a particular degree of precision, the dissimilarities will constitute inaccuracies (Section 1.1.3). It is possible for a dissimilarity between model and target system to be accurate given a sufficiently imprecise measure as precision is determined by purpose. Suárez contends that the mere identification of dissimilarities is not always scientifically valuable (2003, p.235). For him, it is often more important to *explore* the dissimilarities and, in so doing, better understand the target system. Here, Suárez is reminiscent of Hesse and her emphasis on exploration through neutral analogies (1963, p.75). However, the difference between Hesse and Suárez is that she recognizes that exploration is a process of uncovering relevant similarities while Suárez denies the importance of similarity and with it, the prospect for exploration.

Fourth is the *non-sufficiency argument* (2003, p.236). Here Suárez, correctly notes that it is possible for a model to be similar to a target system without representing it. He concludes from this that similarity is not sufficient for representation. This is correct. Similarity is partially constitutive of representation but is not identical to it. Purpose is also required. Consider the international aeronautics industry. Dominated by two firms, it is plausible to represent it using the Cournot model. However, the Cournot model requires that the goods in the industry be homogenous (Section 2.1.3). For certain purposes, Boeing and Airbus produce homogenous goods but for other purposes, the goods are differentiable. Here purpose determines whether or not the model represents the target industry. Suárez is correct in so far as similarity without purpose is insufficient for representation, but this indicates that similarity is constitutive of representation but not identical to it.

Fifth is the *non-necessity argument* (2003, p.235). In it, Suárez argues that similarity cannot be necessary for representation because he considers similarity to be an inferentially useless relationship. Underlying this contention is the observation that any object can be similar to any other object (Section 2.1.2). From this, he concludes that similarity would be trivial and should therefore not be included in the logical structure of scientific modeling. However, by restricting similarity to only those properties deemed relevant, it is possible to avoid the charge of triviality. Suárez grants this (2003, p.235). But he contends that relevant similarity presupposes purpose. Here Suárez is, once again, correct. His error arises from denying that similarity and purpose are *both* partially constitutive of representation. But he does present a principled reason to exclude purpose from an understanding of the logical structure of scientific modeling, it contravenes scientific naturalism.

Scientific naturalism is presented as a principled requirement for an adequate understanding of the logical structure of scientific modeling. For Suárez, there are two plausible ways to understand this requirement (2003, p.229). *Weak naturalism* holds that the target system can be studied scientifically. *Strong naturalism* holds that the target system be studied independently of purpose. Implicit in strong naturalism is the presupposition that purpose is "unnatural". However, this is a contentious position. Trivially, purpose can be considered natural in so far as *homo sapiens* are naturally occurring and possess purposes. More substantively, the social sciences are predicated on the intentionality and purposes of social agents (Popper 1976, p.91). By accepting strong naturalism, social science would need to study social behavior independently of intentionality and purpose, which would make it no long genuinely socially scientific. For this reason, strong naturalism ought to be rejected. It is a wrongheaded principle. And by rejecting strong naturalism, the negative argument presented by Suárez collapses. He created a dilemma. Either abandon similarity or abandon strong naturalism. Given that there are good reasons to abandon strong naturalism, the dilemma dissolves.

However, representation can still be real. Charitably, the worry motivating Suárez is that if representation were unnatural then it would be controllable by scientists. In the extreme, this kind of control would allow the scientist to represent by stipulation. By restricting representation to "just the facts", Suárez seems to be trying to exclude stipulative representation (2003, p.229). But all that is required to prevent stipulative representation is that reality "pushes back" when needed (Section 1.1.2). Purposes determine which properties are relevant but it is the constitution of reality which determines whether those relevant properties are instantiated in the target system.

2.2.2 Decomposition

Second, the *decompositional challenge* purports to demonstrate that scientific models are not decomposable into constituent parts and properties, rendering it impossible to differentiate between similar and dissimilar properties and relevant and irrelevant properties. It has been articulated by Collin Rice who used the challenge to motivate his holistic account of scientific modeling (2019). For him, scientific models not decomposable (2018, p.2809). He focused instead on how models are used to represent disparate target systems in a process he called *universality* (2018, p.2818). However, because Rice conceived of scientific models as inviolable, he lacked the analytic resources to explain how models can represent target systems, leaving an important stage of the universality process as primitive. In this, Rice mirrors Suárez. Both look to the uses of scientific models all the while denying the possibility of understanding those uses in terms of representation.

Returning to the decompositional challenge, it is predicated on the alleged *indispensability* and *pervasiveness* of mathematical frameworks (2019, p.189).[14] Pushing aside, once again, that many models are not mathematical, Rice is correct that mathematical models do depend on mathematical frameworks in an important way. Although it is difficult to articulate precisely what is meant by a "mathematical framework", it is useful to think in terms of operational rules. In order to use a model, the scientist must manipulate the artifact according to some procedure. For example, in the Cournot model, the economist must represent the profitability of an individual firm and then use the operational rule of constrained optimization to determine how the firm will behave. Here, the operational rule is related to an abstract principle (see Section 1.1.2). Specifically, constrained optimization relates to the rationality principle which holds that agents will behave appropriately given particular tastes and constraints (see Section 1.1.2). The same holds for other scientific principles. When an operational rule is instantiated in mathematics, it can reasonably be considered a "mathematical framework".

Granting that mathematical frameworks are indispensable; it does not follow that they are pervasive. It is perhaps unclear what is meant by both indispensability and pervasiveness. Here, indispensability can be understood as simply the observation that

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[14] Rice argued that both mathematical frameworks and difference-makers were indispensable and pervasive (2019 p.194). Given that the argumentative strategy is the same for both mathematical frameworks and difference-makers, it is possible to assess the argument by focusing exclusively on mathematical frameworks.

scientific models would not work without mathematical frameworks. Trivially this is incorrect. Many scientific models are not mathematical and do not have a mathematical framework. From this it follows that *mathematical* frameworks are not indispensable, at least for all scientific models. For some models, the mathematical framework might be genuinely indispensable. However, operational rules can plausibly be considered indispensable for all models. Given that an operational rule is just the process by which a model is used, it does follow that in order to use a model there must be an operational rule. When operational rules are instantiated in mathematics as mathematical frameworks, then they are indispensable. It is worth noting that indispensability can be understood only in relation to purpose. Models without purpose cannot be used and nothing is indispensable for an impotent artifact. But, given all this, indispensability does differ substantially from pervasiveness.

Broadly, pervasiveness could be understood in terms of *contamination*. In fact, Rice speaks about quarantine in order to illustrate the concept (2019, p.190). However, it is important to distinguish indispensability and contamination. Many things which are indispensable are not contaminants. To be a contaminant, something must be counterproductive to the point that it would be useful to contain it.[15] Diseases illustrate this well. In so far as diseases are counterproductive for health outcomes, it is often

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[15] Rice does not endorse the “contaminant” view of pervasiveness in so far as he does not consider mathematical frameworks to be counterproductive. Rather, he sees them as useful (2019, p.196). However, he does employ the “contaminant” language and it is useful to use thing language to clarify what is meant by pervasiveness.

useful to contain them. To do so, it is often advisable to first *identify* the disease and then to *isolate* it. Importantly, these are two distinct activities. Return to the quarantine example. Effective quarantines proceed by first identifying those individuals exposed to the disease and then isolating those individuals from the population. However, Rice fails to draw this distinction and runs identification and isolation together in his understanding of pervasiveness. He argues that mathematical frameworks cannot be isolated and by extension concludes that mathematical frameworks cannot be properly identified (2019, p.189). However, identification is possible without isolation. Consider genetic diseases. First consider an individual level analysis. Genetic diseases are often pervasive *within* an individual person. Because of this, it is often impossible to isolate the disease, although it is often possible to identify it. Second, consider a population level analysis. Due to the transmission pathways, it is often unnecessary to isolate individuals with genetic diseases from the population. However, it is still possible, and often even advisable, to identify such individuals. Here, the isolation and identification come apart in a way not found in Rice.

Given the distinction between identification and isolation, the decompositional challenge can be defused. Granting for the moment that mathematical frameworks are contaminants that it would be advisable to isolate and granting for the moment that mathematical frameworks cannot be contained, it does not follow that mathematical frameworks cannot be identified. In fact, it is often a straightforward process to identify the relevant mathematical frameworks. In the Cournot model, the use of constrained optimization was evident from both the rationality principle and the basic constitution of the model. It did not require any great investigative leaps to identify the mathematical framework. And, importantly, identification is all that is required for relevant similarity. Representation requires that the scientist *identify* properties in the model and the target system and then determine whether those properties are relevant. It does not require that these properties be isolated from one another. Given this, the decompositional challenge does not preclude understanding the logical structure of scientific models in terms of relevant similarity. In fact, the decompositional challenge is no challenge at all.

However, it is not just that the decompositional challenge can be defused, it is that it is *wrongheaded*. In fact, the decompositional challenge is wrongheaded in two distinct ways. First, it is not only possible to isolate mathematical frameworks, it is necessary to do so. Here is it useful to step back and consider how reality must be constituted in order for models to work. Given that models work by identifying relevant similarities between model and target system, reality must be constituted so that it can be decomposed into identifiable properties. Rice is explicit about this (2019, p.181). However, this idea can be traced back to Simon and Rescher who posit two metaphysical principles which they consider necessary for causal identification (1966, p.330). First, the *principle of prepotence* holds that certain causal relationships are sufficiently weak to be safely ignored. The result is a nearly decomposable system which can, for most purposes, be treated as decomposable (Simon 1962, p.474). Second, the *principle of independence* holds that most constituents of reality are not causally related, yielding a genuinely decomposable system.

Decomposability is important because without it, scientists would be unable to effectively intervene on reality in a way which could isolate those causal relationships of interest to them (Simon and Rescher 1966, p.330). In this way, decomposability can be understood as a metaphysical requirement for useful models because without it, models could not be used to provide insight into the constitution of reality. In fairness, Rice conceded that reality could be decomposed, but it is no mere concession (2019, p.189). The decomposability of reality is a requirement for successful modeling. But even more so, by denying that models can be decomposed similarly to reality, Rice denied the possibility that models can represent reality. Given that reality *must* be decomposable in order to be modeled and that models must be decomposable in order to represent a decomposable reality, it follows that non-decomposable models cannot represent reality. This follows from the plausible requirement that a decomposable model is required to represent a decomposable target system. Rice is explicit on this point. He denied the possibility of mapping or representing reality using models (2019, p.190). For him, this denial follows from the decompositional strategy but it not only precludes the decomposability of models; it makes representation itself impossible.

Second, the decompositional challenge is wrongheaded because it is predicated on mathematical frameworks always being contaminants. Throughout, Rice posited that indispensable mathematical frameworks were contaminants which distort scientific models (2019, p.190). From this he concluded that scientific models are *holistic distortions* (2019, p.198). However, it is unclear on what basis one could consider a mathematical framework a distortion. For Rice, it is abundantly obvious that mathematical frameworks are dissimilar from reality (2019, p.189). But in order to justify this conclusion one would need prior knowledge of reality which could be ascertained independently of the scientific model. Not only is this implausible, but it undermines the usefulness of scientific models in the first place (Section 3.3). Rice seemed to presume that scientists were able to determine the constitution of reality without models but failed to provide any reason why scientists would then use models to investigate the constitution of reality. In so far as models are used for exploration, then the presumption that any particular mathematical framework must be a distortion is wrongheaded. It is through using models that scientists come to determine the constitution of reality, it is not known prior to investigating.

2.3 Conclusion

In order to articulate and justify the logical structure of scientific models, it is useful to consider *how* models are used. Throughout this chapter, I have defended the simple claim that models work by representing reality. Or more specifically, models work by representing target systems. However, in order to better understand the means by which models work, I have broken down the representational relationship into its constituent parts: *subject*, *model*, *target system*, and *purpose* (Section 2.1). Models are artifacts used to draw inferences about the constitution of reality and target systems are temporal or spatial slices of reality. Subjects use models and through so doing, determine the particular conventions which govern the constitution of the model. And finally, models are always used for a purpose. Although there are many purposes for which scientific models can be used, my primary focus has been on explanation, prediction, exploration, classification, and measurement. Together, these four parts constitute scientific representation.

However, in order to understand the logical structure of scientific models, it is useful to consider under what conditions representation occurs. Specifically, I identified two interrelated conditions. First, the meaning of the model must remain *stable enough* for them to be used (Section 2.1.1). And second, models must be *relevantly similar* to the target system (Section 2.1.2). Here similarity is understood as the sharing of properties in common. But in order to avoid similarity becoming useless, it must be restricted to only those properties deemed relevant by the subject given the articulable purpose. Given all this, it is possible for models to work. And, in order to demonstrate how models often do work, I detailed the Cournot model (Section 2.1.3). To do so, I first explained the purposes for which the model has been used, including classifying different marketplace interactions, and explaining firm behavior. From there, I detailed the constitution of the model and how that constitution, in part, determined which properties of the Cournot model were relevant.

