

2013

Scientific modeling & scientific realism : a view from biological morphology

Bradley Morgan Wood

Louisiana State University and Agricultural and Mechanical College

Follow this and additional works at: https://digitalcommons.lsu.edu/gradschool_theses



Part of the [Arts and Humanities Commons](#)

Recommended Citation

Wood, Bradley Morgan, "Scientific modeling & scientific realism : a view from biological morphology" (2013). *LSU Master's Theses*. 2860.

https://digitalcommons.lsu.edu/gradschool_theses/2860

This Thesis is brought to you for free and open access by the Graduate School at LSU Digital Commons. It has been accepted for inclusion in LSU Master's Theses by an authorized graduate school editor of LSU Digital Commons. For more information, please contact gradetd@lsu.edu.

SCIENTIFIC MODELING & SCIENTIFIC REALISM:
A VIEW FROM BIOLOGICAL MORPHOLOGY

A Thesis

Submitted to the Graduate Faculty of the
Louisiana State University and
Agricultural and Mechanical College
in partial fulfillment of the
requirements for the degree of
Master of Arts

in

The Department of Philosophy and Religious
Studies

by
Bradley M. Wood
B.A., B.S., Louisiana State University, 2011
May 2013

ACKNOWLEDGMENTS

The development and production of my thesis has been made possible by the thoughtful contributions of friends, family, and advisors. I owe the privilege to pursue my academic interests and the freedom to dedicate the time necessary for the production of the current work to the unwavering financial, emotional, and intellectual support of my parents, Charlie and Tammy Wood.

Their encouragement and understanding has served as a dependable source of inspiration and motivation, and I hope that I have made the most of the opportunities they have afforded me. I am also grateful for the intellectual freedom and philosophical guidance provided by my thesis advisor, Prof. Jeffrey Roland. He has rescued me from countless philosophical confusions, and he has been an invaluable source of clarification (and occasional foil) for the complex views of Richard Boyd and issues in the philosophy of science. I appreciate the time and input of my committee members, Profs. Jon Cogburn, Dominique Homberger, and Husain Sarkar. I would like to thank Prof. Dominique Homberger, Brooke Dubansky, and Michelle Osborn for their help in clarifying the intricate processes of morphological modeling as performed by practicing morphologists. Lastly, I would like to thank Claiborne Macknight for her willingness to act as a sounding board for my ideas and for her dedicated intellectual and emotional support.

TABLE OF CONTENTS

ACKNOWLEDGMENTS	ii
LIST OF FIGURES	iv
ABSTRACT	v
INTRODUCTION	1
1. MORPHOLOGICAL MODELING	5
1.1. Morphological concepts.....	5
1.2. Morphological modeling	13
1.3. Modeling the human bronchial tree.....	26
2. A PHILOSOPHICAL ANALYSIS OF SCIENTIFIC MODELING	40
2.1. Morphological data and organismal features	40
2.2. Morphological data, models, and organismal features	46
2.3. Organismal features, models, and morphological theories.....	56
2.4. Multiperspectival morphological models.....	58
3. AN ACCOMMODATIONIST VIEW OF SCIENTIFIC REALISM	63
3.1. Correspondence truth	65
3.2. The mind-independence of reality	66
3.3. A non-Humean account of causation	67
3.4. Disciplinary matrices, architectures, and accommodation demands.....	69
3.5. Homeostatic property cluster natural kinds.....	70
3.6. A causal notion of reference	73
3.7. The theory-dependence of scientific methods.....	78
3.8. Approximate truth	86
4. A REALIST VIEW OF MORPHOLOGICAL MODELS	90
4.1. Morphological modeling: A process of accommodation	91
4.2. The theory-dependence of model interpretation and representation	105
5. OBJECTIONS TO A REALIST VIEW OF MODELING.....	112
5.1. The underdetermination of models argument	113
5.2. The conventionality of models argument.....	116
5.3. The complexity problem	120
CONCLUSION	125
REFERENCES.....	127
VITA.....	136

LIST OF FIGURES

1. Max-intensity projection of a 6 cm horizontal slab of the HRCT data displayed with inverted grey scale. Taken from Schmidt et al. (2004, p. 204).	33
2. Bronchi surface model extracted from the bronchi segmentation model. Taken from Schmidt et al. (2004, p. 204).	34
3. Geometric surface bronchi model produced from the bronchi topology model. Taken from Schmidt et al. (2004, p. 204).	35
4. Lung model with bronchi segmentation model. Taken from Schmidt et al. (2004, p. 204).	36
5. Pressure distributions across the 17-generation bronchi pressure model for a volumetric flow rate of 30 L per min. Taken from Gemci, Ponyavin, Chen, Chen, and Collins (2008, p. 2050).	37
6. Flowchart combining multiperspectival modeling (MPM) with Boyd's view of reference. MPM tracks causal phenomena to produce the representation, R , which informs the definition of the natural kind, N_k , that is being referred to by the natural kind term, t_k , which is also tracking the same causal phenomena underwriting the definition of N_k and the content of R	111

ABSTRACT

In the ongoing philosophical debates between scientific realists and antirealists, scientific modeling is often taken as an exemplar antirealist scientific methodology due to the abstract, idealized, and metaphorical nature of most scientific models. I argue against the antirealist view and in favor of a realist view of scientific modeling as it is performed in biological morphology. On my view, morphological modeling is a type of what I call *multiperspectival modeling*, which involves multiple related models deployed to represent a single target phenomenon. I show how multiperspectival morphological modeling can be incorporated into the version of scientific realism developed by Richard Boyd, known as accommodationism, based on the role modeling plays in informing the definitions of natural kind terms and on the role theoretical judgments play in model construction and deployment. I claim that multiperspectival morphological models contribute to the inductive and explanatory successes of biological morphology by playing a central role in accommodating (on the one hand) the inferential, conceptual, and classificatory practices of morphology to (on the other hand) independently existing causal phenomena. I intend for the realist view of morphological models presented here to serve as an example for how scientific modeling can be interpreted realistically across scientific disciplines.

INTRODUCTION

Scientific modeling is a method scientists use to bring clarity and coherence to complex problems that cannot be addressed by scientific theories or direct observation alone. The role of scientific models as mediators between theory and the world raises important philosophical questions, such as the extent to which models can be employed to adequately and accurately explain actual phenomena (see for example the papers in Morrison and Morgan (1999)). Antirealist philosophers of science argue that models are nothing more than reliable applications of methodology and should not be taken as realistic representations of the world (Botha, 1986; Horan, 1988; Suarez, 1999). According to the antirealist, the use of models as idealizations, abstractions, or metaphors intentionally detaches the model from the actual world and, therefore, while they may aid a scientist in understanding their phenomena under study, models cannot be used in confirmation or explanation. For the antirealist, models serve only to (heuristically) increase our confidence in our current scientific theories because models can provide a pictorial representation of complex or ambiguous theoretical claims.

I intend to counter the general claims antirealists make about scientific models by providing a realist account of scientific modeling as I understand it to be employed in biological morphology. I think the antirealist claims are based on a distorted view of scientific models, as the predominant philosophical work on models is strongly influenced by philosophy of physics (Bailer-Jones, 2009). By arguing in favor of a realist view of models as used in morphology, I will argue, as a consequence, that any approach to scientific modeling should be discipline dependent, avoiding any attempt at an *en masse* philosophical view of scientific models.

On my view, the process of modeling in biological morphology leads to the deployment of multiple model types and expressions to bear on a particular target, such as a structure or func-

tion of an organism. Each model acts as an independent “lens” through which the structure or function can be viewed. While each separate model of a structure or function may not be able to confirm theories or produce accurate and adequate explanations alone, the amalgamation of all the models *does* provide confirmation for biological theories and is a source of morphological explanations. I intend to argue that the view resulting from the amalgamation of morphological models, a view I call *multiperspectival modeling*, can be readily integrated with the accommodationist version of scientific realism. The end result of model amalgamation is accommodation as understood by Richard Boyd because the process by which morphological models are constructed, tested, and ultimately integrated with one another results in their tendency to track or represent the properties constituting their target phenomenon.

In Chapter 1, I discuss in detail the process of morphological modeling. My approach to morphology and morphological modeling is strongly influenced by Walter Bock, Gerd von Wahlert, and Dominique G. Homberger because they provide the most coherent view of the inferential practices in morphology and the process of morphological modeling. The first two parts of Chapter 1 address the technical aspects of morphology and modeling in morphology, and I illustrate these aspects in the third part of Chapter 1 with a discussion of the morphological modeling of the human lung. A clear exegesis of morphological modeling allows for a thorough analysis of its philosophical attributes.

In Chapter 2, I discuss the philosophical aspects of morphological modeling by applying philosophical studies on scientific modeling to modeling as it is performed in morphology. In this chapter, I discuss the philosophical importance of representations produced by models and the relationships between observational data, models, model users, and theories. I also introduce my philosophical view of models, which I call *multiperspectival modeling*. My approach to the

philosophy of scientific modeling is strongly influenced by the work of Daniela Bailer-Jones, especially since she has (uniquely) approached the study of modeling without committing to a discipline specific modeling process.

In Chapter 3, I describe the main tenets of Richard Boyd's version of scientific realism known as *accommodationism*. Accommodationism follows from an abductive argument for scientific realism proposed and extensively developed by Boyd. His view incorporates a notion of correspondence truth, the mind-independence of reality, a non-Humean account of causation, a notion of discipline specific inferential architectures, an account of natural kinds as homeostatic property clusters, a causal notion of reference, a view of scientific methods as thoroughly theory-dependent, and a reliance on (or at least a notion of) approximate truth. I briefly describe each of these aspects of accommodationism before integrating them with my discussion of morphological modeling.

In Chapter 4, I discuss how my view of scientific modeling (multiperspectival modeling) can be integrated with Boyd's accommodationist view of scientific realism. I argue that the instrumental reliability of morphological modeling, which I take to be uncontroversial, can be explained by the accommodationist version of scientific realism due to the contribution models make to defining natural kind terms and to the role theoretical judgments play in model construction and deployment. Models contribute to the definitions of natural kind terms through the representations they provide of the natural kinds to which the terms refer. Because modeling is largely a theoretical exercise, I contend that the best argument for the contribution models make to defining natural kind terms is that the representations models produce are approximately true.

In Chapter 5, I defend my realist view of modeling against possible objections from empiricism, constructivism, and an account of phenomenal complexity. My replies largely follow Boyd's defense of scientific realism against similar objections in the philosophy of science.

1. MORPHOLOGICAL MODELING

Nearly every practicing morphologist eventually seems to feel the need to philosophize. (Herring, 1988, p. 189)

Biological morphology is the study of organismal phenomena, which can loosely be thought of as the properties (attributes, qualities, or characteristics) and relations that can be predicated of or attributed to biological organisms. Scientific modeling is one of the primary methods morphologists use to carry out their studies of organismal phenomena. Morphologists use models to interpret observational data collected about organisms and to produce from their interpretations descriptions and explanations of particular organismal phenomena.¹ Before providing a philosophical account of morphological modeling (see Chapter 2), I give a brief description of how morphologists construct and deploy their models in practice. First, I describe a conceptual scheme described by Walter J. Bock and Gerd von Wahlert that morphologists might employ to theoretically organize organismal phenomena prior to modeling them (§1.1.). Then, I describe how this conceptual scheme guides the modeling process (§1.2.). Finally, I illustrate the process of morphological modeling using a case study of how morphologists have conceptualized and modeled the human bronchial tree (§1.3.).

1.1. Morphological concepts

Organisms are complex entities comprised of a multiplicity of functionally interdependent parts and processes. Because of the functional interdependence of organismal phenomena, morpholo-

¹ Morphological models are deployed in four main morphological disciplines: *theoretical morphology*, which studies theoretically possible and impossible morphological structures within an imaginary morphospace; *constructional morphology*, which studies morphological structures within adaptational, morphogenetic, and historical constraints; *morphometrics*, which studies morphological structures through use of topological methods; and *functional morphology*, which studies the mechanical properties of morphological structures. In what follows, I focus on how models are constructed and deployed within functional morphology, but my general analysis is applicable to the other morphological disciplines. When I use the terms ‘morphology’ and ‘morphologist’ it will be in the context of functional morphology.

gists have often compared organisms to machines (see for example Bock (1989) and Homberger (1986, 2001)).² Analogizing organisms to machines provides a conceptual framework that enables morphologists to study organisms as individual parts that together produce a functioning whole. This is an especially helpful analogy for modeling organisms as Dominique G.

Homberger (1986) explains in the following passage:

By taking apart such a machine and reconstructing a model from its components, it should be possible to acquire an understanding of the functions, interactions, and interdependencies of the constituent parts, provided one knows the basic function of the machine, the structure and spatial arrangement of the constituent parts, the physical properties of the materials from which the parts are made, and the physical and mechanical laws governing the interactions of the parts.
(Homberger, 1986, p. 78)

There is less agreement on what conceptual scheme morphologists should adopt to describe the arrangement of these parts and their relationship to the entire organism, however. Bock and von Wahlert (1965) describe a conceptual scheme that takes organisms to be dividable into groups of related properties known as *features*, which can be subdivided into subgroups of related properties known as *forms*, *functions*, and *faculties*. I have decided to follow their conceptual scheme for two reasons: 1) To my knowledge, their study is the first attempt at providing a systematic account of the concepts morphologists have historically used to organize their studies of organismal phenomena, and 2) while morphological studies following Bock and von Wahlert (1965) have used various conceptual schemes for organizing organismal phenomena, the concepts those studies have employed (such as organismal structures, characters, traits, etc.) can be incorporated within the conceptual scheme described by Bock and von Wahlert. Although the conceptual scheme Bock and von Wahlert (1965) describe provides greater clarity in discussing morphological modeling, its adoption is a contingent issue and is not necessary for the acceptance or the evaluation of the methodological or philosophical claims I make.