Finally, in order to both better articulate and justify how models work, I considered two different challenges. The first was posited that similarity was either useless or contravened scientific naturalism (Section 2.2.1). However, against this challenge, I demonstrated that similarity could be made useful by restricting it to only relevant properties and that this was accomplishable by incorporating purpose into representation. Additionally, I argued that doing so did not contravene any reasonable understanding of naturalism. The second challenge denied the possibility of decomposing models which would preclude the identification of relevant properties constitutive of similarity (Section 2.2.2). However, by distinguishing between identification and isolation, it becomes evident that properties can still be identified even if they cannot always be isolated. But even more so, models must be decomposable as reality must be constituted in such a ways as it can be decomposed by scientists. The result is not only a better understanding of representation but an assurance that *models can be used*.

In order to fully understand the logical structure of scientific models, it is useful to explore under what conditions scientific models can be considered *ideal*. In the next chapter, I argue that the ideal model is useful and that in order for a model to be useful, it must be idealized (Section 3.1). Here idealization is commonly identified with the purposeful distortions which motivated Rice's challenge. However, I argue that in order to understand something as a distortion, there must exist some undistorted alternative which is complete. But, as I argue, there is no complete model as completeness is a false modeling ideal (Section 3.3). Rather, the ideal model is one which can be used, given a particular purpose.

3. Ideal Models

The ideal model is useful. Here usefulness can be understood in relation to the particularities of the model and the purposes guiding the scientist. There is no single ideal model for any particular target system, only a model which is ideal for a specific subject to use to represent a specific target system for a particular purpose. Given this, the logic of scientific modeling can be articulated by examining how scientists work to achieve general inferential purposes such as explanation, prediction, exploration, classification, and measurement (Section 1.1.1). And yet, in order for a scientist to use a model, it must represent the target system, it must be relevantly similar to the target system (2.1.2). However, representation seems incompatible with distortions by which the model systematically diverges from reality. Known as idealizations, these distortions purport to undermine the capacity of scientists to use models for inferential purposes. But nothing is farther from the truth.

Idealizations are useful. Idealizations are what make models capable of explanation, prediction, exploration, classification, and measurement. It is in this way that idealized models are ideal. Given this, the logical structure of scientific modeling can be understood by considering the different ways in which scientists use idealizations in order to achieve their purposes. From this it is possible to categorize different *idealization strategies*. Explanatory idealizations can isolate relevant causal or unificatory properties. Predictive idealizations can provide computationally tractable models which can then be used for both counterfactual predictions and forecasting. Exploratory idealizations can be used to map out conceptual space and provide embarkation points for future modeling. Classificatory idealizations impose an ontology onto reality in order to allow for the creation of useful data models. And finally, measurement idealizations simplify the target system in order to make it amenable to systematic measurement.

However, it is not just that idealizations are useful. In order for a model to work, it *must* be idealized. And yet, many understandings of scientific modeling do not properly acknowledge the importance of idealization and this can be largely attributed to the prevalence of a false ideal. For many, the ideal model is complete in so far as it is perfectly similar to reality. However, completeness is neither achievable nor aspirational. Between conceptual incoherence and operational intractability, few are under any illusion that complete models are constructable or usable. And yet, it is more than that. A model which cannot be used is clearly useless. And a useless model should not be considered ideal. In this, completeness is not only a false ideal, it is a counterproductive one. Models work not because they are more or less complete but because they are idealized.

3.1 Idealization

Commonly, idealizations are understood in terms of *purposeful distortions*. Here the model is taken to diverge systematically from reality in a manner which precludes or complicates the representational relationship between model and target system (Weisberg 2013, p.98; Strevens 2008, p.297). From this, idealizations can plausibly be identified with dissimilarities between model and reality which are often understood to yield inaccurate models (Section 2.2.1). From this alleged inaccuracy, many have concluded that idealizations are *contaminants* which ought to be isolated and mitigated (Section 2.2.2; Strevens 2008, p.315). Given this common understanding of idealization, it follows that the logic of scientific modeling ought to provide an understanding for how idealizations can be isolated or mitigated. However, while idealizations are purposeful, they are neither distortions nor contaminants.

Rather, idealizations are useful. Specifically, idealizations are purposefully and strategically incorporated into models in order for scientists to be able to draw particular kinds of inferences. In this way, idealizations should not be thought of as contaminants but rather in terms of useful strategies for making models work. Given this, in order to articulate the logical structure of scientific modeling, it is useful to provide a classificatory system for understanding *idealization strategies*. Buttressing the classificatory system are the major purposes which guide scientific modeling: explanation, prediction, exploration, classification, and measurement (Section 1.1.1). Each of these purposes determines both how an idealization is incorporated into a model and how the idealization is then used. Following from this, it is useful to consider each of these idealization strategies in turn.

3.1.1 Explanatory Idealization

In order for scientists to use models for explanatory purposes, it is useful for them to construct abstracted models (Section 1.1.2). Abstraction can prove useful for explanation in a number of ways. First, it becomes possible for scientists to isolate relevant causal properties (Potochnik 2017, p.52; Mäki 2009, p.31). In this way, the idealization helps the scientist identify which properties are relevant and then determine the important counterfactuals associated with those relevant properties. For example, in causal modeling, scientists often employ highly simplified models in order to represent identifiable and relevant causal properties. The properties are then often incorporated into directed acyclic graphs which are then used to generate causal explanations. However, in order for causal models to work, they must be abstracted. Idealizations allows the scientist to ignore negligible causal relations and non-existent causal pathways (Section 2.2.2; Simon and Rescher 1966, p.330). Through this process, the scientist is able to construct a useful representation of the causal process. For this reason, causal models work because they are idealized.

Second, idealizations can be used to identify widely-shared properties. Here, scientists can generate explanations by grouping together seemingly radically different target systems through the identification of a property held in common by all of them. These widely shared properties are sometimes referred to as *universality classes* (Batterman and Rice 2014, p.374). However, in order for the scientist to identify the relevant universality class, they must abstract each representation of the target systems through a process referred to as renormalization (Batterman and Rice 2014, p. 363). In this way, the idealization allows the scientist to identify the universality class and generate explanatory groupings. Here the idealization acts to provide a classificatory system which is itself useful and explanatory.

3.1.2 Predictive Idealization

In order for scientists to use models for predictive purposes, it is useful for them to introduce idealizations which make the models tractable. Here it is useful to distinguish between two different kinds of idealization strategies. First are *counterfactual predictions* whereby the scientist determines how a system will evolve in response to certain counterfactual interventions. Given the structural similarity between counterfactual prediction and causal explanation, those idealizations which are useful for causal explanation are similarly useful for counterfactual prediction.[1] Second are *forecasts* in which the scientist attempts to determine the future state of a system regardless of any interventions. Forecasts are often used in finance or meteorology and are often involved in machine learning exercises. Here, it is often more important that the model be accurate and tractable rather than causally similar to the target system. But

[1] Explanation and counterfactual prediction share important structural similarities but are often considered different. From these similarities have arisen certain problematic cases such as the "flagpole" example (Hempel 1965, p.366-376). The difference between the two is one of purpose. Prediction and explanation are motivated by different goals, but the underlying structural requirements in order to explain or construct a counterfactual prediction are highly similar. For this reason, it makes sense that explanatory idealizations are largely interchangeable with counterfactual predictive idealizations.

in order for a scientific model to be tractable, it must abstract from reality. Without abstraction, the model would be so complex as to be unwieldy. In such a circumstance, the model could not be used, rendering it useless, without idealizations.[2]

3.1.3 Exploration Idealization

In order for scientists to use models for exploratory purposes, it is useful for them to introduce provisional idealizations and then develop upon them in order to construct more complex and targeted models. Here is it important to note that scientists rarely if ever have a perfectly articulated research agenda at the outset of a research program. Rather, it is through the construction and use of models that scientists explore conceptual space. In this way, modeling is analogous to exploration. Unlike mere travel, exploration is predicated on ignorance. Explorers from Columbus to Hudson did not know precisely where they were going but made provisional ventures to uncover new lands and in so doing, open up new avenues for future exploration and discovery. In this, exploration and modeling can be decomposed into three distinct activities: *embarkation*, *discovery*, and *mapping*.

Embarkation is the first stage of any successful exploration. However, embarkation is no guarantee of success. Many a would-be explorer has embarked but

[2] Weisberg distinguishes between minimalist idealizations and Galilean idealizations (2007). For him, minimalist idealizations isolate causally relevant properties and can be identified with explanatory idealizations. Galilean idealizations are introduced in order to improve the tractability of the model and can be roughly identified with forecasting idealizations. Weisberg characterizes Galilean idealizations as eventually eliminable (2007, p.641). However, this claim is founded on the false ideal of completeness and should be rejected (Section 3.3).

turned home after facing an obstacle. But regardless, all explorations must begin with an embarkation. And just as explorers embark, so too do modelers. It is for this reason that the earliest models in a research program are usually quite simple. In order to begin somewhere, it is useful to begin somewhere simple. For example, in early geological modeling, the surface of the Earth was represented as a collection of twenty *perfectly rigid* blocks (Hallam 1973, p.68). From this starting point, geological modelers were then able to safely ignore the internal composition of tectonic plates and explore the interactions between tectonic plates. Following from these early idealized models, geologists developed more precise models which represented both the interactions between tectonic plates as well as the internal composition of major tectonic plates. Here the idealization enabled the scientists to begin representing the surface of the Earth and from there explore the conceptual space within geological modeling.

3.1.4 Classificatory Idealization

In order for scientists to classify reality into useful categories, it is useful for them to represent reality using idealized models. Reality does not come with categories or with a classificatory system (Section 1.1.2). Rather, it is through representation that scientists can group together objects into categories which can prove useful for understanding reality. In this way, classification allows the scientist to interact with reality in a systematic manner characteristic of scientific research. Given that models are representational, they serve as a useful place for both conceptualizing and imposing classifications which can then be used. With these categories, scientists are then able to construct data models which can be used for the empirical assessment of scientific models (Section 1.1.2). Throughout the classificatory process, idealizations prove useful for abstracting reality in a way that makes it possible for the scientist to generate categories which are useful.

For example, biological systematics is predicated on idealized models. Given the complexity of biological reality, scientists have long worked to create useful classificatory systems which can provide relevant information in an accessible manner. Following Darwin, biological systematics attempted to revise the basic classificatory system in a way which was compatible with evolutionary change. The result were numerous classificatory models which represented the relationships among organisms through time.[3] During the twentieth century, the major representational approaches included evolutionary taxonomy, pheneticism, process cladism, and pattern cladism (Ereshefsky 2001, p.51). Similarly, during the same period the category of "species" has been conceptualized in numerous ways including the biological species concept, the recognition species concept, the phenetic species concept, the ecological species concept, the evolutionary species concept, and the phylogenetic species concept (Ereshefsky 2001, p.80-93).

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[3] In the idealization literature, there has been some attention paid to the use of multiple models to represent a single phenomenon. Notably, Weisberg has examined the use of multiple models and Potochnik has gone even further and argued that almost all examples of idealization involve multiple models (Weisberg 2013, p.103-105; Potochnik 2017, p.45)

The representational approach has largely determined which species concept was adopted. For example, on the evolutionary taxonomy representation, biological systematics is predicated on an understanding of evolution in which new taxa arise through either cladogenesis (branching) or anagenesis (divergence) (Ereshefsky 2001, p.52). Here a biological principle is at work to define and situate these two biological processes within evolutionary processes (Section 1.1.2). But the principle is then mediated through a representational model in which the process of speciation is represented spatially through tree-diagrams. By adopting this representational model, biologists were also directed towards the biological species concept in which a species is conceptualized in terms of interbreeding (Ereshefsky 2001, p.81). Additionally, given that the evolutionary taxonomy model is predicated on branching and divergences, the speciation process is often explained by geographic isolation.

Importantly, the evolutionary taxonomy model is idealized. It represents biological change with *perfect branching*. However, under many circumstances, biological divergences occur in a piecemeal manner by which two groups diverge from one another but maintain some interaction. Along similar lines, the biological species concept is often explained by *geographic isolation*. However, geographic isolation is an idealization. Given sufficient precision, few populations ever exhibit perfect geographic isolation from one another. But the idealization of geographic idealization allows scientists to classify species and then conduct future research using the category.

3.1.5 Measurement Idealization

In order for scientists to measure reality, they must first represent reality. Just as reality lacks classificatory categories, it is also recalcitrant to measurement until mediated through a representational medium. For example, consider determining the length of a standard dinner table. In order to measure the table, the scientist must first represent the table. Given the relative simplicity of dinner tables, compared with subatomic particles or national accounts, it may not appear obvious in which way the table is represented. However, in order to measure the length of the table, it must first be treated as flat. With sufficient precision, no ordinary object can be considered flat, but in order to measure the length of the table, the negligible variations in the surface must be ignored. In this way, the table is not being measured "as itself" but rather it is being measured as our "representation of it". In this case, the representation is exceedingly simple and is more often than not a mental representation. But regardless, the table cannot be measured directly, but only when mediated through some representation which is idealized.