² This is not to suggest that morphologists view organisms *as* machines as a Cartesian might.

According to Bock and von Wahlert (1965), organisms are comprised of *features*. I take features to be loosely defined by groups of related properties that co-occur within a population of organisms. The aim of morphology is the identification and investigation of organismal features and the roles they play in the lives and evolution of organisms. According to Bock and von Wahlert, each feature can be subdivided into *forms*, *functions*, and *faculties*, which I take to be loosely defined by subgroups of properties within the group of properties that defines a feature. The forms, functions, and faculties of features are hierarchically related in the following way: the subgroup of properties defining a particular form will partially constitute the subgroup of properties defining a related function, but not (necessarily) *vice versa*. Likewise, the subgroup of properties defining a particular function, including those it shares with related forms, will partially constitute the subgroup of properties defining a related faculty, but not (necessarily) *vice versa*. Forms are the lowest-level in the hierarchy, faculties are the highest-level, and functions are situated in between forms and faculties. Although features are the groups of properties that are subdivided into forms, functions, and faculties, they are not hierarchically related to forms, functions, and faculties because not all properties used to define a feature will be used to also define its forms, functions, and faculties.

1.1.1. Feature

According to my interpretation of Bock and von Wahlert (1965), a *feature* is a group of related properties that corresponds to any part, region, or process of an organism that can be roughly, that is, imprecisely, identified and demarcated by morphologists and made the focus of a biological study. For example, the *mandible* of chimpanzees, the *heart valves* of spiny dogfish sharks, the *chorion* of roach eggs, and the *biceps femoris muscle* of humans are all features that have been the target of morphological studies. According to Homberger (1988), a feature is “estab-

lished” upon the formulation of a precise question posed by a morphologist within the theoretical and inferential constraints of her discipline about a part, region, or process in an organism that is characterized by a related group of properties. According to Homberger (1988), a feature under study will be the target of a question “How does X contribute to the life or evolution of the organism?” where X is a group of related properties that co-occur within a population of organisms. In other words, features are those things about organisms that morphologists seek to explain.

Once a feature is established as the target of a particular study, the group of properties used to define it can then be subdivided into subgroups of related properties to establish forms, functions, and faculties of the feature. However, forms, functions, and faculties cannot be described prior to the establishment of a feature, because a group of properties must (obviously) come before its subgroups. Furthermore, forms, functions, and faculties must be described and established in succession according to their hierarchical relationships. Therefore, a morphologist cannot begin investigating the forms of a feature until that feature is established, and she cannot begin investigating the functions or faculties of the feature until the feature’s forms have been described and established. As such, the establishment of a feature through the posing of a precisely formulated question is the most important step in investigating organismal phenomena and in deploying Bock and von Wahlert’s conceptual scheme.

Because a feature is taken to be some part, region, or process *in* an organism, the group of properties that define it will never encompass all the properties that can be predicated of an organism in its entirety; an organism is defined by a suite of parts, regions, and processes, but the latter are not defined by reference to the former. That is, a whole organism cannot be a single feature. On the other hand, a feature can be any part, region, or process in an organism, no mat-

ter how small, as long as it is defined within the investigative scope of biological morphology. For example, the collagen proteins in cartilage are candidate features for morphological study, but the atoms composing these proteins are not. Atoms are beyond the scope of morphology (but they are not beyond the scope of molecular chemistry or atomic physics). Regardless of what part, region, or process in an organism a morphologist defines as a feature, the properties included in the group defining a feature will be contingent upon the morphological study of which it is a focus. In other words, organisms do not possess essential features.³

The absence of essential features does not preclude the use of a common feature as the subject of multiple morphological studies or as a constituent in the formulation of broad explanations. In practice, different morphologists from different areas of expertise often study different properties within a group defining a common feature in a joint effort to increase their understanding about the feature as a whole. For example, an osteologist, a biomechanist, and a physiologist can independently study different properties of the mandible of a chimpanzee, which acts as the feature or common group of properties shared in each of their studies. Studies of some features, especially those of “model organisms”, are often used in the construction of broad explanations that make claims about multiple species of organisms. For example, extant alligators are often taken as model organisms for understanding the biology of dinosaurs (see for example Sanders and Farmer (2012)). Furthermore, features established by morphologists are often used in other disciplines such as taxonomy and embryology for the formulation of discipline specific explanations. Regardless of where and how they are used, the descriptions of features, that is, the groups of properties that define them, are always susceptible to revision. For example, the bones of the vertebrate skull were once thought to originate from the cranial most

³ By not possessing essential features, I mean that organisms are not defined by a set of necessary and sufficient features such that checking off each feature indicates the presence of an organism of that type. However, as I argue later in the thesis, organisms do have real essences, but not essential features.

vertebrae as described in morphological studies of the 19th century. However, modern morphological studies have undermined this claim, and the bones of the skull have been redefined as being unrelated to vertebrae. This susceptibility to revision also holds for forms, functions, and faculties.

1.1.2. Form

Every feature can be subdivided into at least one form. According to my interpretation of Bock and von Wahlert (1965), a *form* is a subgroup of related properties that corresponds to a temporally specific appearance, shape, configuration, or material composition of a part, region, or process in an organism. For example, the *width* of a chimpanzee mandible, the collagen fiber *direction* in the heart valves of a spiny dogfish shark, the *configuration* of proteins composing the chorion of roach eggs, and the *cross-sectional area* of a human biceps femoris muscle are all forms of their respective features.

Features can be subdivided into multiple forms, especially if a feature is partially defined by a *process* or *action*. A feature defined by a process, such as a pumping human heart, is composed of a sequence of forms that can be identified at specific time slices during the process. For example, the human heart pumps blood in two recurrent phases: a systolic phase and a diastolic phase. If we want to study the internal changes the heart experiences during each phase, we can take a time slice of the heart corresponding to the start of systole and study its internal configuration at that time. Each configuration isolated by a time slice corresponds to an individual form of the heart. In other words, the configurations characterizing the pumping process of a heart can be described as a series of forms.

A feature defined by a part or region may also have more than one form because it may perform some *action*. For example, the biceps femoris muscle in humans adducts the forearm.

Before this muscle contracts, it will have a particular configuration, and this configuration will differ from its configuration after it flexes. These two configurations represent two different forms of the biceps femoris muscle. In both cases of process and action, transition between forms is caused by the feature's function(s).

1.1.3. Function

Every function within a feature can be subdivided into one or more forms. According to my interpretation of Bock and von Wahlert (1965), a *function* is a subgroup of properties that corresponds to a physical or chemical structure or process causally related to a form (a temporally specific appearance, shape, configuration, or material composition of a part, region, or process) in an organism. There are two kinds of functions: static and dynamic. A *static function* is a physical or chemical *structure* associated with a specific form that confers a mechanical attribute. For example, the arrangement of collagen fibers in shark dermis (form) will cause the dermis to resist expansion in some directions but not others (function). A *dynamic function* is a physical or chemical *process* associated with two or more forms that causes a transition from one form to another. For example, the biceps femoris muscle changes from one form to another due to the biochemical function of the proteins actin and myosin.

Functions and forms are hierarchically related such that the properties of forms partially determine the nature or range of the properties and processes of the functions they underlie. Functions are either the static or dynamic actions that arise from a given form, or they are the static or dynamic actions that cause the transition from one form to another. In other words, functions are the causal properties associated with the forms of a feature. Bock and von Wahlert call the relationship between the forms of a feature and their emergent functions the *form-function complex*. Form-function complexes underlie the faculties of a feature.

1.1.4. Faculty

Every faculty of a feature can be subdivided into one or more form-function complexes. According to Bock and von Wahlert (1965), a *faculty* is a subgroup of properties that corresponds to a role a function or functions (a physical or chemical property or process) causally related to a form or forms (temporally specific appearance, shape, configuration, or material composition of a part, region, or process) plays in the life of an organism. For example, one faculty of the human lungs is to supply the pulmonary blood vessels with oxygenated air so the organism can respire. A faculty will usually arise from multiple functions, which act together to aid the organism in some way. A group of functions may also give rise to more than one faculty, especially if they play a role in multiple environments in which the organism lives.

There are two types of faculties: internal faculties and external faculties. *Internal faculties* play a causal role in the life of an organism independent of the organism's external environment. Internal faculties can be studied in a laboratory setting or in nature. *External faculties* play a causal role in the life of the organism dependent on the organism's external environment. External faculties can only be studied in nature. Internal and external faculties should not be confused with the features of an organism, or else features will be equivalent to the role they play in the life of an organism. Features would then be teleologically defined as the purpose they serve for the organism. In order to avoid conflating a feature's faculties with the feature itself, I follow (with some alteration) an attempt by Ernst Nagel (1961, p. 403) to give a non-teleological explanation of what he called "function" in defining faculties as follows:

Internal faculties are defined explicitly as follows: Every organism O with faculty F engages in process P ; if O lacks F , then O cannot engage in P , and if O does not engage in P , then O will die. The role F plays in O engaging in P is O 's internal faculty(s), f_0, \dots, f_n .

External faculties can be defined explicitly as follows: Every organism O with faculty F and in environment E engages in process P ; if O lacks F in E , then O cannot engage in P , and if O does

not engage in P while in E , then O will die. The role F plays in O engaging in P while in E is O 's external faculty(s), f_0, \dots, f_n .

It would be a mistake to substitute the notion of a faculty with the notion of a feature in the above definitions. A feature does not play a direct role in the life of an organism, rather the faculties of a feature do. With this distinction we can see how it is possible for a feature to be made the focus of a morphological study without being required to play a role in the life of an organism. In other words, some features may be nothing more than “spandrels” in the sense of Gould and Lewontin (1979).

Morphologists adopt conceptual schemes such as the one described above to help guide their studies of organismal phenomena because conceptual schemes provide a framework within which morphological studies can be performed. On Bock and von Wahlert's conceptual scheme, the identification of a feature constrains the range of properties that can be used to establish the feature's form(s), function(s), and faculty(s) for study, which focuses the morphologist's attention on the most relevant aspects for answering her initial question. Because the principle method employed in studying organismal phenomena is scientific modeling, the conceptual scheme described above also guides morphologists in their construction of morphological models.

1.2. Morphological modeling

Morphologists use models as a method for interpreting their observational data in order to produce informative descriptions and morphological explanations, and they rely on their conceptualizations of organisms to guide the model building process. In the context of the conceptual scheme given by Bock and von Wahlert (1965) and described above, the ultimate goal of morphological modeling is to produce explanations about organismal features and the properties that constitute them through the use of models that describe their forms, functions, and faculties.

Because the subgroups of properties that define the form, functions, and faculties of a feature are hierarchically related, models deployed to explain them will also be hierarchically related.

Hence, an interdependent system of models that together describe and explain the feature will be established. Morphologists build these models from observational data about the forms, functions, and faculties of features using a combination of background knowledge, biomechanical methods, biomechanical principles, and morphological theories. These basic ingredients for modeling are briefly described below.

1.2.1. Observational data

Morphologists collect observational data using a variety of techniques and instruments. Most often, observations are made using unmediated eyes and visible light microscopy, and they are documented using photography. Observations are also collected using scanning electron microscopy (SEM) and transmission electron microscopy (TEM). Computed tomographic (CT) imaging has become more widely used for the production of data in the last twenty years due to its non-invasiveness and ability to document internal functions in living organisms.

Morphologists prepare organisms for observation using techniques such as dissection, sagittal and transverse sectioning, staining, histological techniques, and MRI and CT machines.⁴ Each technique produces a different class of observational data, which are about the same phenomenon, but perspectively different. That is, observational data can be collected about the same thing by using different observational techniques. The observational data produced with these techniques are the foundational material from which morphological models are built.

⁴ Gans (1974) describes investigative techniques such as these as just finer ways of dissecting.

1.2.2. Background knowledge

I take background knowledge to be the relevant facts possessed by or readily available to a morphologist about a feature and its forms, functions, and faculties. Background knowledge is acquired through the rigorous training associated with acquiring graduate degrees, reading available scientific publications, and research experience. In other words, background knowledge is the knowledge-*that* or the knowledge-*how* morphologists possess prior to a morphological study, and it can be found both in their minds and in the reference books located on the shelves in their laboratory.

1.2.3. Biomechanics and biomechanical methods

I take biomechanics to be the study of living organisms in the context of the physical and mechanical constraints they must obey. Biomechanical methods apply the principles of engineering to organisms to better understand their functions, actions, and processes. According to Y.C. Fung, “biomechanics aims to explain the mechanics of life and living,” and “the method of biomechanics is the method of engineering, which consists of observation, experimentation, theorization, validation, and application” (Fung, 1990, p. vii).

1.2.4. Biomechanical principles

I take biomechanical principles to be general rules or principles about the application of biomechanical methods to organisms. For example, the claim that the cross-sectional area is directly related to the contractile force in vertebrate muscle and the claim that nerves have a quantifiable degree of elasticity are examples of basic biomechanical principles that are applicable to morphological studies. Homberger describes the importance of biomechanical principles for modeling as follows:

The interactions and functioning of the various structures constituting a biomechanical apparatus are governed by biomechanical principles or rules that are derived from the shape and physical properties of the structures. These biomechanical principles must be clearly stated before one can build a mechanical model based on the anatomical structures of a biomechanical apparatus, much in the same way as engineers must know how cogwheels, pulleys, transmission bars, and pistons function and interact before they can assemble a machine. (Homerberger, 1986, p. 91)

1.2.5. Morphological theories

I take morphological theories to be general and abstract claims, that is, specific to neither a time nor organism, about the nature of morphological phenomena. The hydraulic “principle” proposed by Wolfgang Gutmann is an example of a morphological theory. It states that organisms are “systems composed of fluid contained within flexible membranes.... Every form and architectural arrangement has to be conceived as the result of the form-enforcing influence of mechanical elements that operate within an integrated mechanically coherent system” (Gutmann, 1988, p. 257). Morphological theories are distinct from biomechanical principles in that the former make general claims about organisms and the latter make general claims about their mechanical properties.

The conceptual scheme described by Bock and von Wahlert (or whatever conceptual scheme a morphologist adopts) guides the collection of observational data and the application of background knowledge, biomechanical methods, biomechanical principles, and morphological theories to the construction of morphological models. Once a feature is established as the focus of a morphological study, the source of potential observational data becomes restricted to the domain covered by the identified feature. Likewise, the use of background knowledge, biomechanical methods, biomechanical principles, and morphological theories also becomes restricted to the scope of the established feature.

Not only does the conceptual scheme provide constraints on how models can be built, but it also serves to guide the model building process. According to Bock and von Wahlert, faculties

cannot be identified and studied until form-function complexes have been established. But form-function complexes cannot be established until the forms and functions of which they are comprised have been identified and studied. But functions can only be identified and studied with reference to at least one underlying form. Therefore, the conceptual scheme identified by Bock and von Wahlert requires that a feature's forms be identified and studied first. Hence, once a morphologist has identified a feature, the first subgroups of properties she should model are those that underlie the feature's forms. Then, the descriptions produced by models of forms can be used to build models of functions, whose descriptions can in turn be used to build models of faculties. The descriptive totality of these models can then provide an integrated description of the feature they are contained within.

Each of these models share a common set of parameters that are “set” or chosen by a morphologist during the model building process, and each of these models are assessed by one or more morphologically specific tests. The parameterization of a model sets boundaries on the scope of a model's descriptions and explanations such that only a limited amount of observational data can be accounted for by a single model. That is, any given model will be unable to account for all the available observational data. However, morphologists use a variety of tests to ensure that a model adequately and accurately accounts for the observational data it describes as judged by the standards of the model builder. The parameters and tests for models are discussed below.

1.2.6. Model parameters

I identify three parameters of morphological models: a model focus, a model type, and a model expression. I call the target phenomenon that a model describes and explains the *model focus*. Forms, functions, and faculties are potential foci of morphological models. What I shall call the

model type refers to the model's kind, which can be conceptual, visual, or mathematical; conceptual models produce non-empirical descriptions and explanations, such as thought experiments, visual models produce empirical descriptions and explanations, such as diagrams and pictures, and mathematical models⁵ produce mathematical descriptions and explanations.⁶ The third parameter is what Daniela Bailer-Jones (2005) calls the *model expression*, which is the model's medium of description and explanation. Diagrams, graphs, propositions, and computational programs are examples of media that can be used to express descriptions and explanations. *Each model is characterized by only one focus, one type, and one expression.*

1.2.7. Model tests

Morphological models are tested in the ways that are appropriate for assessing their descriptions and explanations of their foci. Homberger (1988, 2001) discusses various tests morphologists use to assess their models. With slight modification, I have distilled from those studies the following tests: the descriptive test, the anatomical test, the theoretical test, laboratory experiments, and natural experiments.

- The *descriptive test* compares a model's descriptions to the observational data acquired about the model's focus.

⁵ These are not mathematical models as they are used in mathematics or logic. Rather, they are models of organismal phenomena produced through the use of mathematical formulations.

⁶ In his *Introduction to Dynamic Morphology*, Edmund Mayer (1963) identifies two additional types of models, which he describes as analogies and substitutes. According to Mayer, *analogy models* are comparisons between familiar systems such as glass blowing and unfamiliar systems such the production of the complicated gelatinous house of the tunicate *Oikopleura* (this analogy comes from Hardy (1956)). I follow Daniela Bailer-Jones (2002a, 2009) by taking models as descriptions or explanations and by taking analogies as relationships. On her view, analogies are useful for building models and models can act as analogues, but models are not analogies themselves. According to Mayer, *substitution models* are models that describe an accessible phenomenon in order to understand the properties of an inaccessible phenomenon. For example, to study the binding properties of different histological (tissue) stains, a morphologist may want to apply the stains to an inanimate material (Singer and Morrison (1948) refer to the inanimate material as 'model tissue') such as a solid fibrin film or a gelatinous material to study how the stains might react to tissues. What Mayer calls substitution models appear to be clear instances of what Bailer-Jones (2002a) describes as models being used *metaphorically*. That is, Mayer's substitution models are metaphorical models on the view of Bailer-Jones. Metaphorical models transfer descriptive information from one domain (say model tissues) to another, structurally similar domain (say the tissues themselves). However, this is not necessarily a particular type of model, since any type of model can be used metaphorically. I decided not to include the two additional Mayer identifies for these reasons.

- The *anatomical test* compares a model's anatomical descriptions and explanations to the anatomical background knowledge about the model's focus.
- The *theoretical test* compares a model's descriptions and explanations to the biomechanical principles and morphological theories relevant to the model's focus.
- *Laboratory experiments* compare a model's descriptions and explanations to the descriptions and explanations of a duplicated model that differs from the original model in at least one variable to assess the original model's ability to account for the model's focus.
- *Natural experiments*, according to Homberger, assess the ability of a model's descriptions and explanations to "permit the incorporation of naturally occurring individual variations of the structural elements while remaining 'operational'" (Homberger, 1988, p. 227).

These tests are used to assess the model during and after its construction. During the model building process, these tests help morphologists detect the underdeveloped aspects of a model and the descriptions and explanations that need the most attention. These tests also help ensure the coherent assembly of a model's components. After a model is constructed, that is, after the descriptions and explanations are satisfactorily finalized and able to produce hypotheses, these tests are used to assess the accuracy and adequacy of a model's descriptions and explanations of its focus.

As described earlier, morphologists begin their studies of features by modeling a feature's forms. This is followed by modeling a feature's functions, which is then followed by modeling a feature's faculties. Important aspects of form-, function-, and faculty-models are discussed below.

1.2.8. Form-models

Forms of a feature are described and explained by what I shall call *form-models*. Form-models synthesize observational data with background knowledge and biomechanical principles using biomechanical methods and morphological theories to produce descriptions and explanations about a feature's forms. Each form-model has a single form as its focus, which it describes and explains with a single model type and through a single model expression. Because a single model will be unable to incorporate all of the available observational data about a form in its descriptions and explanations, a morphologist will deploy multiple form-models of differing types and expressions to adequately describe and explain a given form. The empirical and theoretical accuracy and adequacy of each form-model is ensured by subjecting them to the descriptive test, the anatomical test, and the theoretical test. Failure of an individual form-model to pass either of these tests will signal to the morphologist that she needs to either add or subtract observational data or refine the model's descriptions and explanations (or both).

Morphologists will often deploy multiple form-models to adequately describe and explain the observational data acquired about a single form. Although each of these models will describe and explain a subset of the total available data, they will be related by sharing a common focus. Although any given model in the group will be unable to account for the entire set of observational data, the descriptions and explanations it produces from its subset of data must be coherent with the descriptions and explanations produced by other models within the group. That is, descriptions and explanations produced about the same focus should not contradict one another.

As in the case of individual model assessment for accuracy and adequacy, groups of form-models are also subjected to the descriptive test, the anatomical test, and theoretical test. The failure of a group of form-models to pass either of these tests will indicate to the morphologist that one or more models in the group must be revised. Reapplication of these tests to the indi-

vidual models can aid the morphologist in detecting the model(s) in need of revision. Once at least one form-model has been constructed, the morphologist can proceed with the construction of function-models.

1.2.9. Function-models

Functions of a feature are described and explained by what I shall call *function-models*. Function-models synthesize observational data with background knowledge and biomechanical principles, as well as the descriptions and explanations produced by the form-models, using biomechanical methods and morphological theories to produce descriptions and explanations about a feature's functions. A function-model will incorporate only those descriptions and explanations produced by form-models that are relevant to the forms underlying the function to be modeled. Without the descriptions and explanations of underlying forms, a function-model will be grossly incomplete in that it will lack precisely the information it needs to describe how a transition occurs between two or more forms.

Because there are two modes of function, static and dynamic, a model describing and explaining a given function will either be a static function-model or a dynamic function-model. There is no qualitative difference between static and dynamic function-models other than their use of appropriate model types and media to best describe and explain their static or dynamic foci. Whenever I discuss function-models, it will be in the sense of both static and dynamic function-models, unless otherwise specified.

Like form-models, a single function-model, especially one focused on a dynamic function, will be unable to account for the total available observational data about a function. Therefore, as in the case of form-models, groups of function-models will be deployed to fully describe and explain the available observational data. Hence, the same three tests (the descriptive test, the

anatomical test, and the theoretical test) a morphologist uses to assess individual form-models and groups of form-models are also used to assess individual function-models and groups of function-models for their descriptive and explanatory adequacy and accuracy.

Also like form-models, function-models that describe and explain a single function are related in that they share the same set of observational data. Along with the three tests previously mentioned, the coherence of function-models is also assessed using laboratory tests. Laboratory tests are used because function-models lend themselves to experimental assessment given the mechanical nature of their foci. From the descriptions and explanations produced by function-models, predictions about mechanical actions (both static and dynamic) can be derived, which can be converted into an experimental design. The results of laboratory tests will indicate whether a group of function-models is incoherent and whether one or more models should be revised by the morphologist.

The descriptions and explanations produced by form-models and their corresponding function-models when taken together produce a form-function complex. Comparison between a form-function complex and its biological importance for the organism allows a morphologist to describe and explain its role for the organism through the construction of a faculty-model.

1.2.10. Faculty-models

Faculties of a feature are described and explained by what I shall call *faculty-models*. Faculty-models synthesize observational data with background knowledge and biomechanical principles, as well as the descriptions and explanations produced by both form- and function-models, using biomechanical methods and morphological theories to produce descriptions and explanations about a feature's faculties. As in the case of function-models, the construction of faculty-models

requires the inclusion of the descriptions and explanations of the form- and function-models that compose the form-function complex that the faculty-model is to describe and explain.

Similar to the dichotomy of function-models that describe and explain static and dynamic functions, two variations of faculty-models are used for the description and explanation of internal and external faculties. There is no fundamental difference in the basic structure of internal and external faculty-models other than the respective content they describe and explain. Therefore, the discussion of faculty-models that follows will apply to both internal and external faculty-models unless otherwise specified.

Faculty-models share many of the properties that I described above with form- and function-models. As with form- and function-models, a single faculty-model will be unable to account for the total available observational data about any given faculty. Therefore, multiple faculty-models will be deployed to describe and explain a single faculty using different model types and expressions. Similarly, the group of faculty-models will be related in that they will produce descriptions and explanations of a common set of observational data. Like function-models, the coherence between the models in a group of faculty-models can be assessed using the descriptive test, the anatomical test, the theoretical test, and laboratory experiments, which can be applied to the whole group or individual models. Failure of either the group or an individual model to pass these tests will alert the morphologist that she needs to make refinements to one or more of the models.

Faculty-models can also be subjected to a fifth type of test, natural experiments. Faculty-models describe and explain the faculties of a feature, which are the roles played by form-function complexes in the life of an organism. However, the roles played by the form-function complexes in the life of organisms will vary in extent and significance from one organism to

another. A natural experiment is a comparison between a faculty-model (or group of faculty-models) and the natural variation in the faculty (being described and explained by the model or group of models) across a range of individuals.

Natural experiments assess how well faculty-models are able to account for individual variation. Each time a faculty-model successfully passes the test of a natural experiment, its scope of prediction expands, that is, the faculty-model can be said to account for a larger amount of organismal variation. If a faculty-model fails a natural experiment, then it will be unable to account for the level of variation used in the test. This may be due to a faculty-model's limitations or to an anomalous variant in the natural experiment. Because the descriptions and explanations of faculty-models are in part based on the inclusion of the descriptions and explanations of form- and function-models, natural experiments serve to assess how well form- and function-models are able to account for individual variation as well. As a result, a morphologist is faced with the difficulty of determining which models in the hierarchy need to be revised in order for the higher-level faculty models to successfully pass natural experiment tests. This total assessment is integral to ensuring the accuracy and adequacy of the feature-model.

1.2.11. Feature-models

A feature is described and explained by the totality of the form-, function-, and faculty-models. I shall call the summation of these models the *feature-model*. The feature-model synthesizes the totality of descriptions and explanations of its form-, function-, and faculty-models with background knowledge and biomechanical principles using biomechanical methods and morphological theories to produce descriptions and explanations about an organism's feature. Given the relationships described above between form-, function-, and faculty-models, feature-models are

characterized as interdependent systems, such that alterations in one of its underlying models could potentially affect other models as well as the whole.

Feature-models are amalgamations of their underlying form-, function-, and faculty-models, but they are also distinct models such that discarding one or more of the form-, function-, or faculty-models that underlie it will not necessitate discarding the feature-model itself. However, feature-models are dependent on their underlying models in that the content of a feature-model's descriptions and explanations is predominantly derived from the descriptions and explanations produced by its underlying models. A feature-model lacking underlying form-, function-, and faculty-models is empirically impoverished and unable to produce novel descriptions and explanations.