Using these different idealization strategies, scientists are able to use models effectively. However, in order to better understand precisely how these idealization strategies are enacted and used, it is worthwhile to consider a scientific model in considerable depth. Here I will examine the *Solow-Swan model* which was originally developed independently by Nobel laureate Robert Solow and Trevor Swan (Solow

1956; Swan 1956). The foundational model of growth economics, the Solow-Swan model is today still one of the workhorse models of modern macroeconomics. However, despite its economic content, it can be used as representative of scientific modeling more generally.

3.2 Solow Swan Model

The Great Depression was a human tragedy, a political challenge, and a scientific puzzle. It spurred economists to understand depressions and this task quickly became the defining goal of the newly christened macroeconomics of the 1930's. In the process, economists began to regularly speak of the "business cycle", which referred to the short-run fluctuations in economic output. Downturns were associated with depressions and recessions. However, in the wake of the Second World War, macroeconomic attention shifted from the challenges of the Great Depression to the competition of the Cold War. Economist in both East and West were interested in determining which economic system could deliver greater long-run prosperity. The result was a renewed interest in "economic growth". In this respect, macroeconomic modeling preserved its initial practical focus and policy relevance.

However, in order to understand economic reality, economists *decomposed* the target system into short-run fluctuations which they categorized as the "business cycle" and the long-run trend which they categorized as "economic growth". In this way, economists used a classificatory idealization (Section 3.1.4). Similar idealizations have been used in climate science in which scientists have decomposed reality into short-run "weather" and long-run "climate". Here the idealization strategy allowed the scientist to construct and use a classificatory system which was both projectable and amenable for empirical work. In practice, economists were able to decompose economic data through the judicious use of data filters. The result was two data models. One represented a relatively smooth trend and was used as the empirical basis for growth modeling while the other formed the basis for business cycle modeling.

Given the empirical focus and practical policy importance of growth modeling, considerable macroeconomic research has been devoted to data collection and measurement. However, economists have long faced practical and conceptual challenges measuring economic output because of the diversity of goods and services exchanged in a modern economy. For example, both petroleum and piano lessons are exchangeable economic products. And it is difficult to aggregate and measure such radically different products. In order to do so, economists have measured all economic products in terms of money. By so doing, it becomes irrelevant that petroleum and piano lessons are radically dissimilar objects as both can be exchanged for money and can be measured in terms of their nominal exchange value. From this, economists throughout the twentieth century developed the modern system of national accounts which provide useful measures of the economy. The most prominent national account is Gross Domestic Product, or GDP. For most practical purposes, GDP can be identified with economic output and from this it is possible to create an operational definition of economic growth as the change in GDP with respect to time.

The GDP measurement requires representing the economy as a circular flow in which goods and services are produced, exchanged, consumed, invested, and then used again in production. Here, economists use measurement idealizations in order to represent the economy in a way which is amenable to empirical measurements (Section 3.1.5). From this, economists have developed three conceptually distinct but quantitatively identical measures of GDP. First, the production (or value-added) measure estimates the gross output of each industry and then subtracts all intermediate inputs used in the productive process which were purchased from other industries, yielding the value-added associated with each industry. Summing these industry specific measures yields a measure of GDP. Second, the income measure estimates the different amounts of income generated by the different factors of production which include capital and labor. By summing all of these different incomes, it is possible to measure GDP. And third, the final expenditures measure estimates the different amount of income spent on different kinds of activities such as consumption and investment, which when summed yields another measure of GDP (Landefield Seskin and Fraumeni 2008, p.196). All three of the measures are predicated on the same representational circular flow model but employ different measurement idealizations.

Although the three different measures are predicated on the same model and are meant to be quantitatively identical, they yield different measures of GDP. Commonly, the divergences are attributed to measurement errors which have been found to correlate with changes in the business cycle (Landefield Seskin and Fraumeni 2008, p.196). More generally, these divergences can be attributed to the different measurement idealizations used for each measure. However, given the correlation, economists have tended to average the three measures over the long-run in order to minimize short-run divergences. Here economists use the categories of “business cycle” and “economic growth” in order to construct a useful data model which can correct for measurement errors. In this way, economists are actively introducing and using idealizations in order to conduct empirical research.

With the economy represented and measured using the national accounts, economists can use models to explain the observed trend in GDP. Prominently, the Solow-Swan model does so in terms of the *neoclassical production function*. For a more technical presentation of the Solow-Swan model, see the appendix. The neoclassical production function represents economic output:[4]

$$Y = AK^{\alpha}L^{\beta}. \tag{3.1}$$

[4] The production function represented in equation (1) is in fact a Cobb-Douglas production function which is a particular specification of the more general neoclassical production function. In it, it is assumed that $(\alpha + \beta) = 1$. The Cobb-Douglas production function has been included for ease of exposition.

Here, Y denotes economic output which can be identified with GDP. K denotes capital which represents durable inputs involved in the productive process such as factories, machines, computers, and even software. L denotes labor and represents all productive inputs directly associated with paid employment.[5] A denotes technology which represents the know-how necessary for production. And finally, α denotes the elasticity of capital and β denotes the elasticity of labor. Both represent the relative efficacy of capital and labor.[6] In so far as the neoclassical production function is a representative model of the productive process, it is an idealized one. The model encodes a classificatory system which forms the foundation of the Solow-Swan model. Here, the classificatory idealization proves to be highly useful for economists as they engage in growth modeling (Section 3.1.4).

From the neoclassical production function comes additional idealizations. First, the representation is idealized so that it exhibits positive and diminishing returns to capital and labor. By this I mean, for each additional unit of capital and labor, GDP will increase but will be marginally less productive than the immediately prior unit. Economists often attribute this idealization to a dispersal effect. Consider a circumstance in which the number of workers increases but the amount of capital remains constant.

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[5] In economics, labor can be measured in terms of either the number of workers or the number of hours worked. It is also possible to adjust a labor metric to account for differences in quality.

[6] In the neoclassical production function, both the elasticity of capital (α) and elasticity of labor (β) are assumed to be constant. Moreover, the elasticity of capital is defined as the percentage change in GDP divided by the percentage change in capital and the elasticity of labor is defined as the percentage change in GDP divided by the percentage change in labor.

Given this, the amount of capital *per worker* would decrease as the constant stock of capital was dispersed over an ever-increasing number of workers. The result would be that each individual worker would become less productive. The same holds true in reverse. Another idealization holds that the representation exhibits constant returns to scale for capital and labor. Following from a proportionality principle, this idealization holds that a proportional increase in capital and labor results in a proportional increase in GDP given that there is no dispersal effect as capital and labor increase in concert with one another. With these idealizations, economists are able to *scale* the neoclassical production function, creating any number of scale models. A particularly useful scale model is the per worker neoclassical production function:

$$y = AK^{\alpha} \tag{3.1'}$$

Here y denotes per worker GDP which is equal to $(\frac{Y}{L})$ and k denotes per worker capital which is equal to $(\frac{K}{L})$. And yet another idealization holds that both labor and capital are individually essential so that, without either capital or labor there would be no production. With this idealization, economists are able to capture the idea that a worker without a tool is as useless as a tool without a worker. Both are essential to the productive process.

Given the neoclassical production function, the Solow-Swan model explains the dynamics of economic growth by representing the dynamics of the three constituent categories exemplified in the production function: *capital*, *labor*, and *technology*. First, consider technology. In the simple Solow-Swan model, technology is represented as constant, thereby precluding the possibility for innovations in production. Here, the model does seem to diverge from reality as economic history and everyday life is replete with examples of technological innovations. Similarly, labor growth is represented as constant despite the well documented connection between economic prosperity and labor growth. Greater prosperity can sometimes spur population booms or lead to reduction in birth rates. In fact, contrary to the Malthusian mechanism by which prosperity promotes labor growth, modern history indicates that economic growth may be the most effective form of birth control in human history. However, in the simple Solow-Swan model, the percentage of labor growth, denoted by n, is represented as constant. The result is that economic growth does not affect the labor growth rate.[7]

Note that the Solow-Swan model represents both technological dynamics and labor dynamics as *exogenous*. From this follows a causal asymmetry in which changes in technology or labor can cause changes in other factors but other factors cannot cause changes in technology or labor. In this way, it is possible to understand these exogeneity idealizations as explanatory idealizations (Section 3.1.1). By introducing the idealizations and creating the causal asymmetries, economists then became able to use the model in

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[7] In general, the rate of change per unit of time (t), for any variable X is notated $\dot{X}$ ($\equiv \frac{\partial X}{\partial t}$), so that the percentage growth rate is $\dot{X}/X$. Thus $n = \dot{L}/L$.

order to explain economic growth by analyzing the changes in technology and labor. However, it is equally possible to understand these idealizations as exploratory (Section 3.1.3). For example, when economists introduced these idealization, they were well aware that labor growth was important to economic growth. Economists have also been equally aware since Malthus that economic growth is important to labor growth. However, for economists developing the early growth models, the exogeneity idealization was a *useful starting point* for future modeling. It was an embarkation (Section 3.1.3). And since the development of the Solow-Swan model, economists have developed a family of "Malthusian" growth models which explicitly represent the effect of economic growth on labor growth. And importantly, these Malthusian models have been developed in the wake of the Solow-Swan model and share many structural properties in common with it.

Returning to the Solow-Swan model, capital dynamics are governed by a simple process. Capital is accumulated through investment and lost through the ordinary wear and tear accumulated from regular use, referred to as depreciation. However, as GDP is an aggregate of total economic output, investment must be understood as the proportion of GDP which is not consumed but is saved. From this, it is possible to represent capital dynamics:

$$\dot{k} = \iota y - (n + \delta)k. \tag{3.2}$$

Here $\dot{k}$ denotes the rate of change in capital with respect to time. The proportion of GDP saved is referred to as the investment rate and is denoted by ι and the depreciation rate is denoted by δ. From this representation, it follows that capital accumulates faster when the investment rate increased or when GDP increases. Likewise, capital accumulates slower as the depreciation rate increases or the labor growth rate increases. However, in the simple Solow-Swan model, both the investment rate and depreciation rate are represented as constant. Here again, exogeneity idealizations have been used for both explanatory and exploratory purposes.

From the neoclassical production function and the various dynamical equations, economists have constructed a single dynamic structural equation which represents the overall dynamics of the Solow-Swan model:

$$\dot{k} = \iota A k^{\alpha} - (n + \delta)k. \tag{3.3}$$

Known as the "fundamental equation" of the Solow-Swan model, using it economists can determine a number of results and analyze various policy interventions. However, for expository purposes, it is useful to focus on those results which involve the "steady state" of the model. Defined as the condition in which per worker capital is constant with respect to time, the steady state can be represented by setting $\dot{k}$ equal to zero. Given that per worker capital is equal to gross capital divided by gross labor, the steady state occurs when the percentage change in capital is equal to the percentage change in labor. But given that the percentage change in labor is represented by the constant n, in the steady state the percentage change in capital must also equal n. From this it follows that in the steady state capital, labor, and GDP increase at the same rate n but that per worker GDP remains constant. In other words, in the steady state, there is economic growth proportional to labor growth but individual workers do not become wealthier.

However, there is extensive empirical evidence indicating that economic growth in the developed world does involve individual workers becoming wealthier. From this, economists have concluded that there is an important empirical deficiency with the *simple* Solow-Swan model. However, it is perhaps more correct to understand this divergence between the simple model and empirical evidence in terms of exploration (Section 3.1.3). In the simple model, the rate of technological change, denoted by $\dot{A}$, is assumed exogenous and constant over time:

$$\dot{A} = 0. \tag{3.4}$$

But given the empirical deficiency associated with the simple model, Solow refined the model and explored the possibility of representing technological change as an exogenous but increasing process:

$$\dot{A} > 0. \tag{3.5}$$

By changing how technological change was represented, Solow constructed a model which was both relevantly similar to many growing economies and which allowed for per worker economic growth in the steady state. Perhaps just as importantly, the refined Solow-Swan model paved the way for new growth models in which economists began to explore additional possibilities. However, the initial burst of research in the immediate aftermath of the Solow-Swan model petered out as economists faced intractable technical difficulties (Boianovsky and Hoover 2014, p.214-218). And yet, in the decades following these initial setbacks, Nobel laureates Robert Lucas and Paul Romer separately constructed tractable growth models with endogenous technological change (Lucas 1988; Romer 1986). In many ways, these innovative "endogenous" growth models can be understood as acts of discovery emanating from the embarkment of the Solow-Swan model.

Additionally, the Solow-Swan model has been used for empirical measurement. Before the development of the model, economists had developed measures of GDP, capital, and labor. However, economists had not yet been able to develop a measure of technology. Solow used the model to measure technology using time series data sets (1957). The basic procedure involved recognizing that the percentage change in per worker GDP could be accounted for by the percentage changes in per worker labor and technology:

$$\%\Delta y = \alpha\%\Delta k + \%\Delta A \qquad (3.6)$$

By subtraction, the percentage change attributable to technology could then be isolated as an estimable residual:

$$\%\Delta A = \%\Delta y - \alpha\%\Delta k \qquad (3.7)$$

Solow and others then used the residual as a measure of technology.[8] It has since become known as the "Solow residual" and follows directly from the idealizations of the Solow-Swan model. Additionally, the model provided the interpretative framework needed to understand the residual (Crafts 2009, p.201). In other words, the model allowed economists to identify the residual with technological change. Here, the idealizations allowed for both the construction of the measure as well as its interpretation (Section 3.1.5). In this way, the idealizations of the Solow-Swan model are what make it useful.

3.3 Completeness

Idealizations are useful for scientific modeling, but they are more than that. In order to work, models *must* be idealized. Behind this is the interdependency between representation, classification, and measurement. In order to represent reality, reality must be categorized and measured and both of these tasks are impossible without introducing idealizations into the representation. This interdependence is but a reiteration of the old thesis that observation is necessarily theory-laden. Except, in this circumstance, it is more appropriate to say that observation is necessarily model-laden.

[8] The measurement strategy deployed by Solow has analogs across the sciences. For example, astronomers have used astrological measurements and models of the rate of expansion of the universe to show that observable matter is insufficient to explain observed cosmological dynamics. Subtracting the effects of the observed matter from the estimate of what would be necessary to explain the cosmological dynamics yields a residual. This residual has been dubbed dark matter. By analogy, the Solow residual could be called dark technology.

However, this does not imply the impossibility of observation, but rather that idealized models are ideal.

Commonly however, idealizations are not considered ideal but rather as purposeful distortions which contaminate scientific models. But the identification of idealizations with distortion implies that there is some alternative which is not distorted. But there only appears to be two plausible candidates for such an alternative. First is reality (Section 1.1.2). By definition, reality is not a distortion of itself. However, as reality is independent of all representational constructs, it is without a classificatory system and is recalcitrant to measurement. All this makes reality not directly epistemically accessible. There is no unmediated way to know what reality is "really" like. Given this, it is impossible to determine whether or not an idealization is a distortion from reality without implementing the exact kinds of systematic classifications and measurements which require idealizations. In other words, in order to know about reality, the scientist must first represent it. The second candidate is a *complete model* which is, by definition, in no way distorted (Weisberg 2013, p.98; Wimsatt 2007, p.101-102). Here completeness serves as a modeling ideal by which idealizations can be understood as distortions away from the ideal. But completeness is a false ideal. It is not only unachievable; it is not a worthy aspirational goal as it renders models useless. The ideal model must be useful and the complete model could only ever be useless.

3.3.1 False Ideal

Completeness is a false ideal. But in order to demonstrate its shortcomings, it must be fully articulated. However, doing so is not straightforward as it is an ideal which operates in the *background* (Weisberg 2013, p.105). The result is that there are few detailed accounts of completeness. One notable exception comes from Paul Teller:

> The photograph provides a good icon. The ambition has been to produce a perfect likeness of nature, a [complete] model. The natural law ideal can be seen as the theoretical side of this more general enterprise, complemented by efforts to get untainted observations [Teller 2001, p.393].

However, while the photography metaphor is evocative and Teller is correct in drawing connections between completeness and the myth of theory-free observation, this articulation of completeness is still somewhat threadbare.

Weisberg presents a far more comprehensive account of completeness.[9] In it he distinguishes between *inclusion requirements* and *fidelity requirements*. Inclusion requirements detail which objects and properties must be included in the complete model and fidelity requirements detail how those included objects must be represented. The inclusion requirement is straightforward, the complete model must include *every* property of the target system, regardless of how irrelevant it may be (Weisberg 2013, p.105-106). The fidelity requirement then holds that the model must represent all properties of the target system with *maximum precision and accuracy* (Weisberg 2013,

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[9] It is worth clarifying that Weisberg does not claim that completeness is the only modeling ideal. Far from it. But it is the first modeling ideal he details and the one "against which every kind of idealization can be discussed" (Weisberg 2013, p.105).

p.106). Together, these requirements yield a highly demanding ideal. In fact, Weisberg and Teller are clear that not only is the complete model unlikely to be constructed or used, it is in almost all circumstances downright impossible (Weisberg 2013, p.106; Teller 2001, p.410). They are, in this respect, completely correct.

3.3.2 Achievable Completeness

Completeness is unachievable in not only most circumstances; its conceptual incoherence renders it unachievable in all circumstances. It is incoherent because completeness simultaneously precludes idealization and yet idealizations are necessary for representation. The result is that complete representations are contradictory. To be complete is to be reality. All representations necessarily differ from reality, meaning that they are incomplete. In more detail, the inclusion requirement holds that a complete model must include *all* properties of the target system. But in order for a model to include something, there must be some kind of classificatory system. And yet, classificatory systems require an idealization strategy (Section 3.1.3).

For example, the Solow-Swan model possesses a classificatory system which allows the economist to categorize all objects as one of *GDP*, *capital*, *labor*, or *technology*. However, this classificatory system comes from the idealized neoclassical production function. Within this broader representational framework, this classificatory system is both necessary and exhaustive (Section 1.1). Given this, there is no procedure for incorporating an object into the Solow-Swan model without first classifying it as belonging to one of these categories. It is possible to alter the model so as to allow for additional categories, such as *land*, and yet doing so would still require idealization. Effectively, all classificatory systems require idealization regardless of how many categories are within the system. The result is that completeness is a contradictory ideal. Consider a complete model. By definition it will include everything in reality. But inclusion requires the simultaneous classification of everything in reality. However, as idealization is required for classification, the complete model will need to be idealized in order to include the objects in reality. Here completeness simultaneously requires idealization and definitionally precludes it. The result is a straightforward contradiction.

To salvage completeness, classification and inclusion would need to be disconnected. It would need to be possible to include an object in a model without simultaneously classifying it. However, not only is this implausible, it would create a different contradiction. Representation requires classification but to separate inclusion from classification would then make a representation that includes unclassified objects. However, this is impossible. Consider again the complete model which includes everything in reality. But in this case, imagine that inclusion does not require simultaneous classification. The resultant unclassified mess of objects would not be a model at all. It would just be unclassified and unmeasurable reality. And reality is no model. By divorcing inclusion from classification, the complete model would simultaneously be and not be a model. Here again is another straightforward contradiction.

Additionally, the fidelity requirement is contradictory. It holds that the complete model must represent objects with *maximal precision*. However, there are circumstances in which maximal precision is not only unachievable, it is inconceivable. Specifically, there are measurement exercises in which measurements diverge as the degree of precision changes, rendering the idea that maximal precision is a definite target unfounded. For example, scientists have long attempted to measure the boundary between land and sea, resulting in *two* measures: *coastline* and *shoreline* (Oertel 2019). Coastline is a relatively imprecise measure of the boundary between land and major bodies of water, while shoreline measures the precise boundary between land and water. The difference between the two measures is primarily a matter of precision (Section 1.1.3). And yet, scientists employ both measures for different purposes, specifically because they diverge from one another. The United States coastline is 12,383 miles while the shoreline is 88,663 miles (Hoover 2012, p.235). All this undermines the unsubstantiated faith that there is a conceptually achievable maximally precise representation of any target system, making the fidelity requirement for completeness highly suspect and undermining the conceptual coherence of completeness.

3.3.3 Aspirational Completeness

Charitably, completeness is not meant to be achievable. No one endorses it as a practical goal for scientists. Rather, it is held up as an aspiration. Given this, perhaps one could argue that it does not matter that completeness is incoherent. I would not. But granting that completeness is merely an aspirational ideal is also untenable.

Weisberg details how completeness is an aspirational ideal for scientific modelers by distinguishing between the *evaluative* use and the *regulative* use of completeness (2013, p. 106). To use the ideal evaluatively, scientists can construct an ordinal ranking of models. Each model can be ranked in order of completeness, with the "more complete" the model the better. Supposing that this kind of evaluation was useful, it is worth noting that in order to construct the ordinal ranking, the scientist would need to prepossess knowledge of the complete model for the specific target. However, given that the complete model is optimal and already known, it undermines the value of analyzing any suboptimal model. Additionally, it is implausible to suppose that such an ordinal ranking is straightforwardly achievable. Different models possess different classificatory systems, meaning that in order to construct a *single* rank ordering of models the scientist must be capable of translating between these different classifications. Although this may be possible, it is certainly not straightforward in the manner described by Weisberg (2013, p.106). And in many circumstances, it is almost certainly impossible.

However, as an aspiration, the operational hurdles associated with constructing a rank ordering of scientific models is perhaps unfair. Charitably, completeness is meant only to guide scientific modeling rather than to be used in practice. Weisberg indicates as much through his discussion of the regulative use of completeness (2013, p.106). He contends that completeness guides inquiry by directing scientists to always "add more detail, more complexity, and more precision" to models (2013, p.106). Broadly, this regulative use is coherent. Scientific research is often dedicated to incorporating more details, complexity, and precision to representations. However, there are three flaws with this regulative use which renders completeness a false ideal.

First, it is not always useful for scientists to add more detail, complexity, or precision. Scientific modeling always proceeds in a piecemeal manner and often this process involves simplification and abstraction. Doing so allows scientists to represent reality in terms of identifiable properties (Section 2.2.2). For example, recall that that economists decompose economic reality into the short-run fluctuations of the "business cycle" and the long-run trend of "economic growth". However, this decomposition is a representational artifact, not necessarily reflected in the constitution of reality. It results from a classificatory idealization strategy. However, its artificiality is no evidence that it is only conventional. Rather, the decomposition resulted from a number of methodological innovations, including data filters which could be used to separate fluctuations from trend.

Similar decompositions are used in climate science. Regularly, scientists distinguish between the short-run fluctuations of “weather” from the long-run trend of “climate”. Additionally, in the analysis of fluid flows, the complexity of fluids has prompted the creation of fluid dynamic models with an artificial “boundary layer” where the viscosity of the fluid can be neglected (Morrison 2011, p.344). The boundary later results from a classificatory idealization which decomposes the target system into multiple sub-systems with distinguishable properties which can be studied in a piecemeal manner:

> [W]e cannot have a single model of the fluid that represents the way it flows around a boundary. Despite the two different models they needn’t be interpreted as inconsistent since they apply to different types of behavior in different parts of the fluid [Morrison 2011, 345].

The success of fluid dynamic models, climate models, and growth models are all predicated on their *in*completeness. Generalizing, there are many modeling exercises in which it is not useful to add more detail, complexity, or precision to a model. Here completeness does not regulate scientific modeling.

Second, even when scientists do add more detail, complexity, and precision, it is not attributable to the regulative use of completeness. Quite the opposite. For example, in the simple Solow-Swan model, technology was represented as constant and exogenous. However, through the exploratory processes of modeling, economist altered the model in order to represent it as technology as increasing and endogenous. Plausibly, these innovations in growth modeling could be interpreted as instances in which scientists were regulated by completeness. However, this interpretation is mistaken as it misses a critical element of these alterations and refinements. The changes were neither motivated nor guided by completeness but rather by the specific contours of the initial Solow-Swan model. Exploration is always path dependent and historically guided. It is in this way that completeness most obscures the creative process of modeling and scientific change. Scientists do sometimes add more detail, complexity, and precision, but not because of completeness. They do so *in response to specific needs* and to further *specific purposes*, often empirical ones. Completeness obscures precisely because it posits an ahistorical standard. But when scientist to alter their models, it is guided and regulated by specific exploratory idealizations, not completeness at all.

Third, completeness can often make models less useful. By adding in more detail, complexity, and precision, scientists can sometimes render a useful model effectively useless. And this defeats the purpose of modeling. Models are not constructed to merely satiate the curiosity of the scientist or the philosopher. Models are meant to work, to be used. Scientific representation is a fundamentally practical endeavor. Economic growth models are constructed to explain, predict, and measure economic growth so that economists can design and implement effective policy interventions. The same holds true for climate modeling, and so on. Reality, in all its unclassified and unmeasurable complexity is unmanageable. It is not directly observable or completely controllable (Section 1.1.2). Models are observable and controllable. Models are useful. And in so far as completeness directs scientists to mimic reality's complexity, it directs scientists to create useless models. And useless models are certainly not ideal.

3.4 Conclusion

In order to articulate and justify the logical structure of scientific models, it is useful to examine the *ideal model*. Throughout this chapter, I have defended the claim that the ideal scientific model is useful. More specifically, ideal models are idealized. In order to demonstrate the ways in which idealized models are useful, I examined five different *idealization strategies* which correspond to the primary kinds of inferential purposes which guide scientific modeling (Section 3.1). Explanatory idealizations allow the scientist to identify causally relevant properties and universally shared properties. Predictive idealizations increase the tractability of models and thereby allow scientists to conduct forecasts. Exploratory idealizations allow scientists to develop different models from a common origin point. Classificatory idealizations create classificatory systems which can be used to observe reality in a systematic manner. And measurement idealizations allow scientists to measure reality and construct data models. Together, these idealization strategies make models useful and make models ideal.