The accuracy and adequacy of descriptions and explanations produced by feature-models are a measure of how well the feature-model is able to account for the available observational data about a feature within a given morphological scope. However, feature-models are only tested indirectly through the assessment of their underlying models. Because feature-models are amalgamations of their underlying models, the predictions derived from feature-models will always be made in reference to some aspect of the feature, such as its forms, functions, and faculties. Therefore, any tests of the feature-model must be performed with reference to an individual form-, function-, or faculty-model, or to a related group of form-, function-, and faculty-models. Only the models underlying a feature-model can be directly tested. However, refinements made to the underlying models as a result of testing serve to indirectly refine the descriptions and explanations produced by the feature-model. Once its underlying models have been refined, the descriptions and explanations of the feature-model can be reassessed for how well they are able

to account for the available observational data. The underlying sub-models of the feature-models can then be further refined. And so on.

1.3. Modeling the human bronchial tree

To illustrate how morphologists conceptualize and model organismal phenomena in practice, I present a brief analysis of the morphological modeling of the human bronchial tree as a representation of an organismal feature. Following the structure of §1.2., I first discuss some relevant background knowledge, biomechanical principles, biomechanical methods, and morphological theories needed for modeling the bronchial tree. I then describe how the forms, functions, and faculties of the bronchial tree are modeled and how they ultimately contribute to the formation of a feature-model. Like nearly all anatomical structures, the human bronchial tree is too complex to give a full rendering in just a few pages. Hence, I present only the basic elements needed to appreciate the process of morphological modeling.

1.3.1. Background knowledge about the human bronchial tree⁷

The *human bronchial tree* facilitates the transport of air from the upper respiratory tract, which includes the pharynx (mouth cavity) and larynx (voice box), down to the respiratory tissues in the lungs. The “trunk” of the bronchial tree is called the trachea, and it lies superior⁸ to the

⁷ Background knowledge is acquired through years of education, training, and research, which cannot be adequately summarized in a few paragraphs. I have instead used *Gray's Anatomy* (2008) and a collection of journal publications as the source for the background knowledge presented here, because they represent an authoritative summary and/or review of the current knowledge we have about the human bronchial tree.

⁸ Morphologists use specific terms to indicate location and directionality relative to the specimen under study. A standing human with arms hanging on the sides of the body represents the standard anatomical position. *Superior* means “toward the head”. The superior side of an anatomical structure is the side closest to the head, and likewise a blood vessel running superiorly means the blood flow is toward the head. The opposite of superior is *inferior*. *Lateral* means “toward the external surface”, and *medial* means “away from the external surface”. The lateral sides of the lungs are the sides closest to the ribs, and the medial sides of the lungs are the sides closest to the heart. *Distal* means “away from a point of origin”, and *proximal* means “toward a point of origin”. The elbow is more proximal to the shoulder than the distal phalanges (fingers).

“branches”, that is, the bronchial “tree” resembles an upside down tree. During inspiration, air enters the larynx from the pharynx before entering the trachea as it makes its way to the lungs.

The trachea runs inferiorly for approximately 10 cm before bifurcating just above the level of the heart into the left and right principle (primary) bronchi. The left principle bronchus enters the left lung without branching further, but the right principle bronchus bifurcates again to give off the superior lobar (secondary) bronchus before entering the right lung as the right intermediate bronchus. Within each lung, the bronchi bifurcate multiple times to form ten structurally separate and functionally independent respiratory regions within each lung called *bronchopulmonary segments*.

The right lung is divided into three lobes: a superior, middle, and inferior lobe. The superior lobe is supplied by the superior lobar bronchus (secondary), which branches into three segmental bronchi (tertiary) to supply the three bronchopulmonary segments (I, II, and III) that compose the superior lobe. The middle lobe is supplied by a branch from the intermediate bronchus called the middle lobar bronchus (secondary), which branches into two segmental bronchi (tertiary) to supply the two bronchopulmonary segments (IV and V) that compose the middle lobe. After giving off the middle lobar bronchus, the remnant of the intermediate bronchus becomes the inferior lobar bronchus (secondary), which supplies the inferior lobe of the right lung. Within the inferior lobe, the inferior lobar bronchus branches into five segmental bronchi (tertiary) to supply the bronchopulmonary segments (VI, VII, VIII, IX, and X) that compose the inferior lobe.

The left lung is divided into only two lobes: a superior and inferior lobe. After entering the lung, the left principle bronchus (primary) bifurcates into the superior and inferior lobar bronchi (secondary) to supply the superior and inferior lobes, respectively. Within the superior lobe, the

superior lobar bronchus branches into five segmental bronchi (tertiary) to supply the bronchopulmonary segments (I, II, III, IV, and V) that compose the superior lobe. Likewise, the inferior lobar bronchus branches within the inferior lobe to give the five segmental bronchi (tertiary) that supply the bronchopulmonary segments (VI, VII, VIII, IX, and X) that compose the inferior lobe.

Within each bronchopulmonary segment, the bronchi continue to bifurcate until their diameter decreases to reach approximately 1 mm. At this point, the bronchi are referred to as *bronchioles*, which are the last non-respiratory passages of the bronchial tree before reaching the alveoli and alveolar sacs. The last bronchiole, the terminal bronchiole, carries air into the *respiratory bronchioles* and the alveolar sacs. Alveoli are distributed along the sides of respiratory bronchioles. Alveoli are thin walled respiratory surfaces that facilitate gas exchange between the air inside the lung and the surrounding pulmonary blood vessels. Respiratory bronchioles open into chambers of alveoli called alveolar sacs. There are on average 300 million alveoli within the lungs of an adult human, which provide approximately 143 m^2 of respiratory surface area.

There are three widely used classification schemes for tracking the bifurcations of the bronchi distal to the segmental bronchi. The first and most intuitive scheme classifies the branches by *generations*. The first generation usually begins at the trachea and continues to the bronchioles, with each child branch from the trachea on being one generation higher per bifurcation. For example, the trachea, left principle bronchus, left superior lobar bronchus, and left segmental bronchus (I) would be classified as generations 1, 2, 3, and 4, respectively. The human bronchial tree has a maximum of 23 generations, but generations can be assigned starting from any part of the tree.

The other two classification schemes are types of ordering schemes. *Horsfield ordering* classifies the most distal bronchiole as *order 1*, and, moving proximally to the trachea, each parent branch is assigned one order higher than the child branch of the highest order. For example, the parent branch of a child branch with *order 1* and a child branch with *order 5* will be assigned *order 6*. In contrast to Horsfield ordering, *Strahler ordering* assigns the parent branch one order higher only if both child branches have the same order. Otherwise, the parent branch in Strahler ordering is assigned the same order as the highest ordered child branch. For example, a parent branch with two child branches of *order 3* will be assigned *order 4*, but a parent branch with a child branch of *order 3* and a child branch of *order 2* will be assigned *order 3*. Unlike the use of generations, ordering must begin at the terminal bronchioles, and, therefore, full knowledge of the bronchial tree's branching pattern is required before it can be ordered.

1.3.2. Biomechanical principles relevant to the human bronchial tree

There are many biomechanical principles about vertebrate lungs that a morphologist can utilize in her study of the human airways. Karel F. Liem provides two important principles about vertebrate lungs. He states that a vertebrate “lung must be built as an elastic bag which must be able to move freely so as to allow expansion and contraction of all its parts. Elasticity is important so that its gas content can be ventilated” (Liem, 1988, p. 742). These two principles can be stated more concisely as 1) vertebrate lungs must be able to change in volume, and 2) vertebrate lungs must be continuously ventilated. Other principles can be derived from the two given by Liem. For example, the second principle necessitates a route by which air can enter and exit the lungs. By combining the first principle with the ideal gas law ($PV = nRT$)⁹, the changes in lung volume will be due to a change in pressure, since the principle of homeostasis rules out changes in tem-

⁹ In the ideal gas law, $PV=nRT$, P stands for the gas pressure, V stands for the gas volume, n stands for the amount of moles of gas, R stands for the gas constant, and T stands for temperature. Interestingly, the ideal gas law is itself a mathematical model.

perature. As a token of vertebrate lungs, human lungs must adhere to these principles, so a study of the airways in the lower respiratory tract must take into account these biomechanical principles.

1.3.3. Biomechanical methods relevant to the human bronchial tree

Fluid mechanics is a standard biomechanical method for studying the airways in the lower respiratory tract of human lungs because airflow through the airways is governed by the laws, principles, and mechanical relationships of fluid of mechanics.

1.3.4. Morphological theories relevant to the human bronchial tree

There are two main theories about the process of ventilation in vertebrate lungs: the buccal pulse pump theory and the aspiration pump theory. The *buccal pulse pump theory*¹⁰ says that air is taken in through the mouth of an organism to fill a buccal chamber, from which the air is pumped *via* muscular action into the lungs. This theory explains how air-breathing fish and amphibians respire. According to the *aspiration pump theory*, air is not pumped into the lungs. Rather the volume of the lungs is changed directly due to their placement within the pump itself. This theory explains how many reptiles, birds, and mammals respire.

In mammals, air is drawn into the lungs by increasing the volume of the pleural cavity, which decreases the internal pressure. According to theory, contraction of the diaphragmatic muscles and costal muscles increases the volume of the pleural cavity and provides space for the lungs to expand. The increased volume causes a decrease in internal pressure, which draws external air into the lungs. Once expanded, the elasticity of the lungs and the relaxation of the diaphragmatic muscles decrease the volume within the pleural cavity and the lungs, which increases the internal

¹⁰ Brainerd and Owerkowicz (2006) discuss a variation of the buccal pump theory, which explains gular pumping in tetrapods. It's status as a distinct theory, the gular pump theory, is undetermined.

pressure, causing the air to be pushed out of the lungs. The airways in the lower respiratory tract facilitate the transport of air in and out of the lungs in accordance with this mechanism. Therefore, a study of the airways must be able to integrate with other studies about the features of this mechanism.

Modeling the human bronchial tree allows morphologists to address questions about the flow rate, pressure, and distribution of air as it passes down the trachea and into the bronchi and bronchioles for respiration. Answers to these questions can provide important insights into respiratory physiology, the fluid dynamics of breathing, and how inhaled particles are deposited. The studies discussed below are focused on answering the following question: *What is the transport pattern of air as it flows throughout the human bronchial tree?*

1.3.5. Form-models of the human bronchial tree

Schmidt et al. (2004) described and explained the human bronchial tree using 18 visual and mathematical form-models. Observational data for the models were collected from an individual human lung that was excised from an adult male cadaver. Through the use of visual models expressed through physical and 3D-virtual mediums and mathematical models expressed as graphs, Schmidt et al. (2004) investigated the branching pattern and diameter diversity of bronchi in the lung's bronchial tree.

First, they built two physical models using a liquid rubber solution. The first physical model was made by injecting the rubber into the lumen of the entire bronchial tree. I refer to this model as the *rubber tree model*. A second physical model was built by injecting the rubber solution into the pulmonary blood vessels of the left lung. I refer to this model as the *rubber vessel model*. Once the rubber hardened, the surrounding lung tissue was digested away, leaving only the rubber injected into the bronchial tree and pulmonary blood vessel models. Both models repre-

sent the three dimensional configuration of the bronchial tree and the major left pulmonary vessels. Because these models were made using the actual specimen, they were used as reference models for construction of the 3D-virtual and mathematical models.

In order to build the 3D-virtual and mathematical models, Schmidt et al. (2004) scanned the rubber models using high-resolution computed tomography (HRCT). HRCT digitally “slices” a specimen to produce image slices, which can be “stacked”, as shown in Figure 1, to virtually reconstruct the three dimensional spatial configuration using computational programs. Because raw HRCT data are acquired as x-ray absorption values in a 3D array, algorithms can be used to mathematically analyze the data. The 3D-virtual and mathematical models were built using computational programs and sophisticated algorithms.

First, a basic 3D-virtual model was built from the HRCT data using a threshold based segmentation algorithm. I refer to this model as the *bronchi segmentation model*. The bronchi segmentation model is a 3D-virtual representation of the volume in the bronchial tree. This means only the voxels¹¹ pertaining to the absorption values of the rubber models are rendered. Therefore, the bronchi segmentation model was compared to the rubber models for coherence and so that imperfections on the rubber models could be removed from the bronchi segmentation model. Schmidt et al. (2004) also used the rubber models as guides for distinguishing between the bronchi and the pulmonary vessels rendered in the bronchi segmentation model. The *order* at which bronchi could be distinguished from the pulmonary vessels in left lung was set as the rendering threshold. That is, all branching below this *order* was eliminated from the right lung in the bronchi segmentation model.

¹¹ Voxels can be thought of as three dimensional or cubic pixels.