In order to defend my position, I examined the possible modeling ideal of completeness (Section 3.3). It holds that the ideal model includes everything in reality and represents with maximum precision and accuracy. But I demonstrated that completeness is a false ideal. It is unachievable in not only most circumstances; its conceptual incoherence makes it unachievable in all circumstances. Even more, it is not a viable aspiration for scientific modelers. By adding more detail, complexity, and precision to models, scientists can often turn useful models into useless models. Just as the complete map just is the world, the complete model just is reality. And reality is neither directly observable nor controllable. Models are observable and controllable precisely because they are idealized. And it is these differences which make scientific models useful, which make them ideal.

In order to understand the logical structure of scientific modeling, it will be useful to examine how scientific models are altered. Throughout the discussion of *idealization strategies*, it became evident that scientific models undergo changes and can be tailored for specific purposes. In the next chapter, I detail the alteration process by which models are made to work. Alteration is situated within the exploratory process by which scientists use models to explore conceptual space and determine the relative likelihoods of different outcomes (Section 4.1). And finally, by detailing how models can be altered, I demonstrate how models become ever more specialized and useful (Section 4.3). In this way, scientific modeling is a genuinely *progressive* endeavor.

4. Making Models Work

Models do work, but they do not just work. By this I mean, that modeling is not some passive procedure, but is rather an active and creative intellectual enterprise. Scientists use models to explore reality, but in order to do so, models must represent it. However, scientists rarely if ever have a fully articulated research agenda at the outset of a research program. It is through modeling that scientists uncover possibilities and test likelihoods. In this way, modelers are explorers. Discovering new regions, opening up routes for development, and creating maps for posterity. But as with all exploration, scientific modeling is constrained. It is constrained by the constitution of reality as well as by those principles which guide the explorer. And yet, it is these constraints which make scientific modeling possible and useful.

Underlying scientific modeling is an *exploratory logical structure*. Exploration itself can be decomposed into embarkation, discovery, and mapping (Section 3.1.3). Embarkation is the required first step for any exploration. For modeling, embarkation involves the construction and use of some initial representation. Without it, the scientist would be unable to observe reality and begin the classificatory process. Through embarkation, they are then able to begin determining which properties are relevant given their purposes and to determine initial estimates of relative likelihoods. From there, discovery involves venturing out into the unknown. Making piecemeal alterations. One by one. Until eventually the embarkment model is something new. It is tailored for a highly specific purpose, constrained by reality and by principles. And then finally, mapping involves representing, presenting, and disseminating the altered model.

Against this exploratory logical structure, scientific modeling is commonly conceived of as a reversible idealization process and deidealization process by which models are made either more or less similar to reality. However, this conceptualization is reliant on the false ideal of completeness and should be rejected in favor of an exploratory understanding of scientific modeling.

Importantly, exploration is *progressive*. Through implementing alterations, the scientist constructs ever more specialized scientific models. With specialization comes inferential gains. The scientist is then able to use the altered model more effectively for specific targets, making the model more useful. Gains in usefulness are constitutive of scientific progress. For this reason, progress should not be identified with verisimilitude because in order to determine verisimilitude the scientist would need to know the constitution of reality independently of the model, which is impossible. But by understanding progress in terms of usefulness, it is possible to avoid this epistemic barrier. And finally, scientific progress is explicable because through specialization, models become more useful due to an "inferential division of labor". However, in order to understand the source of the inferential division of labor, consider how models are used to explore and how scientists implement alterations.

4.1 Exploration and Deidealization

Modeling is exploratory. It is uncertain with no guarantee of success. And yet, it is through exploration that reality is understood and represented. The underlying exploratory logical structure of scientific modeling can be decomposed into embarkation, discovery, and mapping. However, in order for scientists to engage in discovery, they must *alter* existing models. Beginning with some embarkation model, scientists regularly implement highly specific alterations in order to accomplish a particular specifiable purpose. In this way, the scientist is able to represent additional possibilities, creating opportunities for yet more modeling. Through alteration, scientific modeling becomes an *iterative process* by which highly specialized models can be constructed and used. However, commonly this alteration process is referred to as "deidealization" because it is considered the reverse of the idealization process.

Idealization and deidealization are usually considered reversible processes by which models are made either more or less similar to the world (Nowak 1982; Knuuttila and Morgan 2019; Peruzzi and Cevolani 2021). The product of this reversible process is usually known as an idealization and understood as a purposeful distortion which can contaminate a representation (Section 3.1). The product of the deidealization process is understood as the elimination of a purposeful distortion. However, this reversible conceptualization of scientific modeling is flawed because in order for a model to be idealized or deidealized, there must be a possible unaltered model which is in no way idealized, which is complete. But completeness is a false ideal (Section 3.3).

Specifically, deidealization is commonly understood as the elimination of purposefully false distortions. In the limit, deidealization ought to yield the complete model. For example, Peruzzi and Cevolani contend that,

> [r]oughly, de-idealizing a theory or model means removing one of its idealized assumptions and replacing it with a new one that is less idealized, that is more realistic in being closer to the actual phenomena [Peruzzi and Cevolani 2021, p.4].

Given the reliance of the deidealization conception on the complete model, it is preferable to understand modeling in terms of exploration. However, in order to retain some of the rationale behind the deidealization conception that models can be made more or less similar to target systems, it is useful to incorporate the conception of *relevant similarity* into the exploratory logical structure. By doing so, it becomes possible to preserves the sense in which certain models more resemble reality than others.

However, few if any models are perfectly relevantly similar to a target system. Even once the scope of properties is restricted by the purposes of the scientist to those properties which are relevant, there will almost always be some relevant dissimilarities between model and target system (Section 2.1.2 and Section 3.3). But it is still true that some models will be more relevantly similar than others. Consequently, by altering a model, it is possible to make a model more relevantly similar and, in this way, better represent the target system and become more useful. In this way, the alteration process is crucial to understanding how scientific models can be *made to work*. The alteration process is the iterative process by which models are made more relevantly similar to a specific target system. Importantly, by so doing, the alteration process achieves the required relevant similarity between model and target system to make models representative. However, in order to understand *how* the alteration process can make a model more relevantly similar, it is necessary to characterize the process.

In the deidealization literature, this process is commonly characterized as an almost algorithmic reversal by which idealizations are easily identified and then eliminated. However, as indicated above, this conceptualization is fatally reliant on the false ideal of completeness. In order to better understand the alteration process, conceive of it in terms of exploration. But exploration is a broad category which encompasses many distinct kinds of modeling practices. Following Knuuttila and Morgan, it is possible to identify the particular kind of exploration distinctive to the alteration process with *situating* (2019, p.652).[10] Specifically, situating can be understood as the means by which a *theoretical model* is transformed into an *empirical model*. By doing so, the scientist is able to alter and tailor the empirical model in such a way as to ensure that it is

[10] Knuuttila and Morgan identify *four* distinct kinds of processes which they categorize as deidealization. These include: (i) recomposing, (ii) reformulating, (iii) concretizing, as well as (iv) situating (2019). It is useful to summarize the other three kinds of process. Recomposing can be understood as the process by which a model is reconstructed after alterations are made. Reformulation involves transforming an artifact into a representational object. And finally, concretizing occurs when the classificatory framework is altered (Section 3.1.4). All three of these processes are important to scientific modeling; however, it is unclear to what extent they are distinctively associable with deidealization as commonly understood. Rather, all three of these processes seem to be commonly employed throughout modeling.

sufficiently similar to the target system so as to make the empirical model useful. The alteration process can be understood as one of the primary ways in which scientists actively craft models capable of doing work.

Here is it is necessary to distinguish between theoretical models and empirical models. In order to do so, recall the *representational continuum* which captures the various kinds of abstract representational objects employed across the sciences (Section 1.1.2). The most concrete representations are data models. And although data models are relatively concrete, they remain to some degree abstract. Data, no matter the circumstances, are always constructed representations of reality, not reality itself. In this way, data models act as the first mediator between the world and the scientist and consequently are to some degree abstract. However, such models are still relatively concrete as data are closely tied to reality. On the other end of the continuum are the general principles which organize scientific research (Section 1.1.2). For example, in classical mechanics, the Newtonian principles determine the basic ontology of force and mass and encode fundamental relationships between those categories and position, velocity, and acceleration. Similarly, in economics, the rationality principle holds that economic agents generally do not make systematic mistakes in achieving their ends given their constraints.

Between data models and principles are those representational objects most commonly thought of as scientific models. The previously detailed Cournot model and the Solow-Swan model both fall into this broad category (Section 2.1.3 and Section 3.2). And importantly, scientific models are constrained by principles and data models. However, within this category, it is possible to differentiate theoretical models from empirical models. Theoretical models are closer to principles than to data models. By this I mean, theoretical models are distinguishable by the fact that although certain parameters are specified, much is left open. In practice, theoretical models represent *generalizable patterns* which emerge across numerous data models rather than any specific data model. The result is that theoretical models are generally used for explanation, exploration, and classification. For example, the Cournot model is a theoretical model. It provides the classificatory system needed to differentiate monopolistic, oligopolistic, perfectly competitive arrangements. In this way, the theoretical model imposes an ontology and provides the scientist with a classificatory framework (Section 3.1). Similarly, the Cournot model allows one to explore the model space by altering certain theoretical constructs. For example, by relaxing the homogenous good requirement in the original Cournot model, it is possible to create the *D-Bertrand model* (Peruzzi and Cevolani 2021, p.11). And finally, it is possible to use the Cournot model to explain competitive firm behavior.

Commonly, philosophical analysis of scientific models is limited to theoretical models. In fact, throughout this dissertation, the focus has been primarily on theoretical models. However, an important component of scientific research which has been undertheorized by philosophers revolves around empirical models. Unlike theoretical models, empirical models are distinguished by their greater concretization in that they are less abstract and more closely related to data models. Empirical models are more often used for prediction and measurement, although it is possible for them to classify, explain, and explore. Importantly, although there is a substantive distinction between theoretical, empirical, and data models, there will be certain vague boundaries existing between these categories and trouble cases which cannot be easily classified. And yet, vagueness at the borders does not undermine the value of distinguishing between theoretical, empirical, and data models. Oftentimes, in order for a theoretical model to be used in particular ways, it must become an empirical model. It must be altered in particular ways so as to better reflect the world by being more relevantly similar to the target. Commonly one begins with a theoretical model and transforms it into an empirical model, although this is not necessarily the case. Knuuttila and Morgan refer to this as a "positive fitting" (2019, p.653). Such transformations *situate* the theoretical model within the target system, resulting in greater relevant similarity.

Both the alteration process and the difference between theoretical and empirical models are most easily understood by considering examples. To that end, I will present and analyze a pair of models from industrial organization. First, the theoretical Green and Porter model provides an explanatory framework for understanding collusive cartel organizations in terms of optimizing behavior and unexpected demand shocks. Second, the empirical Porter model alters the more abstract and general theoretical model and applies it to the freight railroad industry in the United States during the Gilded Age. Together, these models exemplify not only how models work, but also the alteration process by which they are made to work.

4.2 Cartel Models

Cartel organizations have historically provided the institutional framework in which rival firms can organize and collaborate in order to generate higher profits than would be possible under competitive arrangements. Historically though, cartels have not only formed, they have also collapsed as individual firms can often generate even higher profits by undercutting their rivals and capturing a disproportionately large market share. However, once one firm defects from the cartel organization, all other firms will follow suit resulting in a suboptimal competitive market arrangement. And yet, despite these collapses, cartels can often reform. There appears to be a dynamic cartel process in which firms regularly collude, defect, and then re-collude.

Economists have studied this process, resulting in a number of theoretical models. Early prominent examples attempted to explain cartel organization in terms of coercion. The organization was understood as a "policeman" who could enforce certain policies so that firms could maximize their profits (Stigler 1964). However, these early models were deemed problematic as they contravened one of the general principles of economic analysis which holds that agents ought to be represented as optimizing agents.

This rationality principle does not preclude isolated instances of suboptimal behavior under particular conditions, but structures the explanatory framework employed throughout economics (Section 1.1.2) Specifically, economists are trained to conceive of behavioral patterns in terms of optimality conditions and the result of mutually beneficial incentive structures. Consequently, the conceptualization of cartels as essentially coercive undermined the possibility for cartels to be genuinely beneficial for the participating firms. Here the principle organizes the less abstract representations by imposing constraints. In order for a model to provide a genuinely *economic* explanation of cartel dynamics, it must represent participating firms not as coerced victims but as rational optimizing agents. In this way, the principle acts as a constraint as well as a classificatory tool. It simultaneously excludes certain theoretical models and helps the scientist determine the limits of their disciplinary program.

The rationality principle constrained Green and Porter. For this reason, they reconceived cartels as collaborative and voluntary organizations which were upheld by the optimizing expectations of participating firms (1984, p.88). It is worth noting that before Green and Porter, there were economic models which approached the cartel problem in precisely this way (Osborne 1976; Friedman 1971). However, these earlier theoretical models failed to represent the dynamics of cartel formation, collapse, and reformation. Specifically, by conceiving of firms as optimizing agent and cartels as optimal arrangements, there were no circumstances in which a cartel would collapse once formed. In order to represent the *dynamism* of cartels, Green and Porter introduced sources of uncertainty which could disrupt the optimality of cartel organizations. In this way, Green and Porter simultaneously cohered with the rationality principle and represented the dynamic process of interest. Here, the theoretical model can be understood as resulting from constraints coming from both principles and data models. Effectively, both extrema of the representational continuum acted as constraints which determined the contours of the theoretical model.

Given the complexity of the Green and Porter model, it is often useful to decompose the model into three distinguishable parts. It is worth noting that although the parts are identifiable, it is not always possible to isolate them (Section 2.2.2). However, it is still possible to analyze each of the three part in turn and then to consider how they interact with each other. First, the *competitive sub-model* represents the industry in the absence of a cartel organization. Following Green and Porter, the competitive sub-model is designated as a Cournot model (Section 2.1.3). In the Cournot model, firms produce homogenous goods and compete on production output. Moreover, by incorporating the Cournot model, Green and Porter take on many of the idealizations which characterize the sub-model (Section 2.1.3). Specifically, the Green and Porter model holds that the goods produced within the industry are homogenous and that firms make simultaneous production decisions. The result is that the industry possesses a uniform market price which is known to all consumers and producers.

Moving to the second part of the Green and Porter model, the *collusive sub-model* represents the industry given the existence and maintenance of the cartel organization. The result is that firms that participate in the cartel are able to generate higher profits than would be possible under the competitive Cournot arrangement. Specifically, colluding firms are able to create a facsimile of a monopoly in which the total industry output is equal to that produced by an optimizing monopoly firm. However, in order to achieve this, each firm is allocated a particular market share and produces in accordance with it in order to maximize collective profits. Each firm maintains control over its own production and can increase or decrease production accordingly. By decreasing production, the market price will rise and by increasing production, the market price will fall. Each firm maintains partial control over the market price and possesses the capacity to *monitor* the market price as it changes in accordance with variations in supply and demand. In other words, each individual firm possesses limited market power.

With market power comes a collective responsibility among the colluding firms to limit production so as to maintain the collusive arrangement and associated monopoly price all while adjusting to changes in demand. But for each individual firm there is an incentive to increase production beyond the amount allocated by the cartel organization. By doing so, the firm can capture an increased market share and bolster their profits. However, if multiple firms were to simultaneously increase production, the market price would eventually fall significantly below the monopoly price and eventually reach the price exhibited in the Cournot model. In this way, the cartel becomes effectively irrelevant. In order to prevent this socially suboptimal arrangement, the cartel institutes an enforcement mechanism to disincentivize production increases. Although there are multiple different specific mechanisms which could fulfill this function, Green and Porter posit the existence of a *trigger price agreement* in which all firms consent to increase production to the level exhibited in the Cournot sub-model (1984, p.89). In this way, the trigger price effectively dissolves the cartel and returns the industry to a competitive arrangement. Given that firms can generate higher profits by coordinating rather than competing, activating the trigger price is suboptimal and therefore each and every individual firm has a strong incentive to not increase production beyond the levels specified by the cartel, thereby lowering the market price to the point of reaching the trigger price. By so doing, the trigger price agreement enforces all firms to maintain production levels roughly in accordance with those specified by the cartel.

The trigger price agreement can represent either explicit contract or implicit agreement. On one conception, the trigger price is formally agreed to by all firms participating in the cartel organization. It could involve, in extremis, a written contract signed by the executives of all firms and the central committee of the cartel. Alternatively, it could be a mere handshake agreement in which all the firms come to a common understanding. Even less formally, the agreement could just be a rough and ready agreement among the firms, despite never being explicitly articulated or circulated. All that is required for the *theoretical* Green and Porter model is that trigger price be known to all members and that each and every firm be aware of the importance of the trigger price in preventing mutually disadvantageous price wars. Once one does develops an empirical model, it becomes necessary to specify the particularities of the price trigger agreement in order to be able to measure and study it.

However, under ideal conditions, the trigger price would never be reached and the market would remain in the socially optimal cartel arrangement. But in order to represent the dynamic cartel process, conditions cannot be ideal. By this I mean, it must be possible for the trigger price to be reached and for firms to sometimes engage in price wars in which the market price reverts back to the level exhibited in the competitive sub-model. Additionally, it is necessary to represent the process by which the cartel reforms after the trigger price has been activated. In order to do so, Green and Porter incorporate a *switching model* which allows the overall model to transition between the competitive sub-model and the collusive sub-model, and back again (1984, p.89-90). To accomplish this, the switching model posits that the trigger price agreement specifies that price wars ought only last to for a *limited period of time*. After this prespecified period of time, all firms are then directed to limit production back to the levels represented in the collusive sub-model. The switching model provides the correct incentive structure to disarm the collective action problem which undermines cartel stability and allowing cartels to form, collapse, and then reform.

Throughout, the Green and Porter model coheres with the rationality principle. It is both optimal for firms to engage in price wars when the trigger price has been reached and it is optimal for firms to restrict production back down to those levels exhibited in the collusive sub-model at the end of the prespecified period of time. In this way, the dynamic is understood as the result of rational behavior. However, the switching model is insufficient to recreate the cartel dynamic. Recall that firms possess limited market power and can therefore control, to some extent, the market price. Firms are also incentivized to avoid the trigger price. Given that firms are optimizing agents, it follows that they ought to always restrict production so as to avoid the trigger price and avoid costly price wars. And yet, in order for the model to represent the dynamic of cartel collapse and reformation, the trigger price must occasionally be reached. In order to allow for the possibility of periodic price wars, as exhibited in the data model, Green and Porter introduce a *probabilistic element* into the model (1984, p.91). This probabilistic element comes in the form of a *demand shock* which can either raise or lower the market price. Importantly, from the perspective of the firms, demand shocks are exogenous and unpredictable. In this way, demand shocks limit the market power of all firms to control the market price.

There are two different constraints on market power in the Green and Porter model. Individual firms have limited market power because each firm must compete with other firms. If any individual firm were to increase or decrease production unilaterally, they would be countered by other competing firms who could undersell them. However, these kinds of competitive forces limit the market power only of the individual firm acting unilaterally. But when all the firms cooperate under the auspices of the cartel, these competitive forces are not present and the cartel possesses complete market power in the same manner as a monopoly. And crucially, a monopoly would never lower the price to the point of hitting the trigger price. Consequently, when the cartel exists it would restrict output so as to ensure that a price war never occurred.

However, by introducing probabilistic and exogenous demand shocks into the model, Green and Porter limited the market power of all firms, even when they cooperate through a cartel organization. Even a monopoly could not completely control the market price as it is the result of both supply, which it would control, and demand which is at least partially determined by these unpredictable demand shocks. Given a sufficiently large downward demand shock, the trigger price could be hit even though no firm defected from the cartel organization. Following this, firms are only capable of an *imperfect monitoring* of the market. Even with complete knowledge of the supply side of the industry, changes in the demand remain unknown and cannot be counteracted by compensatory changes in supply. In this way, the Green and Porter model is able to recreate the cartel dynamic, as exhibited in the data model, all the while retaining the essential rationality of firms.

Demand shocks however risk eliminating the agency of firms. In so far as demand shocks are neither expected nor controlled by firms, if the trigger price was reached only because of demand shocks then firms would have no say in whether or not to engage in a price war. Or in other words, the constraints facing the firm would be completely restrictive. There is a sense in which cartel organizations are designed specifically to eliminate this choice and maintain collusive market arrangements. However, in order to cohere with the rationality principle, firms must not be represented making systematic mistakes which entails that firms possess *agency*. Given this, Green and Porter posit that firms can increase or decrease *the likelihood* of reaching the trigger price (1984, p.93). Recall that firms do possess limited market power. By increasing production, an individual firm can decrease the market price. Moreover, the individual firm can get away with this production increase because of the imperfect monitoring of the cartel organization. By lowering the market price, the firm makes it more likely that a downward demand shock will result in the trigger price being activated and a price war commencing. Similarly, by increasing the market price, an individual firm can decrease the likelihood of a price war. In this way, firms retain an *imperfect control* over the occurrence of price wars. The agency of firms is maintained along with the possibility for switches between the competitive sub-model and the collusive sub-model.

The Green and Porter model succeeds in creating a dynamic model in which the cartel organization forms, collapses, and reforms in broad accordance with the relevant data models. However, it is unclear to what extent, if any, the model is representative of any real industry. Green and Porter are sensitive to this worry by

> [A]ddress[ing] the question of exactly the sort of industry [the] model might appropriately describe. Such an industry would have a structure possessing four [properties] [Green and Porter 1984, p.90].

These properties are: (i) temporal stability, (ii) homogenous goods, (iii) public information, and (iv) imperfect monitoring. Consider each in turn.

(i) *Temporal stability* holds that the structural determinants of the model remain invariant with respect to time. In the model, the industry is characterized by a number of parameters such as the number of firms, the profit function, and the discount rate. Together, these parameters allow Green and Porter to construct both the competitive sub-model and the collusive sub-model. However, in order for agents to be able to form rational expectations, these parameter values need to remain relatively stable over time (1984, p.90). The model precludes any of structural breaks that could be caused by the exit of a firm or a change in the cost of raw materials.

(ii) *Homogenous goods* holds that the goods produced and sold within the industry are not differentiable except by price, thereby precluding differences in either fundamentals or branding. The result is that there exists a uniform market price.

(iii) *Public information* holds that all participating firms have equal access to certain information relating to the overall industry and larger market environment (1984, p.90). Specifically, in order for firms to collaborate under the auspices of the cartel organization, it is necessary that all members have a generally accurate sense of market conditions and can determine whether the trigger price has been reached. Without this, the enforcement mechanism would be inoperable. There must be some private information in that only an individual firm can know its own production schedule.

(iv) *Imperfect monitoring* holds that the public information available to all member firms must be imperfectly correlated with firm behavior (1984, p.90). In other words, in order for the model to operate appropriately, there must remain probabilistic elements which constrain the collective market power of the firms. Green and Porter posit that this probabilistic element comes in the form of exogenous demand shocks, but what is important is that these demand shocks disrupt the inferential capacities of firms. Specifically, no firm can perfectly predict the market price conditional on past production schedules because demand is partially determined outside of the model.

By enumerating these four properties, Green and Porter explicitly detail the conditions under which the model must be *relevantly similar* to reality. It becomes possible to assess the representational capacity of the model by observing reality and determining to what extent any particular target industry instantiates these four conditions. Specifically, if a particular target industry were to exhibit temporal stability, homogenous goods, public information, and imperfect monitoring then it would be reasonable to deem the Green and Porter model representative of that industry. However, these four properties are not always instantiated in particular target industries. In fact, Green and Porter note that

> [w]e realize that the assumptions about industry structure are quite restrictive… [And] even though the direct applicability of our model is severely limited, it would be valuable to examine an industry for which it would be *appropriate*. We believe that the American rail freight industry in the 1880's was one example of an industry which satisfies our structural conditions *quite well* [Green and Porter 1984, p.94 (emphasis added)].

Here Green and Porter concede that few if any industries will be fully relevantly similar. However, rather than conceding defeat, they contend that some target industries might well be "appropriate" and instantiate these structural properties "quite well". In some respects, Green and Porter have shifted the goal posts away from *full* relevant similarity towards something which is more achievable.

The Green and Porter model is theoretical. It was constructed using relatively abstract concepts which were constrained by the most general principles of economic analysis. However, it remained relatively isolated from empirical studies of cartel behavior. Specifically, it was isolated from time series data on collusive industries. In order to *bridge the gap* between the theoretical model and the data models, Porter developed an empirical model of cartel behavior. In it, he altered the Green and Porter model in a number of ways which allowed him to estimate certain parameters using existing data sets and thereby it became possible for him to use the empirical Porter model to test an empirical hypothesis concerning cartel dynamics (1983 p.304). The transition from the theoretical Green and Porter model to the empirical Porter model was achieved through an *alteration process* by which more abstract theoretical properties were situated within a particular empirical setting.

Following Porter, begin by considering the particular empirical setting. In this case, he was primarily interested in the American rail freight industry of the Gilded Age. During that period of time, it was legal for firms to cooperate and coordinate prices and output levels in order to minimize costly competition. Such actions did not become illegal in the United States until the establishment of the Interstate Commerce Commission in 1887 and the passage of the Sherman Anti-Trust Act of 1890 (Porter 1983, p.302). Before cartels became illegal, they maintained detailed records. In particular, during the 1880's, the Joint Executive Committee was a cartel organization which coordinated the activities of rail freight firms conducting shipments from Chicago to the eastern seaboard of the United States. Historically, the Joint Executive Committee maintained cohesion among member firms through a trigger price mechanism akin to the one posited in the theoretical Green and Porter model. However, despite the relevant similarities between the Joint Executive Committee and the Green and Porter model, there does exist some relevant *dis*similarities between the two.