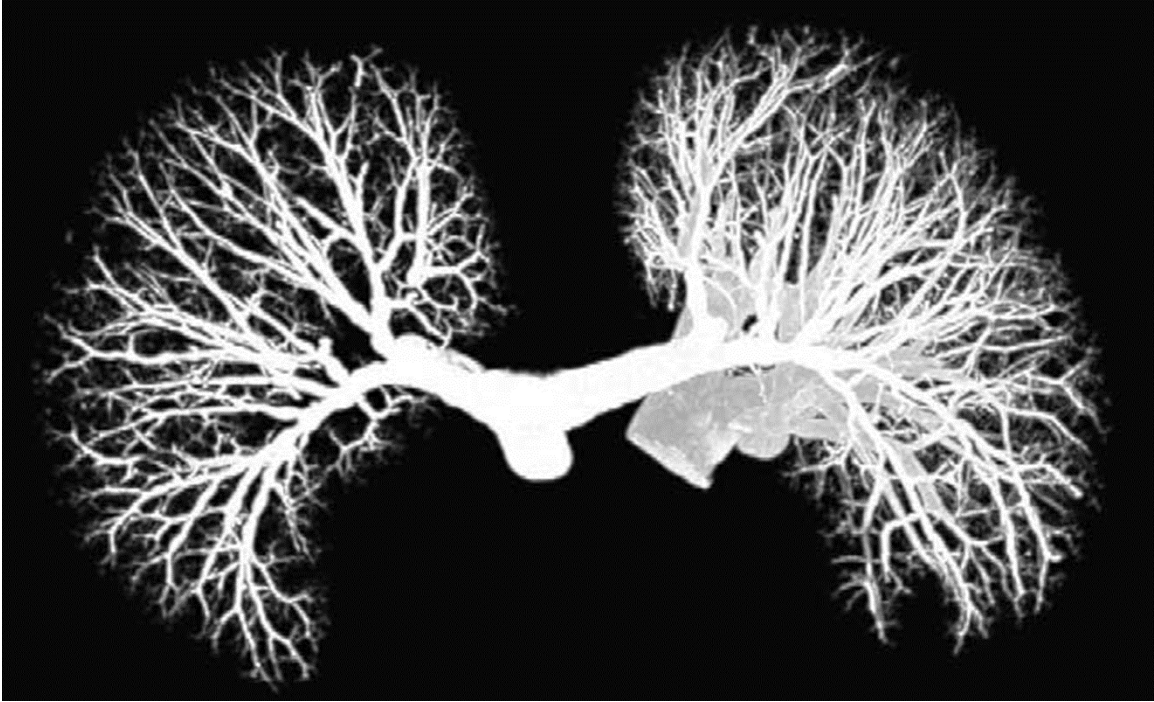


Figure 1: Max-intensity projection of a 6 cm horizontal slab of the HRCT data displayed with inverted grey scale. Taken from Schmidt et al. (2004, p. 204).

The bronchi segmentation model was used to build the second 3D-virtual model, which I refer to as the *bronchi surface model*. A marching cube algorithm was applied to the bronchi segmentation model, which removed the voxels inside the rendered bronchi to leave only a smooth surface representing the external edge of each bronchial tube as shown in Figure 2. A surface rendering can be further processed to acquire quantifiable data about the specimen, so the representation produced by the bronchi surface model was used to build the mathematical model.

The mathematical model was built by skeletonizing the bronchi surface model. Skeletonization is an image processing module that erodes each surface down to a center line, which produces numerical data that can be further processed into a mathematical model. I refer to the mathematical model as the *bronchi skeleton model*. A visual representation of the mathematical model can be made using an abstracted graph, which describes the bronchial tree topology. I refer to this visual rendering of the mathematical model as the *bronchi topology model*.

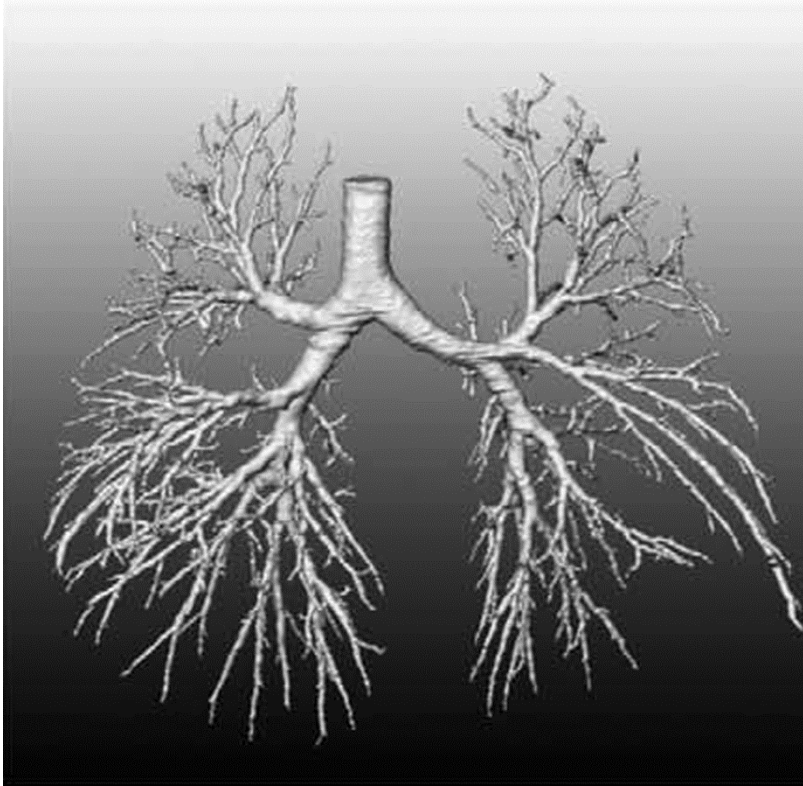


Figure 2: Bronchi surface model extracted from the bronchi segmentation model. Taken from Schmidt et al. (2004, p. 204).

In turn, the bronchi topology model was then used to produce a third 3D-virtual model that I refer to as the *geometric surface bronchi model*. The geometric surface bronchi model represents the geometrical surface of the bronchial tree, shown in Figure 3, which can be used to evaluate the volume and cross-sectional area of the bronchi, as well as to perform statistical analyses of the bronchi branching patterns.

The fourth and final major 3D-virtual model built by Schmidt et al. (2004) represents the spatial configuration of the lungs. The original HRCT data was “closed” using a morphological¹² module to produce a surface rendering of the lungs. Zones where bronchial structures are least dense were identified as boundaries of the lobes, and they were isolated using watershed transformation. I refer to this model as the *lung model*. The lung model, along with the bronchi

¹² Here, morphological refers to an image processing module, not biological morphology.

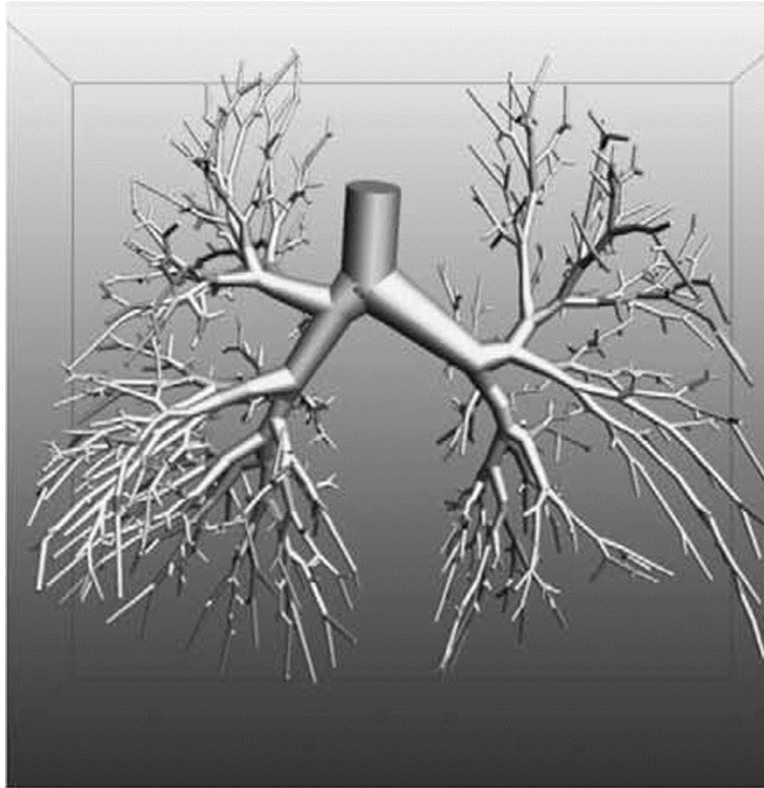


Figure 3: Geometric surface bronchi model produced from the bronchi topology model. Taken from Schmidt et al. (2004, p. 204).

segmentation model, is shown in Figure 4. By combining the lung model with the geometric surface bronchi model, the drainage volume of each lobe can be calculated.

From their study of the human bronchial tree form, Schmidt et al. (2004) concluded that the branching pattern in the bronchial tree is highly asymmetric, meaning the child branches differ greatly in their cross-sectional area. Bronchi with diameters between 2 mm and 3.5 mm showed especial asymmetry in their branching, although asymmetric bifurcation is found throughout the *orders*. Their results are important for the construction of function-models. According to Schmidt et al. (2004), “any modeling and functional simulation of organs critically relies on the precision and representativeness of the structural models available” (Schmidt et al., 2004, p. 209).

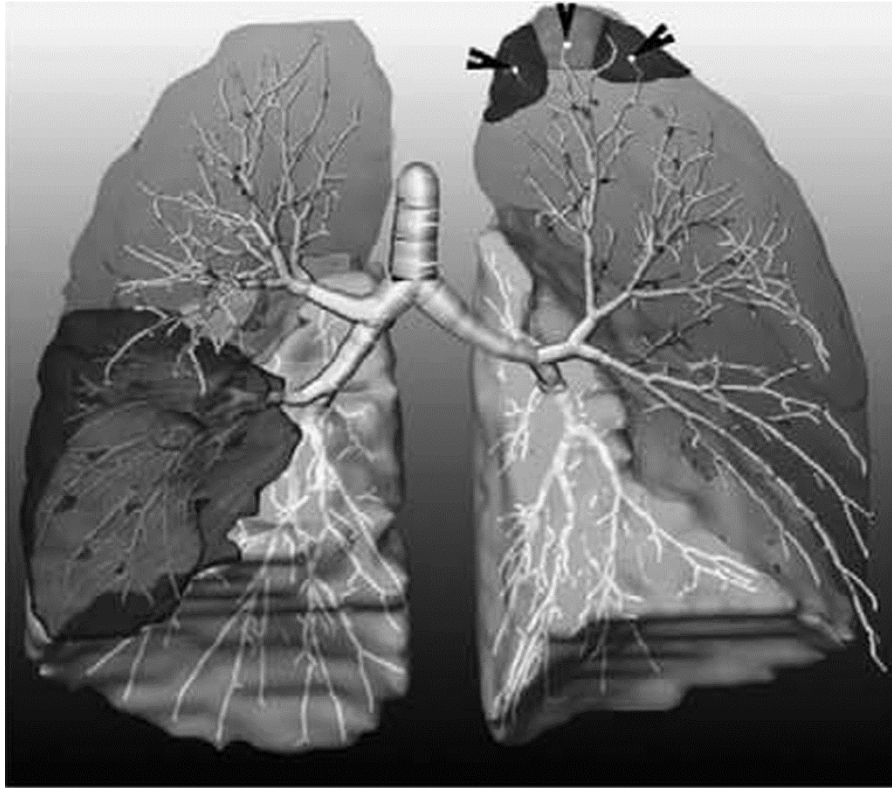


Figure 4: Lung model with bronchi segmentation model. Taken from Schmidt et al. (2004, p. 204).

1.3.6. Function-models of the human bronchial tree

Gemci, Ponyavin, Chen, Chen, and Collins (2008) built computational function-models using the form-models built by Schmidt et al. (2004). Gemci et al. (2008) used computational simulations to represent the changes in pressure and flow rate as air enters the trachea and travels down to the leaves of the bronchial tree. The computational fluid dynamic algorithms were applied directly to the geometric surface bronchi model and the underlying bronchi topology model built by Schmidt et al. (2004).

To assess changes in pressure, the bronchi topology model was combined with computational software called GAMBIT® to generate a mesh of computational cells, which were parameterized for measuring the decrease in pressure from the tracheal opening to the lowest *order* bronchioles. I refer to this model as the *bronchi pressure model*, which is shown in Figure 5. The bronchi

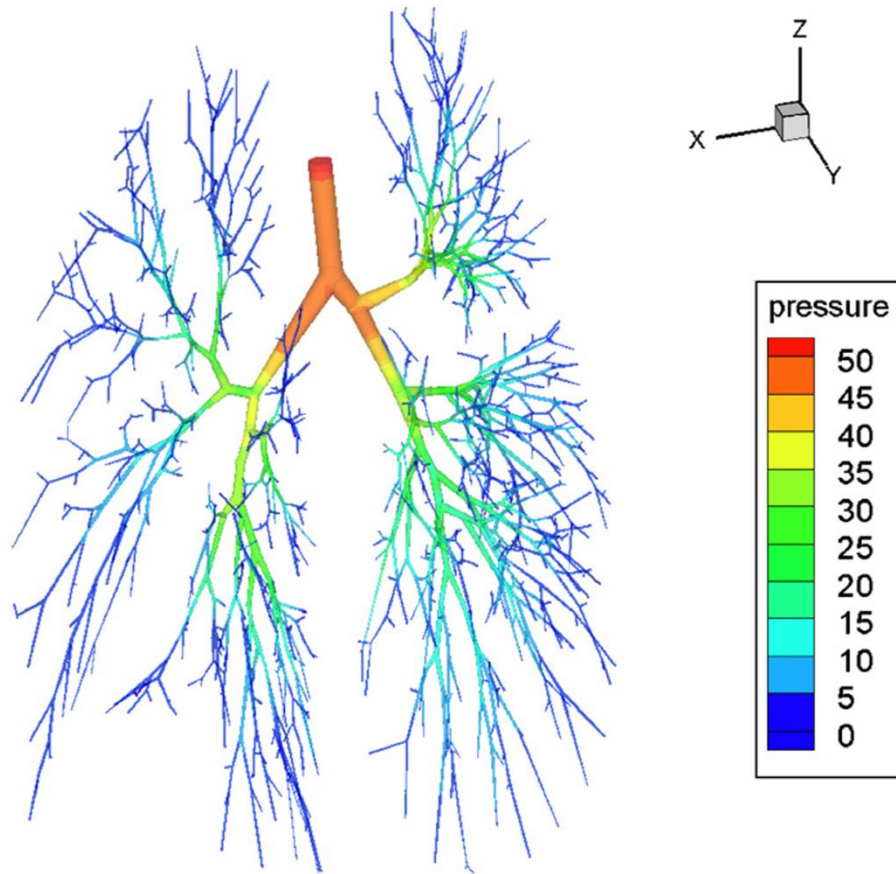


Figure 5: Pressure distributions across the 17-generation bronchi pressure model for a volumetric flow rate of 30 L per min. Taken from Gemci, Ponyavin, Chen, Chen, and Collins (2008, p. 2050).

topology model was also used to perform a large eddy simulation (LES) to assess the presence of inter-bronchial turbulent airflow.

The computational fluid dynamic analysis revealed that at a volumetric gas flow rate of 28.3 L/min into the trachea, the internal bronchial pressure dropped 50 Pa upon reaching the distal bronchioles. This large pressure drop is due, according to Gemci et al. (2008), to the downstream increase in total cross-sectional area of bronchi, as described in the form-models built by Schmidt et al. (2004). In their study, Gemci et al. (2008) also observed asymmetric pressure drops, especially when comparing the pressure profile in the right and left lungs. This too is most likely due to the asymmetric branching pattern indicated by the form-models of Schmidt et

al. (2004), indicating the close dependence of the results from the computational analysis on the results from the configuration analysis. The implications of combining the form-models with the function-models serve to underwrite potential faculty-models.

1.3.7. Faculty-models of the human bronchial tree

There are many possible faculties of the bronchial tree. Based on the results of the form- and function-models described above, Nakayama, Kuwahara, and Sano (2009) propose a faculty-model, which they call a simple one-dimensional mass transfer model, for the bronchial tree. I refer to their model as the *mass transfer model*. According to the mass transfer model, the branching pattern and pressure drop described by the models above results in “minimum overall mass transfer resistance for the mass transport from the external air to the red blood cells” (Nakayama et al., 2009, p. 351). In other words, the form-function complex of the bronchial tree maximizes the transfer of oxygen from the alveolar sacs to the blood and, conversely, the transfer of carbon dioxide from the blood to the alveolar sacs. The form and function of the bronchial tree maximizes the respiratory capacity of humans.

1.3.8. A feature-model of the human bronchial tree

The form-, function-, and faculty-models described above represent a fraction of the published studies on the airflow within the human bronchial tree. Furthermore, these studies say nothing about the blood supply, the innervation, and the histological structure of the bronchial tree. A feature-model of the bronchial tree would require the inclusion of the form-, function-, and faculty-models about these areas relevant to the bronchial tree. Nevertheless, the brief analysis above indicates the dependence of higher-level-models, such as a faculty-model, on the results of lower-level-models, because any change in the form-models described above would directly alter the

function- and faculty-models that incorporated their representational results. Likewise, the chain of airway models described above could be equally relevant for form-, function-, and faculty-models about the bronchial vessels and the tissues lining the inside of the bronchi that they supply. This multiple perspectival dependence characterizing morphological modeling necessitates a philosophical analysis of this methodology, which I present in the following chapter.

2. A PHILOSOPHICAL ANALYSIS OF SCIENTIFIC MODELING

Scientific models appear to be, contrary to past research traditions, as central in scientific practice for describing and communicating aspects of the empirical world as metaphors are in ordinary language. (Bailer-Jones, 2002a, p. 124)

In the previous chapter, I described how models are deployed in morphology and how they aid morphologists in their studies of organismal features. In this chapter, I focus on the philosophical aspects of morphological modeling by addressing the role models play in the *interpretation* of morphological data and in the *representation* of organismal features to produce descriptions and explanations about them. My philosophical analysis largely follows the work on scientific models done by Daniela Bailer-Jones (1999, 2000, 2002a, 2002b, 2003, 2005, 2009; Bailer-Jones & Bailer-Jones, 2002). Although her work has primarily focused on the use of models in physics, I think her general views on scientific modeling can adequately account for morphological models.

There are many philosophically interesting facets of morphological modeling, but I shall focus on four main themes: the relationship between morphological data about an organismal feature and the organismal feature itself (§2.1.); the relationships between morphological data, morphological models, and organismal features (§2.2.); the relationships between organismal features, morphological models, and morphological theories (§2.3.); and multiperspectival morphological models (§2.4.).

2.1. Morphological data and organismal features

When a morphologist sets out to model an organismal feature, one of her first and primary tasks in the modeling process is to acquire morphological data about the feature being modeled. Acquiring data *about* a feature implies that morphological data and organismal features are distinct

from one another. If we want to understand the role morphological models play in morphological investigations, we must first understand the relationship between the morphological data upon which the model is based and the organismal feature that the model is intended to represent.

In order to understand the distinction between morphological data and organismal features, much can be learned from a more general distinction between scientific data and natural phenomena. James Bogen and James Woodward (1988, 1992, 2005) (see also Bogen (2011) and Woodward (1989, 2000)) argue that we should distinguish the *phenomena* scientists study from the *data* scientists acquire about phenomena. They emphasize this distinction because of important differences they identify between data and phenomena. According to Bogen *phenomena* “are processes, causal factors, effects, facts, regularities, and other pieces of ontological furniture to be found in nature and in the laboratory.... Some phenomena are observable, but many are not” (Bogen, 2011, p. 8), and according to Woodward, phenomena “are relatively stable and general features of the world which are potential objects of explanation and prediction by general theory” (Woodward, 1989, p. 393). Ian Hacking further elaborates by claiming that the word ‘phenomenon’ “has a fairly definite sense in the common writings of scientists. A phenomenon is *noteworthy*. A phenomenon is *discernible*. A phenomenon is commonly an event or process of a certain type that occurs regularly under definite circumstances” (Hacking, 1983, p. 221).

Morphologists are interested in modeling and explaining organismal features. Based on Bogen and Woodward’s notion of phenomenon, we can take organismal features as being kinds of phenomena, specifically kinds of *organismal phenomena*. Organismal phenomena can be directly observed (processes, effects, and facts) or indirectly detected (causal factors and regularities). Direct observations are made by our unmediated (although not necessarily unaided when

instruments such as microscopes are used) senses, whereas indirect observations are made by mediated senses (x-ray tomography). Therefore, organismal phenomena are discernible by morphologists and are often the subject of systematic investigation. That is, morphologists can acquire data about organismal phenomena using various means of observation. If we return to the distinction made by Bogen and Woodward between phenomena and data, we can begin to see that data about phenomena are distinct from phenomena themselves. When a morphologist acquires data about a particular organismal phenomenon, she is not collecting parts or aspects of the phenomenon itself. Rather, the data she is acquiring are something distinct from, yet nevertheless related to the phenomenon of interest. So, what are data?

According to Bogen and Woodward, *data* are records of “effects produced by elaborate causal processes that may involve the operation of the human perceptual and cognitive systems as well as measuring and recording devices and many other sorts of natural and manufactured non-human systems” (Bogen & Woodward, 2005, p. 234). According to Woodward (2000), these effects result from a systematic causal interaction between an observer and a phenomenon, which may be facilitated by experimentation, photography, and other measuring or recording devices. Woodward describes this interactive process as *data production*. So, while morphologists are not collecting actual parts or aspects of organismal phenomena, they are recording or reporting the detectable effects the parts and aspects of organismal phenomena produce as a result of our causal interaction with them.

According to Bogen and Woodward (1988, 1992), there are many causal factors involved in data production, such as the experimental setup, the measuring or recording devices used, and, most importantly, the beliefs of the observer. The observer’s beliefs play an important role in how data are produced, because the observer’s beliefs determine the methods used to produce

data. The choices regarding the experimental setup, such as determining the controls and variables, the observational medium, such as microscopes and CT scanners, the observational perspective, such as the specimen orientation and lighting, etc., are all dependent upon the beliefs of the observer. Because each of these choices leads to different possible scenarios of data acquisition, the beliefs of an observer play a central role in what kinds of data are produced.

According to Bogen and Woodward the *content* of the data an observer produces is not, however, dependent upon the observer's beliefs. Data can be shared and analyzed in the form of public reports or records by a community of scientists without reference to the beliefs of the observer who produced the data. That is, data can be assessed independently of the mental states of the observer who produced them. For example, a morphologist can adjust the focal point or magnification of a microscope while acquiring photographic data about an organismal feature in order to accommodate her beliefs about the best conditions for data acquisition. Her beliefs may differ from her colleague's beliefs about how best to acquire the data, and as a result, she and her colleague will produce different photographs, that is, they will each produce different datasets. However, the content of each photograph will be independent of the beliefs behind their production. That is, once the photograph is produced, the content of the image cannot be materially altered by the beliefs of the scientist viewing it. In summary, while the data production process is heavily theory-laden, the content of the data produced is not.

According to Bogen and Woodward, the production of data about a phenomenon is not equivalent to the observation of a phenomenon. The large number of causal factors involved in data production can potentially (and most likely do) contribute to observational error. Observational error occurs when effects not produced by the phenomenon of interest are recorded as if they were. If data about a phenomenon is equivalent to the phenomenon itself, then the constitu-

tion of the phenomenon will be dependent upon the influences of the causal factors present during observation. Because the beliefs of the observer are also causal factors themselves, conflating data with phenomenon means equating an observer's beliefs about a phenomenon to the phenomenon's constitution. If this is so, then the beliefs a morphologist has about the constitution of a particular organismal feature will directly affect its material nature. But, it seems unlikely (see Chapter 3) that the beliefs a morphologist has about the constitution of her own lung will play a role in dictating its constitution. So, how do scientists determine which reported or recorded effects are caused by the phenomenon of interest and which are not?

According to Woodward, phenomena are *detected* within a data report or record through a process he calls *data interpretation*. He describes data interpretation as

the use of arguments, analytic techniques, and patterns of reasoning which operate on the data so produced to reach conclusions about phenomena. Typical components of this stage are techniques of data analysis and reduction, including statistical procedures of various sorts and procedures for smoothing, transforming, and discarding data. Data interpretation may also include the explicit use of background or theoretical assumptions... (Woodward, 2000, p. S165)

In other words, data interpretation is a theory-laden process used to derive claims about phenomena from data reports or records. For example, once a morphologist produces a set of photographs of an organismal feature, she and her colleagues must *interpret* the photographs using their expertise, which may lead to different conclusions about the phenomenon the photographs represent. However, data interpretation does not dictate the content of data reports or records; the content of data reports or records is equally independent from the beliefs involved in data interpretation as it is from the beliefs involved in data production. Rather, data interpretation determines which subsets of data within data reports or records shall count as *evidence* for the detection of a phenomenon. In other words, evidence for a phenomenon will be whatever data a morphologist determines by theoretical judgment to be caused or produced by the phenomenon itself. For example, not all of the pixels composing a photograph of an organismal feature will

be useful to a morphologist; only certain regions of the photograph will provide evidence for the feature, and these regions must be identified by the theoretical judgment of the morphologist. That is, data interpretation is a process in which only those reported or recorded effects that are produced by the phenomenon of interest are isolated and used to make claims about the phenomenon. According to Bogen, data's "epistemic usefulness depends on features whose possession reflects the causal influence that phenomena or closely related causal factors exert on their production" (Bogen, 2011, p. 8).

An important consequence of Bogen and Woodward's view is that some data may be epistemically useless. Some data thought to be about a phenomenon may actually not be about it, because not all of the data produced are necessarily produced by the phenomenon of interest. Remember, many causal factors play a role in data production, including the beliefs of the observer. According to Bogen, scientists "apply background knowledge and inferential techniques" (Bogen, 2011, p. 8) to datasets in order to determine which parts are relevant and which are not. This has an even more interesting consequence: when scientists say they have "observed" a phenomenon, what they mean is that they have, according to Bogen and Woodward, *detected* an evidential pattern or signal within the dataset they have collected.¹³

Morphologists acquire morphological data about organismal phenomena, and they interpret the data they produce in order to cull epistemically useless data and to acquire an accurate description of their feature of interest. For example, after acquiring data from high-resolution computer tomography (HRCT) imaging of a human lung, Schmidt et al. (2004) interpreted their large datasets by applying a threshold-based algorithm to isolate only those data they judged to

¹³ McAllister (1997) argues against the claim by Bogen and Woodward that phenomena can be detected as patterns in a dataset, since there are a multitude of possible patterns that can be "detected" depending on theoretical assumptions of the interpreter. Bailer-Jones (2009), therefore, suggests that we identify phenomena as things, processes, etc. that have the potential to be theoretically explained. It follows from her view that phenomena can be identified prior to the production of data, although we won't be able to say much about them until the data is interpreted.

be epistemically useful. As I illustrated in Chapter 1, morphologists often rely to a large degree on the aid of morphological models for the interpretation of their data. As I describe in the next section, morphological models play a key role in the interpretation of data about organismal features, and the representations models produce as a result of their role in data interpretation serve as a key source of information and explanation about those features.

2.2. Morphological data, models, and organismal features

The intended result of the process of data interpretation is the production of new and informative (and testable) claims about phenomena of interest that can be used with scientific theories to produce explanations and predictions. Morphologists use a variety of methods to interpret their data, including statistics, image processing modules, and data thresholding.¹⁴ However, due to the complexity of organismal phenomena and the likely consequence of acquiring large datasets, morphologists often deploy models to aid in the data interpretation process. Modeling is a helpful methodology because, according to Bailer-Jones, models “only address *aspects* of the phenomenon and only take into consideration selected pieces of information about the phenomenon” (Bailer-Jones, 2000, p. 51). That is, models help morphologists focus on limited subsets of data at a time without having to account for the entire dataset. By using models, datasets can be broken down into manageable sizes, which can be analyzed in greater detail than would be possible by analyzing the dataset as a whole.