Foremost among the relevant dissimilarities between the theoretical Green and Porter model and the Joint Executive Committee is that the eastbound rail freight industry tended to compete on price rather than output. In the Green and Porter model, competitive market arrangements were represented by the implanted Cournot model. However, Cournot competition is predicated on firms competing on output rather than price (Section 2.1.3). By incorporating the Cournot model as an identifiable part of the Green and Porter model, the theoretical model took on that particular kind of competition which was relevantly dissimilar to that exhibited in the target industry. In order to rectify this relevant dissimilarity, Porter altered the theoretical Green and Porter model. He substituted the Cournot model with the Bertrand model in which firms compete on price rather than on output.[11] By so doing, Porter was able to better recreate the conditions exhibited in the target industry and bring model and target system into closer alignment. He situated the theoretical model in reality and thereby began its transformation into an empirical model. This alteration was possible only because the theoretical model was decomposable (Section 2.2.2). Without decomposability, Porter would not have been able to identify the Cournot model as a distinguishable part of the

[11] For a more extensive analysis of the difference between competing on price and competing on output, it is useful to refer back to the previous discussion of the Cournot model (Section 2.1.3).

Green and Porter model and from there, substitute it with the more applicable Bertrand model.

The transformation from the theoretical Green and Porter model to the empirical Porter model was predicated on the specificities of the target industry. In constructing the theoretical model, Green and Porter were less constrained in how they represented competition. All that was necessary was that they incorporated *some* model of competition into the theoretical model. But it was underdetermined whether firms needed to compete on output or on price. Green and Porter decided to represent firms as competing on output. However, there have long been critics who have argued that firms more generally compete on price rather than output.[12] But given that that no target system had been specified, Green and Porter were at liberty to incorporate *either* the Cournot model or the Bertrand model. It is in this way that the theoretical model can be considered as more abstract than the more highly specified Porter model in which the kind of competition is determined not by the preferences of the modelers but by the material conditions of the target industry.

[12] Commonly, historians of economic thought (and economists) have associated the major criticisms with the Cournot model with Joseph Louis François Bertrand. In 1883, he published an attack on Cournot which attempted to demonstrate that oligopolistic competition ought to be conceptualized in terms of price competition rather than output competition (1883). However, it is worth noting that these criticisms seem to have been anticipated by Charles Sanders Peirce (Wible and Hoover 2015, p.525).

By substituting the Cournot model for the Bertrand model, Porter significantly altered the empirical model. However, this alteration was relatively containable and easily achievable:

> [T]he specification of Green and Porter (1984) that industry conduct during [competitive] periods was Cournot might be considered unrealistic. Econometrically, it is not very difficult to *modify* the model so that firms revert from collusive to Bertrand behavior (as they would if they were price setters) [Porter 1983, p.303 (emphasis added)].

Porter is explicit that the model is constructed by altering or modifying the theoretical model. Moreover, he claims that it is not particularly difficult to undertake this kind of alteration. The reason why this particular alteration is relatively easy is because the theoretical model is decomposable. The sub-model exists in relative isolation from the rest of the model (Section 2.2.2). By isolating and substituting the competitive sub-model with an alternative, Porter was able to better represent the specific target industry by generating more relevant similarities between model and target system.

Following from this, Porter introduced additional alterations in order to better situate the theoretical model in the target industry and thereby construct an empirical model. He introduced alterations in order to accommodate the assumption of temporal stability built into the theoretical Green and Porter model. One implication of temporal stability was that it precluded any changes to the number of firms operating within the industry. However, the Joint Executive Committee welcomed in new firms on two occasions during the period of study (Porter 1983, p.303). The resultant relevant dissimilarity between the Green and Porter model and the target industry might well have undermined the representational capacity of the model. In order to accommodate the inclusion of new firms, Porter introduced an *indicator variable* to correct for the relevant dissimilarity (1983, p.308). Here an indicator variable refers to any variable whose range is restricted to zero and one. By introducing such a variable, it is possible to decompose a data set into distinguishable parts and incorporate certain kinds of qualitative properties into quantitative data models. Specifically, indicator variables are able to represent binary properties. Whether or not new firms joined the Joint Executive Committee in any particular time period can be constructed as a binary property and accommodated by an indicator variable.

Consider the role of data models in the alteration process. The focus on empirical models has largely been centered on how theoretical models are altered and transformed into empirical models. The Green and Porter model into the Porter model. And yet, throughout this discussion, I have detailed how the specifics of the target industry determined *which* alterations were necessary but the modeler can be aware of these specifics only by constructing and interacting with a data model. Porter was able to determine from the historical record that the number of firms in the Joint Executive Committee changed twice during the period of study. But in order to incorporate this fact into his modeling exercise, he needed to construct a data model which included the relevant indicator variable. Implicit in the creation of the indicator variable is the imposition of a uniform periodicity for the data. All the different variables were divided into weekly units. Any decomposition of the data set into distinguishable parts had to take each week as an indivisible unit. Or in other words, the decomposition required a specific degree of precision (Section 1.1.3). In this way, an indicator variable could capture only a weekly change, but not a daily change or an hourly change. The periodicity of the data constrained the indicator variables and, in this way, partially determined which alterations were undertaken to transform the theoretical model into a useful empirical model.

Additionally, the eastbound rail freight industry faced competition from an alternative shipping method. At the time there existed maritime shipping routes from Chicago through the Great Lakes which would eventually reach ports on the eastern seaboard (1983, p.303). Through these maritime routes, farmers were able to circumvent the Joint Executive Committee and bring their produce to market without relying on railroad companies. Great Lakes shipping acted as a major competitor to the Joint Executive Committee and succeeded in driving down both prices and output for member firms. However, the Great Lakes freeze in winter, limiting navigation to the warmer months. The result was a stable seasonality effect in which the Joint Executive Committee faced competition from maritime shipping during only half of the calendar year. Seasonality effects however contravene the need for temporal stability built into the Green and Porter model and act as a relevant dissimilarity between the theoretical model and the target industry. In order to correct for this seasonality effect, Porter introduced another indicator variable which decomposed the data set into two mutually exclusive categories. One of the categories included all observations in which the Great Lakes were navigable and the other included all observations in which the Great Lakes were not navigable (1983, p.307). By doing so, Porter created a data model which accommodated the seasonality of the maritime shipping and allowed him to alter the theoretical model into the empirical model.

Particular properties of the Green and Porter model did *not* need to be altered when situating it in the target industry. In the theoretical model, it was assumed that the goods were homogenous. By this I mean, the goods could be treated as perfectly interchangeable with no loss associated with substituting one unit of the good for another (Section 2.1.3). However, when considering the Joint Executive Committee and the rail freight industry of the Gilded Age, it appears that the shipped goods were not homogenous. Approximately seventy-three percent of the shipped goods during the relevant period of time were grains, but this leaves twenty-seven percent of the shipped goods as something else (1983, p.303). Importantly though, Porter deemed the goods as homogenous for his *purpose* (1983, p.303). In order to justify this, he first deemed all grain shipments as homogenous, despite the fact that different kinds of grains were shipped eastward aboard trains of the Joint Executive Committee. He then decided to treat the entire shipments as homogenous, despite the presence of non-grain goods. From a certain perspective, such moves could appear illegitimate or self-serving;

however, in order to understand them, consider the difference between precision and accuracy (Section 1.1.3)

Accuracy refers to the correspondence between properties and precision refers to the fineness of measure by which that correspondence is assessed. Precision importantly is always determined by purpose and given different degrees of precision; the accuracy of a correspondence can differ. By treating the goods in the target industry as homogenous, Porter employed a relatively coarse degree of precision. In this way, it became accurate to treat the goods shipped by the Joint Executive Committee as genuinely homogenous. Or as he put it,

> the assumption that a homogenous good was sold seems to have been approximately satisfied, and attention can be focused on the movement of grain with little loss of generality [Porter 1983, p.303].

Here, Porter signals his priorities. He is less interested in the precise composition of the freight tonnage. He argues that as grains constituted the bulk of goods shipped by firms in the target industry, it was reasonable to treat freight shipments as homogenous especially as shipping costs are only determined by weight and volume. In order to justify this, he referenced the *generality* of his empirical model and with it, the purpose behind the broader modeling exercise.

For Porter, the theoretical model was designed in order to *explain*. He and Green designed it to explain how a cartel dynamic could arise given the rationality of participating firms (Porter 1983, p.301).[13] However, he conceived of the empirical Porter model as fulfilling quite a different purpose. He stated that

> [i]ndustrial organization economics have recognized for some time that the problem of *distinguishing* empirically between collusive and noncooperative behavior, in the absence of a "smoking gun", is a difficult one [Porter 1983, p.301 (emphasis added)].

The model was designed in order to *detect* a particular phenomenon. The theoretical Green and Porter distinguished between competitive market arrangements and collusive market arrangements. And while it was easy to distinguish these kinds of arrangements theoretically, it was not always apparent in the data which regime was in place at a particular time without access to unknown, and often unobservable, structural properties. By situating the model in the target industry, Porter was able to gain empirical insights. Specifically, he claims that

> [t]his article adopts econometric techniques which employ aggregate time series price and quantity data for a particular industry, and which are designed to *detect* the behavioral switches implied by the enforcement mechanism [Porter 1983, p.302 (emphasis added)].

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[13] It is worth noting that the explanatory power of the Green and Porter model is essentially *counterfactual*. Through the construction of the model, they determined under what conditions the cartel dynamic could possibly occur. In this respect, the explanatory strategy pursued in the model could be roughly identified with a *how-possibly explanation*, as opposed to a *how-actually explanation* (Grüne-Yanoff and Verreault-Julien 2021). Commonly, how-possibly explanations are treated as less informative than how-actually explanations for reasons similar to those used to characterize idealizations as distortions (Section 3.1). And yet, given that all models necessarily differ from reality in regard to certain properties, it is unclear whether there is a genuine distinction here. All models provide a counterfactual how-possibly explanation which, when restricted to particular properties and a particular degree of precision, can become actualized in a target system. In other words, the distinction between how-possibly explanations and how-actually explanations arises from the false ideal of completeness and can be dispensed with.

Detection can, roughly, be identified with *measurement*. For this reason, it is reasonable to understand the Porter model as a measurement tool (Section 3.1.5). It is designed explicitly to allow the scientist to measure both the frequency and the duration of collusive arrangements.

Throughout, purpose determines both relevancy and precision (Section 2.1.2). It is for this reason that the goods shipped in the target industry can legitimately be treated as homogenous. The precise composition of the cargo is irrelevant to the measurement exercise behind the Porter model. For him, it did not matter what precisely was being transported but rather the underlying market arrangements among firms. Given a relatively uniform market price, it was unimportant whether approximately a quarter of the goods shipped by members of the Joint Executive Committee were not grains. In fact, given the purpose guiding Porter, it is reasonable to adopt a relatively imprecise metric for assessing the homogeneity of goods. This entails that the goods were genuinely homogenous. On the other hand, given the measurement purpose, it was necessary to account for the seasonality effect associated with maritime shipping across the Great Lakes as such competition would impact the relevant time series data and undermine the empirical basis for the model. In this way, *purpose* determined which alterations were necessary for the model to be made to work.

4.3 Alteration and Progress

In order for models to work they must be constrained. Purpose constrains by determining relevance and precision, and reality constrains in so far as accuracy is determined by the interaction of model and reality. However, these constraints are always mediated. Purposes are always pursued under the auspices of some principles which determine the broad ontological and metaphysical commitments of the scientist (Section 1.1.2). Similarly, reality is mediated through data models, as reality is not directly epistemically accessible, as perception always entails some interpretative representational process. The result is that the logical structure of scientific modeling is largely determined by constraints. But not all models face the same constraints. Specifically, theoretical models face different constraints than empirical models.

Consider the theoretical Green and Porter model. In it, the constraining principle was the *rationality principle*. It required that the model represent agents as rational and optimizing. For this reason, they could not represent the cartel as a coercive organization but rather as a voluntary and beneficial collaboration (Section 4.2). The theoretical model was also constrained by data models, but only imprecisely. From the data, Green and Porter were able to discern a general empirical pattern in which cartels would form, collapse, and then reform. The theoretical model did not need to recreate any specific data set, but rather the generalizable pattern evident throughout. The empirical Porter model faced similar constraints but there was an important difference. It too was constrained by the rationality principle and the data models. However, because the Porter model was altered so as to represent a *particular target system*, it was constrained by the precise composition of a specific data model. In this way, it was not enough that the Porter model cohere to a general regularity, but rather it was constrained by the precise data set. In this case, the purposes determined the relevant degree of precision which in turn determined the specific contour of the constraint.