If we follow the distinction Bogen and Woodward make between data and phenomena, then it should be apparent that the process of modeling phenomena from data will not be straightforward. Bailer-Jones identifies three types of models that scientists, including morphologists, may deploy during the data interpretation process: a data production model, a data model, and a theo-

¹⁴ Data thresholding works when each datum within a dataset is assigned a numerical value and a threshold is set such that all data above or below the threshold are eliminated.

retical scientific model. On my view, a *data production model* is an interpretative description of the procedures, both experimental and non-experimental, used for the production of data.¹⁵ A data production model may be as simple as a diagrammatic model of a microscope used to aid observation or as intricate as a model of the steps involved in the methods for the statistical analysis of phylogenetic trees such as maximum likelihood or bootstrapping. However, data production models are not models of the data produced. According to Bailer-Jones, data production models are models “about the conception of the experiment and not about the actual empirical results that the experiment may produce” (Bailer-Jones, 2009, p. 171). Nevertheless, data production models lead to claims about experimental or observational setups, and these claims can be assessed based on their ability to produce reliable data, that is, data that serve as evidence for phenomena.

According to Bailer-Jones and Bailer-Jones (2002) and Bailer-Jones (2009), *data models* are interpretative descriptions about datasets. They serve “to put raw data in canonical form” (Bailer-Jones, 2009, p. 171) by correcting measurement errors or detecting trends or patterns. Data models can be as simple as graphs, which link formally disparate data points together in a relationship, such as a quadratic, linear, or polynomial relationship, or they can be as complicated as multilevel flow charts that describe various algorithms of error analysis. According to Bailer-Jones (2009), data models identify relevant facts about datasets, but they do not identify facts about phenomena. In other words, data models help scientists eliminate irrelevant data or noise from datasets, but they do not lead to claims about the phenomenon that played a role in producing the data. According to Bailer-Jones and Bailer-Jones, data models

¹⁵ What I call data production models, Bailer-Jones refers to as *models of experiments*. I felt that the name she used was too restrictive based on her analysis, since data can be acquired using both literal experiments with precisely defined controls and variables, and less precisely defined empirical investigations that may involve nothing more than a microscope. I intend for my use of the terms ‘data production’ to cover the use of both strictly experimental and non-experimental methods of data production.

are *application-neutral*. They are *specific to a type of data analysis problem, but not to the nature of the specific empirical problem*.... From which empirical phenomenon the data derive, or if they even derive from an empirical phenomenon, is secondary, just as long as the problem representation conforms to some very general requirements. (Bailer-Jones & Bailer-Jones, 2002, p. 14, emphasis in original)

Bailer-Jones and Bailer-Jones (2002) and Bailer-Jones (2009) refer to models that lead to claims about phenomena as *theoretical scientific models*.¹⁶ According to Bailer-Jones, a theoretical scientific model

is an interpretative description of a phenomenon that facilitates access to that phenomenon.... This access can be perceptual as well as intellectual.¹⁷ If access is not perceptual, it is often facilitated by visualization, although this need not be the case. Interpretative descriptions may rely, for instance, on idealizations or simplifications or on analogies to interpretative descriptions of other phenomena. Facilitating access usually involves focusing on specific aspects of a phenomenon, sometimes deliberately disregarding others. As a result, models tend to be partial descriptions only. (Bailer-Jones, 2009, pp. 1-2) Scientific models are singularly about empirical phenomena (objects and processes), whether these are how metals bend and break or how man has evolved. (Bailer-Jones, 2002a, p. 109)

In contrast to data models, theoretical scientific models (theoretical models hereafter) are specific to particular phenomena, and they are *theoretical* because they are developed using scientific theories and concepts that apply to the phenomena of interest (Bailer-Jones & Bailer-Jones, 2002, p. 13). Theoretical models lead to new information or facts about phenomena that can be described and explained using higher level theories. These models lead to facts about phenomena by identifying theoretical links between the claims identified by data models. That is, theoretical models serve as a bridge between empirical data and theory about phenomena (Bailer-Jones,

¹⁶ The notion of theoretical models Bailer-Jones subscribes to should not be confused with the notion of theoretical models typically associated with the semantic view of scientific theories (as understood by Giere (1988); Suppe (1972, 1977, 2000); Suppes (1960); Van Fraassen (1980)). Rather, Bailer-Jones' notion of theoretical model is similar to what Demetris Portides describes as *phenomenological models*, which "are constructed by the deployment of semi-empirical results, often by the use of ad hoc hypotheses, or by the use of a conceptual apparatus that is not directly related to fundamental theory and not always straightforwardly compatible with the theoretical calculus" (Portides, 2011, p. 335).

¹⁷ Theoretical scientific models may be more interpretative or more descriptive depending on the particular problems they are deployed to help address. Ankeny (2000) argues that descriptive models address "what" forms of questions instead of "how" or "why" forms of questions that are addressed by more interpretative or explanatory models. He argues that descriptive models are precursors to more interpretative or explanatory models. Chin (2011) argues that we should further distinguish between interpretative models and explanatory models. He claims that explanatory models are concerned with explaining how some parts of a phenomenon fit together or behave, whereas interpretative models bring new meaning or new conceptualizations to a phenomenon's behavior or construction.

2009). The relationship between theoretical models, data models, and phenomena is described by Bailer-Jones as follows:

The theoretical model plays an important role in applying theory to phenomena because it provides the link to the data model of data derived from the phenomenon. The theory is applied to the phenomenon only via the theoretical model, and the theory is only every confirmed via the theoretical model that links it to the data model. So theories are linked to empirical findings indirectly: via the theoretical *and* via the data model (and whatever else lies in between). Finally, the way a phenomenon is delineated is closely tied to the theoretical model about the phenomenon, which in turn is shaped by theoretical assumptions that go into its formulation. (Bailer-Jones, 2009, pp. 173-174)

Theoretical models are a source of claims about phenomena through their role in *representing* phenomena.¹⁸ Representation is a complex and controversial philosophical notion, which I do not have space to fully explore here. Instead, I follow the account of representation and its relationship to models as given by Bailer-Jones. At base, representations of phenomena are descriptive and interpretative depictions of phenomena. According to Bailer-Jones,

[m]odels are, in the widest sense, a description of the phenomena they are about. ‘Description’ is here intended as a term wide enough to admit various different forms of external representations, propositional and nonpropositional—for example, texts, diagrams, plots of empirical data, objects, or mathematical equations. These ‘external representations’ are tools in terms of which the model is expressed. These tools are chosen such that the description is not purely phenomenal, but one that aims at an interpretation of the phenomenon in question. (Bailer-Jones, 2009, pp. 185-186)

In other words, the interpretative descriptions of models are expressed through various media, such as diagrams, clay figures, or propositions, and these expressions represent the phenomenon or some aspect of the phenomenon that the model is about.

Bailer-Jones identifies three factors that play an important role in representation: function, selection, and the model user. According to Bailer-Jones, each model is deployed to fulfill a particular *function*. She identifies two levels at which models fulfill their functions. At the first level, models can fulfill different *types* of functions such as explanation (Achinstein, 1968;

¹⁸ Some philosophers, such as Murad (2011), identify representation as a position within the philosophical view known as scientific realism. However, De Regt (2005) argues that antirealists can adopt a view of representation, which only includes reference to observables. Therefore, I do not think my discussion of representation at this point necessarily commits my philosophical analysis of models to a realist view prior to my discussion of realism in Chapter 3.

Harré, 1960; Hesse, 1966), technical application (Morrison, 1998, p. 73), guiding experimentation (Hesse, 1953, p. 199; Hutten, 1954, p. 289), promoting creative insight (Harré, 1970; Hesse, 1966), and imagination (Harré, 1970, pp. 34-35; Hutten, 1954, p. 285).¹⁹ For example, a plastic model of the human lungs will fulfill its function of helping a young medical student imagine the spatial configuration of the lungs within her cadaver to aid her dissection, but it will not fulfill the function of explaining respiratory biomechanics to the student. Rather, an explanation of respiratory biomechanics requires a different model with a different representation. That is, the representation a model produces will depend on the type of function it must fulfill.

According to Bailer-Jones, the second level where models fulfill their functions occurs when models fulfill different functions *within* the same type of function. This occurs when multiple models are deployed to represent different aspects of the same phenomenon. These multiple *sub-models* are deployed to represent a subset of data claims produced by data models instead of all the claims about the entire dataset. For example, form-, function-, and faculty-models are sub-models deployed to represent particular aspects of an organismal feature, and neither of these models represents the organismal feature in its entirety.

The second factor Bailer-Jones identifies in representation is *selection*. Because phenomena, especially organismal phenomena, exhibit a high degree of complexity, scientists must be selective about which aspects they focus on and model. This factor, as discussed above, is a necessary consequence of models having multiple functions within a single type of function. In order to explain a complex phenomenon, it must be “deconstructed” into isolatable aspects that can be independently modeled by the deployment of sub-models. By localizing models to specific aspects, scientists are better able to manage the complexity of the entire phenomenon. For ex-

¹⁹ These are the references Bailer-Jones (2003, p. 70) cites to support her claims.

ample, form-models are deployed by morphologists to represent only the forms of an organismal feature, function-models are deployed to represent only the functions, and faculty-models are deployed to represent only the faculties. In other words, models will represent what they are *selected* to represent.

The selective deployment of sub-models depends on what Bailer-Jones identifies as the third factor in representation, *the model user*. Models are selectively deployed to fulfill chosen functions. The selection and function of a model are dependent upon the *intentions* of model users, and determining the function of a model and the deployment of sub-models requires a certain amount of *creativity* (Bailer-Jones, 1999). The creativity of the model user is especially important for bringing the sub-models and their representations together into a *unified model* of a phenomenon. Bailer-Jones describes the process of amalgamating sub-models into one unified model as *embedding*. Embedding is a process by which a model user eliminates or minimizes inconsistencies between sub-models and produces a unified representation of the entire phenomenon (or at least the sum total of the aspects represented by the sub-models). In morphology, for example, the form-, function, and faculty-models are embedded by a morphologist within a feature-model to produce a representation of the entire feature.

These three factors play an important role in determining the epistemic worth of a model's representation and the claims derivable from it. Bailer-Jones claims that model representations, which are usually expressed in non-propositional forms (visual), can sometimes *entail* propositions. According to Bailer-Jones,

the propositions thus entailed by a model state what the model is taken to state about the phenomenon modelled. The propositions *are not* the model, but the model can be transcribed into propositions that express its 'message'. Some propositions are (trivially) entailed by the model because they may be necessary to constitute that particular model, e.g. certain assumptions made in the model. Propositions are less trivially entailed by a model if, for instance, a geometrical configuration may be a substantial element of the model. Such a configuration can then be captured and described by propositions, but is not itself propositional. (For example, one may propositionally state the angle

which the oxygen and hydrogen atoms form in a water molecule; the angle may otherwise be expressed by a rods and balls model.) So, propositions can be entailed by a model whether the model is expressed by propositional or by non-propositional means. Thus I do not limit myself to models being either propositional or non-propositional. In fact, partial propositionality may be not all that uncommon in models. (Bailer-Jones, 2003, p. 60)

Because propositions can be true or false, the entailment of propositions by models can provide some indication as to the truth or falsity of models. As Bailer-Jones points out, the assessment of the truth and falsity of entailed propositions is an ominous possibility for models, since models are notorious for entailing false propositions due to the abstractions, idealizations, and simplifications of their representations as compared to the phenomena they represent. Yet, models seem to play an important role in the inductive and explanatory successes of our scientific practices no matter how abstract (Love, 2008; Morrison, 1999), idealized (Greaves, 1995; Love, 2010; McMullin, 1985), or false (Wimsatt, 1987) they may be. The entailment of false propositions combined with the important role their representations play in scientific research precludes a straight up or down judgment as to the truth and falsity of models. They are neither absolutely true nor absolutely false.

We can, however, assess the partial truth or falsity of models by closely examining *which* entailed propositions are true and which are false. Bailer-Jones (2009) identifies three possible reasons why a model's representation might entail false propositions: representational inaccuracy, inconsistency, and incompleteness. According to Bailer-Jones, models may *inaccurately* represent phenomena because their interpretative descriptions are based on approximations and simplifications. Approximations and simplifications result from a process Bailer-Jones calls *idealization*, "whereby existing properties of a phenomenon are changed deliberately to make modeling easier" (Bailer-Jones, 2009, p. 148).

Following Ernan McMullin (1985), Bailer-Jones identifies two different ways in which idealization can affect the truth or falsity of the propositions entailed by a model's representa-

tion: construct idealization and causal idealization. According to Bailer-Jones (2003), *construct idealization* involves the simplification of the conceptual representation of the phenomenon, not the phenomenon itself. We enact a form of construct idealization, for example, when we assume the Earth's orbit is circular, rather than slightly elliptical. Construct idealizations are often used in morphology where complexity can be overwhelming. In their studies of air flow within the bronchi of the human lung, Schmidt et al. (2004) modeled the walls of the bronchi as smooth tubes without taking into account the surface imperfections within the bronchi caused by the topography of surrounding cartilaginous rings. This simplification allows for easier air flow calculations, because localized turbulence caused by the imperfections of the bronchial walls, which are not directly related to the overall air flow, are not included in the parameters. These simplifications can be “de-idealized” at a later stage if localized turbulence becomes explanatorily relevant.²⁰

In contrast to construct idealization, *causal idealization*, according to Bailer-Jones, involves the simplification of the problem situation or phenomenon itself. Causal idealization ignores relevant causes or factors that could play a role in the model's representation by focusing on one or two causes. For example, we commit a causal idealization if we assume there are no other planets in our solar system when we study the orbit of the Earth around the Sun. Causal idealizations are quite common in morphological models, and most commonly occur in biomechanical models, which represent organismal structures as levers and pulleys (see for example Greaves (1995)). In other words, causal idealization assumes the phenomenon is something it is not in order to make modeling it easier.²¹ This is in contrast to construct idealization, which acknowledges the complexity of the phenomenon, but selectively idealizes certain aspects not directly

²⁰ Portides (2005) describes “de-idealization” as a process of concretization, since the model is becoming more like the concrete phenomenon it represents.