Following from the different constraints facing theoretical and empirical models, these different kinds of models can be associated with different kinds of inferences. Theoretical models are, generally, better constructed for general or type-based explanation. Because theoretical models are relatively abstract, they possess the versatility to represent a wide range of targets. In this way, theoretical models can provide a *unificatory explanation* by which disparate targets are understood to possess relevant properties in common (Section 3.1.1). Additionally, the relative abstractness of theoretical models can prove useful in representing causal relationships. Given that causality is only ever understandable when the causal relationships are relatively distinguishable, the relative paucity of theoretical models allows the scientist to effectively represent only those causal relata of interest.[14] Consequently, theoretical

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[14] Following Simon and Rescher, it is useful to understand *causal identifiability* as depending upon the scarcity of causal relationships (1966, p.330). More specifically, Simon and Rescher conjecture that the identification of causal structure depends upon two postulates: (i) prepotence and (ii) independence. Prepotence holds that the differential power amongst causal relationships that certain such relationships are overwhelmed and can be treated as negligible. Similarly, independence holds that certain objects are just not

models are often excellent vehicles for *causal explanation*. It is necessary to clarify that unificatory and causal explanations are neither in conflict nor exhaustive. There are plausibly other explanatory strategies which could be employed throughout the sciences. Moreover, while theoretical models may be best suited for explanation, both unificatory and causal explanations can be accomplished with empirical models, especially when scientists are interested in explaining highly specific or precise facts. Here it is once again necessary to recall that the boundaries between theoretical and empirical models is necessarily vague and certain borderline cases may be used for explanation and still plausibly be considered empirical.

Along a similar line, classification is generally best achieved with a theoretical model, although early embarkation classificatory systems are often developed without input from theoretical models or principles. However, in order for scientific research to commence, there must be *some* classificatory system (Section 3.1.4). The ontology can, and most often does, change in response to new theoretical insights and empirical observations. But some classificatory framework is required to begin research. Oftentimes, the most general ontological categories are provided by principles but even when this is the case, it is almost always necessary to concretize such ontological

causally related at all. The result of these two postulates is that there is no dense causal web in which all objects are causally related to each other, thereby precluding causal identification. The abstractness of theoretical models similarly precludes such a causal web and consequently allows for the kind of causal identification necessary for causal explanation.

categories by enmeshing them in a richer classificatory framework. In classical mechanics, the Newtonian principles provide the category of force, but in order to apply it to any particular target, it is necessary to concretize the system through a theoretical model. By so doing, it becomes possible for the scientist to classify the objects in a particular target and thereby engage in the other kinds of epistemic purposes characteristic of scientific research. Once again, it can be possible for empirical models to further the classificatory program and this is often done in biological systematics (Section 3.1.4). However, even in those circumstances where empirical models are used to enlarge and deepen a classificatory framework, it is done using the categories supplied by theoretical models.

Empirical models are however better suited for measurement and prediction. In regard to measurement, the usefulness of empirical models is relatively straightforward. The alteration process involves melding the theoretical model with the data model in such a way as to create relevant similarities. In this way, the empirical model is designed to cohere well with the accessible data sets. But as data are almost always incomplete, in that there are certain properties of the world which cannot be observed, the empirical model provides the resources to allow the scientist to measure unobservable properties. For example, the Solow-Swan model is used to measure technological change (Section 3.2). Known as the *Solow Residual*, the model when altered so as to incorporate time series data, can reveal which proportion of economic growth ought to be attributed to technological change by its observed absence. By this I mean, as the Solow-Swan model represents economic growth as a function, in part, of technological change, by analyzing all other factors through the model, the unaccounted residual can be, reasonably, attributed to the contribution of technological change. In this way, the empirical model can be used for measurement. Similarly, such models can be used for forecasts and counterfactual predictive analysis (see Section 3.1.2).

Given that scientists regularly shift between different kinds of purposes, it is understandable that numerous procedures have been developed for altering theoretical models into empirical models, and vice versa. In this way, through the alteration process, models can be made to work. And moreover, models can be made to work for various purposes. Broadly, alterations are made by changing the relevant constraints. For example, if one is attempting to represent a very specific target system, then the precise data model will become relevant in a way which it would not be when representing a more general target system. In this way, the constraints facing the model change. When Porter altered the theoretical model, he changed the constraints by incorporating the specifics of the relevant data set. For example, the freight industry faced maritime competition, as evidenced by the data. In order to accommodate this new constraint, Porter altered the model by incorporating an indicator variable. However, the alteration process is not algorithmic. It is an essentially creative processes which cannot be fully explained nor recreated (see Section 1.3). But from this creative process, scientific modeling becomes a *progressive* endeavor.

By altering models, scientists create more and more scientific models. Each newly minted model is altered given a particular purpose, resulting in *tailor made* models. These bespoke models are designed to be useful and usually are. Consider the empirical Porter model. Unlike the theoretical Green and Porter model, which was designed to *explain*, it was a primarily constructed for *measurement*, although oftentimes token-based explanation can blur into measurement. It was altered specifically in order to allow for the measurement of changes in the market arrangement. Through the alteration process, Porter created a new model which could do something new and do it well. In other words, he allowed for an important kind of specialization. The theoretical model no longer needed to be used to measure, it could be used exclusively for explanation. Similarly, the empirical Porter model needed to be used for only measurement and did not need to be used for classification or explanation. Generally, alteration allows for *inferential specialization*.

From specialization comes productivity. As each model becomes tailored for a specific purpose and a particular target system, each model can be more effectively used. The result is that most models become more useful. In many ways, this is a Smithian insight:

> [t]he greatest improvement in the productive powers of labour, and the greater part pf the skill, dexterity, and judgement with which it is any where directed, or

> applied, seem to have been the effects of the division of labour [Smith 1776/1981, p.13].

Thinking of models as tools, through specialization models become better suited to different purposes, both resulting in and causing useful divisions in inferential labor. Here, the gains from alteration should not be understood in terms of verisimilitude. It is possible that through the alteration process, some newly minted models are more "truth-like", but it is difficult to define precisely what is meant by verisimilitude and it is impossible to empirically confirm verisimilitude. However, given that the ideal model is a useful model, it follows that any improvement in the usefulness of scientific models ought to be thought of as progressive (Section 3.1). Alterations enable specialization which leads to a division of inferential labor which in turn enables scientific modeling to progress through time.

4.4 Conclusion

In order to understand the logical structure of scientific modeling, it is useful to consider the *iterative alteration process* by which models are made to work. Throughout this chapter, I have defended the claim that scientific modeling is an exploratory process rather than a reversible idealization and deidealization process. From this, I distinguished between *theoretical* models and *empirical* models and detailed how the alteration process allows the scientist to transform models and tailor them for specific purposes. Given this, the alteration process results in *specialized* scientific models which yield a division of inferential labor, making scientific modeling progressive.

5. Conclusion

Generally, throughout this dissertation, I have articulated and defended an account of the logical structure of scientific modeling. The account began with the simple observation that scientific models do work (Section 1). I then noted the major kinds of inferential purposes for which models are used: *explanation*, *prediction*, *exploration*, *classification*, and *measurement* (Section 1.1.1). Building from there, I developed an account of representation in which conventions, purpose, and relevant similarity constituted the representational relationship by which models were able to work (Section 2.1). I then defended this account of representation against potential challenges which attempted to either preclude the inclusion of purposes into scientific representation or hold that models were not decomposable (Section 2.2). With both challenges defused, I demonstrated *how* models are used.

In order to better understand the logical structure, I then detailed the *ideal model*. Specifically, I argued that the ideal model is useful (Section 3.1). More specifically, I argued that in order for a model to be useful, it needed to be idealized. I then detailed a number of *idealization strategies* through which scientists were able to make their models useful (Section 3.1). However, in order to defend my claim that the idealized model is useful and idealized, I needed to demonstrate that completeness is a false ideal. To that end, I demonstrated that completeness is both unachievable and not a worthwhile aspiration (Section 3.3). But in order to understand how models are made useful, I detailed how models could be altered through a process of discovery (Section 4.1). Through all this, I have demonstrated not only *how* models work, but how models are *made to work*.

Appendix A: Solow Swan Model

1. Cobb-Douglas Production Function

In the Solow-Swan Model, the primary theoretical object is the neoclassical production function. For expositional ease, I have opted to present a particular specification of the neoclassical production function known as the Cobb-Douglas production function:

$$Y = AK^{\alpha}L^{\beta}. \tag{1}$$

In this equation, GDP (Y) is a function of technology (A), capital (K), labor (L), the elasticity of capital (α) and the elasticity of labor (β). In the Cobb-Douglas production function, it is assumed that $(\alpha + \beta) = 1$ and that the two elasticities remain constants. Moreover, all of the idealizations which characterize the neoclassical production apply to the Cobb-Douglas production function, including constant returns to scale. This idealization allows one to transform equation (1) into *per worker* terms:

$$y = A(K/L)^{\alpha}(L/L)^{\beta}. \tag{2}$$

Here *per worker* GDP (y) is equal to Y/L. Moreover, because $L/L = 1$, it is possible to simplify equation (2):

$$y = Ak^{\alpha}\,, \tag{3}$$

where *per worker* capital (k) is equal K/L.

2. Model Dynamics

Given that the Cobb-Douglas production function specifies that GDP is a function of technology, labor, and capital, in order to characterize the dynamics of GDP it is necessary to characterize the dynamics of these three factors. In the simple version of the Solow-Swan model, technology is assumed to remain constant:

$$\dot{A} = 0. \tag{4}$$

Here the rate of change of technology with respect to time ($\dot{A} \equiv \frac{\partial A}{\partial t}$) is treated as *exogenous*.

Similarly, the dynamics of labor are treated as *exogenous*:

$$n \geq 0. \tag{5}$$

Here the percentage rate of change in labor with respect to time ($n \equiv \dot{L}/L$) is assumed to be constant. Finally, the rate of change in capital ($\dot{K} \equiv \frac{\partial K}{\partial t}$) is represented as a function of investment minus depreciation:

$$\dot{K} = \iota Y - \delta K. \tag{6}$$

Here the investment rate ($\iota \equiv 0 \leq \iota < 1$) represents the proportion of income saved and invested by households in the model economy and the depreciation rate ($\delta \geq 0$) represents the percentage of the capital stock which breaks down in any given time period. Given the idealization of constant returns to scale, it is possible to transform equation (6) into a *per worker* form:

$$\dot{k} = \dot{K}/L - (K/L^2)\dot{L}. \tag{7}$$

Here $\dot{k}$ denotes the *per worker* rate of change in capital. By substituting equation (6) into equation (7) and simplifying, it is possible to derive

$$\dot{k} = (\iota Y - \delta K)/L - (K/L)(\dot{L}/L). \tag{8}$$

By further simplifying equation (8) it is possible to derive the following:

$$\dot{k} = \iota(Y/L) - \delta(K/L) - kn \tag{9}$$

$$\dot{k} = \iota y - \delta k - kn \tag{10}$$

$$\dot{k} = \iota y - (n + \delta)k \tag{11}$$

In equation (11), the rate of change in *per worker* capital is a function of *per worker* investment and decreases in the *per worker* capital stock. Note that the *per worker* capital stock can decrease due to both depreciation and the increasing labor force due to the dispersal of a finite amount of capital across a larger population. However, note that in equation (11), *per worker* investment is a function of the investment rate and *per worker* GDP. It is therefore possible to substitute the *per worker* Cobb-Douglas production function into equation (11):

$$\dot{k} = \iota A k^{\alpha} - (n + \delta)k. \tag{12}$$

The substitution, by combining the dynamics of capital with the Cobb-Douglas production, yields the *fundamental equation* of the Solow-Swan model.

3. Steady State

The steady state of the model is formally defined as the condition in which $\dot{k} = 0$ which means that the *per worker* capital stock is constant. Therefore, in the steady state, increases in capital due to investment must counteract decreases in the capital stock due to depreciation and an increasing population. This can be formally represented by rearranging equation (12):

$$\iota A k^{\alpha} = (n + \delta)k. \quad (13)$$

Moreover, in the steady state, it is possible to determine percentage rate of change of capital:

$$\dot{k} = \frac{\partial(K/L)}{\partial t} = 0. \quad (14)$$

From equation (14), by multiplying through by (L/K),

$$\dot{K}/L - (K/L^2)\dot{L} = 0. \quad (15)$$

By multiplying through by (L/K) again,

$$\dot{K}/K - \dot{L}/L = 0. \quad (16)$$

Moreover, because $\dot{L}/L = n$, through substitution,

$$\dot{K}/K - n = 0. \quad (17)$$

Finally, because equation (17) is equal to zero, it is possible to rearrange:

$$\dot{K}/K = n. \quad (18)$$

Equation (18) entails that the percentage rate of change of capital with respect to time is equal to the percentage rate of change of labor with respect to time (n). Moreover, because n is assumed to be an exogenously determined constant, the percentage rate of capital is an exogenously determined constant in the steady state of the Solow-Swan model. This entails that there remains a constant amount of *per worker* capital as increases in the capital stock are offset by increases in the labor force which entails that in the steady state, *per worker* GDP remains constant.

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Biography

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Kobi Finestone has published two articles during his doctoral studies. The article *Crisis Prices: The Ethics of Market Controls during a Global Pandemic*, co-authored with Ewan Kingston, appeared in Business Ethics Quarterly in 2021. The article details the ethical implications raised by spatially and temporally extended crises which disrupt supply chains and which can result in acute price increases. The article *Darwinian Rational Expectations* appeared in the Journal of Economic Methodology in 2022. The article presents an evolutionary account of rational expectations which represents economic forecasting as a survival mechanism rather than a deliberative process.

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