²¹ In my view, the classic technique of “rounding the cow” is a form of causal idealization.

related to the function of the model. In short, construct idealization leads to simple models, whereas causal idealization leads to models of simple phenomena.

In the case of construct idealizations, a model's representation can entail propositions about the selected aspect or about its idealized parts. For example, a model of the bronchi in the human lungs can entail propositions about the overall air flow through the bronchi, which is the selected aspect, or about localized turbulence due to internal imperfections. The former entailed propositions are more likely to be true than the latter, since the latter refer to intentionally idealized parts. False propositions about the idealized parts will be tolerated so long as the propositions about the selected aspect are approximately true. After all, only the latter propositions are relevant to the model's function.

In the case of causal idealization, a model's representation will be based on an idealized phenomenon. Hence, most of the propositions it entails will be false, regardless of whether they are about the selected aspect or not. While these models may provide an interesting metaphor (Bailer-Jones, 2002a; 2009, pp. 117-121; Black, 1962), act as an analogue (Bailer-Jones, 2002a; 2009, p. 56; Hesse, 1966), or play some other heuristic role, their representations should not be taken as accurate depictions of the aspects they have been selected to represent. Although models produced with causal idealization are more obviously false than those produced with construct idealization, causally idealized models can still play an important role in scientific explanation (see for example Love (2010)).

According to Bailer-Jones, models may *inconsistently* represent phenomena because their interpretative descriptions may contradict accepted principles or known facts. For example, the models deployed by Gemci et al. (2008) representing the airflow in the bronchi of the human lungs did not account for the differences in viscosity between “normal” air and air inside the

lungs. Air inside the lungs is heated and humidified, which changes the viscosity of the air and affects its rate of flow and flow vector. Because the models deployed by Gemci et al. (2008) neglect this information, their representations are inconsistent with known facts. Therefore, entailed propositions about airflow will not be as accurate as they could be by taking these known facts into account.

According to Bailer-Jones, models often represent phenomena *incompletely*. As I discussed above, models are usually deployed to represent a particular aspect of a phenomenon, and, as a consequence, they usually neglect to include a large portion of data about a phenomenon in their representations. For example, form-models are deployed to represent the forms of an organismal feature, but they say nothing about the functions and faculties the feature possesses. Hence, propositions entailed by model representations will only be about a part of a total phenomenon, and so while they tell us something about the phenomenon, they will be strictly false when compared to the phenomenon as a whole.

In summary, representational inaccuracy, inconsistency, and incompleteness are three possible reasons why the representations of models might entail false propositions. However, only the entailed propositions that are deemed relevant will play a role in the assessment or test of a model. That is, propositions not directly relevant to a model's function will have a negligible impact on a model's assessment if they turn out to be false. Because a model's function is determined by model users, model users decide which entailed propositions are important and which can be accepted if false. This means that the important question a model user must ask is not whether a model is strictly true or strictly false, but rather which *part* of a model is accurate or inaccurate. This judgment is especially important since, according to Bailer-Jones, not all of a model's representational content will necessarily entail assessable propositions, because some

information contained within a representation may only be conveyed through non-propositional means.

We can now gain a clearer understanding of how modeling works in morphology. I think the picture is something like the following: Morphologists deploy *data production models* to guide their acquisition of data about organismal phenomena. These models may describe a formal experimental procedure to be carried out in a laboratory setting, or they may describe a plan of action for carrying out field work in the Amazon Rainforest. Once data are produced, morphologists set about analyzing them with the help of *data models*. A data model might provide a detailed mathematical representation of a data set, or it might describe a simple algorithm for chronologically arranging hundreds of pictures acquired during field work in the Amazon. Then, morphologists attempt to link the descriptions given by data models with the phenomenon they intend to study by deploying *theoretical models*. Theoretical models, such as form-, function-, faculty-, and feature-models help morphologists interpret the claims derived from their data models by providing interpretative descriptions of the phenomenon by reference to data model claims using morphological theories. The representations produced by theoretical models are empirically confirmed (disconfirmed) by comparing the propositional and non-propositional content they contain to the claims derived from data models. The results of testing provide an indication to morphologists whether the representations produced by theoretical models should be changed or discarded.

2.3. Organismal features, models, and morphological theories

Morphologists use the interpretations provided by data models as a bridge to relate observational data with theoretical models, which allow data models to connect with organismal phenomena. In other words, morphological models serve as a link between observational data and organismal

phenomena. I now want to address how morphological models also serve to link individual organismal phenomena to morphological theories. This latter link is important, because morphologists study particular organismal phenomena in order to generate explanations about classes of phenomena in the form of morphological theories. Without a link between particular phenomena and theory, it will not be possible to build or test theories from observations of individual phenomena. So, what role do morphological models play in connecting particular organismal phenomena to morphological theories?

Bailer-Jones (2005, 2009) argues that an important difference between models and theories is that models are about *concrete* empirical phenomena, but theories are not.²² Models produce interpretative descriptions of particular or concrete phenomena. In other words, models provide descriptions of the properties phenomena possess. Theories, according to Bailer-Jones, are *abstract* descriptions distilled from particular descriptions about phenomena. Bailer-Jones takes abstraction to be “a process where properties are taken away from a phenomenon, and are not replaced by another property” (Bailer-Jones, 2005, p. 249).²³ We can, therefore, think of theories as general descriptions that cover a wide range of phenomena and are not particular to a specific set of phenomenological properties. That is, theories do not provide *direct* descriptions about phenomena in the way that models do. According to Bailer-Jones, “if at all, theories are about concrete phenomena only in a very derivative sense” (Bailer-Jones, 2005, p. 248). For example, the hydraulic principle proposed by Gutmann (1988) is a morphological theory that describes the functional aspects of morphological forms across a wide range of taxa. The hydraulic principle is as applicable to a lamprey as it is to a jellyfish. That is, the hydraulic principle is not a de-

²² Her distinction between models and theories largely follows the analogy Cartwright and Le Poidevin (1991) make between models and fables, which tell a concrete story, and theories and morals of fables, which those concrete stories instantiate.

²³ Bailer-Jones (2005) distinguishes abstraction from idealization, which she claims involves the *changing* of properties rather than their omission.

scription particular to the properties of lampreys or jellyfish. Rather, the hydraulic principle has been distilled from particular descriptions of lampreys, jellyfish, and a host of other taxa.

Bailer-Jones claims that models and theories are dependent upon one another, such that it would be unlikely for one to exist without the other. She claims that without models, theories would have no way of relating to observational data, because observational data are acquired from particular phenomena and theories apply to wide ranges of phenomena. Models link observational data to theories by providing interpretative descriptions of particular phenomena, which taken together can be distilled down into a theory. Models depend on theories, because, according to Bailer-Jones (2005), the interpretative descriptions characterizing models are produced within a theoretical framework with certain ramifications and boundary conditions. For example, the hydraulic principle would not play a role in models about volcanism, because volcanism is a phenomenon outside the framework of morphology.

In summary, theories only apply to concrete phenomena *via* models, and models are only able to produce interpretative descriptions within the appropriate frameworks indicated by reference to theories. This means that the successful use of theories in deriving explanations and predictions depends in large part on the interpretative descriptions of models. Furthermore, it means that the accuracy of the interpretative descriptions produced by models depends in large part on the guidance they receive in their formulation from relevant theories.²⁴

2.4. Multiperspectival morphological models

As a final component in my philosophical analysis of morphological modeling, I now argue that feature-models are kinds of what I call *multiperspectival models*. Multiperspectival models are types of what Bailer-Jones (1999, 2000, 2009) calls *over-all* models. Over-all models are mod-

²⁴ Due to the theoretical dependence of the descriptions and representations produced by models, I do not think they should be considered strictly autonomous in the sense of Morrison (1999).

els composed of embedded sub-models. Morphological models are types of over-all models because form-, function-, and faculty-models (sub-models) are embedded within feature-models (over-all models). On my view, there are three ways sub-models must be related in order to be multiperspectival: they must stand in a focused-based-relation, a combinatorial-based-relation, and a hierarchical-based-relation with one another. These three relations describe how the representations sub-models produce can influence one another as well as the over-all model itself.

Recall from section 1.2.6. Model Parameters in Chapter 1 that a model's *focus* is the aspect of a phenomenon the model represents, that a model's *type* is a model's kind, which can be conceptual, visual, or mathematical, and that a model's *expression* is its medium of representation, which can be non-propositional, such as diagrams, graphs, or virtual renderings, or it can be propositional. Also recall that form-, function-, and faculty-models are hierarchically related.

2.4.1. Focused-based-relations

On my view, two or more sub-models stand in a *focused-based-relation* if they share a common focus, that is, if they represent the same aspect of a phenomenon. For example, in the study by Schmidt et al. (2004), two form-models, a segmentation model and a geometric model, were deployed to represent the form of the human bronchial tree. Although the representations of sub-models that share this relationship may differ in their medium of expression, they can be compared for informational consistency and adequacy. For example, if the segmentation model represents a smaller number of bronchial bifurcations than the geometric model, then the representations of the models will be inconsistent. Inconsistent representations indicate that one or more sub-models are either inadequately or inaccurately representing their focus, which will alert the morphologist that she needs to collect more data or to revise a sub-model's representation of

data that are currently available. Focus-based-relations are horizontal relations that hold among two or more sub-models on the same level in a hierarchy of sub-models.

2.4.2. Combinatorial-based-relations

On my view, two or more sub-models on the same level in a hierarchy of sub-models stand in a *combinatorial-based-relation* if the information contained in their representations is combined within a higher-level model to produce a higher-level representation. For example, the representations produced by the segmentation and lung form-models built by Schmidt et al. (2004) were combined in the study by Gemci et al. (2008) to build a function-model of airflow within different regions of the lungs. If the information contained within the representations of two or more lower-level sub-models cannot be consistently combined within a higher-level sub-model, then one or more of these lower-level sub-models, as in the focus-based-relation, are either inadequately or inaccurately representing their foci, which will alert the morphologist that she needs to collect more data or to revise a lower-level sub-model's representation. Combinatorial-based-relations, like focus-based-relations, are horizontal relations that hold among two or more sub-models on the same level within a hierarchy of models.

2.4.3. Hierarchical-based-relations

On my view, two or more sub-models on different levels in a hierarchy of sub-models stand in a *hierarchical-based-relation* if the information contained within the representations of the lower-level sub-models is used to produce the representations of higher-level sub-models. For example, the representation of the cross sectional area in the bronchial tree produced by the geometric model in the study by Schmidt et al. (2004) was used by Gemci et al. (2008) to produce the bronchi pressure function-model of airflow within the tree. As a consequence of this

relation, the representational accuracy of higher-level sub-models will be directly dependent on the representational accuracy of the lower-level sub-models it contains. The accuracy of a function-model representing the fluid dynamics in the primary bronchus depends directly on the accuracy of the form-models' representations of the cross sectional area. In contrast to focus- and combinatorial-based-relations, hierarchical-based-relations are vertical relations that hold between two or more sub-models at different levels.

The representations of two or more sub-models will be dependent upon one another if the sub-models share one or more of the relationships described above. As a result of this dependence, two or more sub-models are said to be amalgamated into an *interdependent system*, such that alterations in one sub-model could potentially affect the representation of another. For example, because cross sectional area is directly related to the flow rate of a fluid, any alteration in a form-model representing the cross sectional area of the bronchial tree will directly affect a function-model that uses this area in its representation of the bronchial tree's fluid dynamics. In a large system composed of more than two sub-models, changes in one sub-model can potentially affect multiple other sub-models that may incorporate the representational content of the altered sub-model directly or indirectly *via* intermediary sub-models. If a morphologist alters a single sub-model within an interdependent system of sub-models, then her alterations will likely generate a propagating wave affecting the representational content of related sub-models such that the sub-models more closely related to the altered sub-model will be affected to a greater extent than those more distantly related.

There are two ways in which a morphologist may alter an individual sub-model. A sub-model may be altered either because 1) it produces an inadequate representation or because 2) it produces an inaccurate representation. *Inadequate representations* are due to the lack of suffi-

cient observational data and are rectified by the collection or incorporation of more observational data. However, the inclusion of more data may affect the altered sub-model's representation, which may also affect the representations of other sub-models that are related to the altered sub-model in the ways described above. The sub-models affected by the altered sub-model will have to be resynthesized and their representations will have to be reassessed for changes in adequacy or accuracy. *Inaccurate representations* (or misrepresentations) are due to an incoherent synthesis of the observational data contained within the sub-model with the theoretical principles and background knowledge. Inaccurate representations can be rectified either by the inclusion of more observational data or by a revised synthesis. Regardless of whether a sub-model is altered to rectify an inaccurate or inadequate representation, the representations of related sub-models may be affected and may require similar rectification.

As each datum, each theoretical principle, and each representation is locally refined in individual sub-models by the morphologist, the adequacy and the accuracy of the total representational content of the interdependent system of sub-models will increase. Because the system is composed of form-, function-, and faculty-models that represent the components of a feature, the total representational content produced by the amalgamation of these sub-models constitutes the feature-model. According to the view I am advancing, *multiperspectival modeling*, it is only at the level of the amalgamated over-all model, the feature-model, that we are able to derive important morphological explanations and confirmation for morphological theories.

3. AN ACCOMMODATIONIST VIEW OF SCIENTIFIC REALISM

The philosophical methods here are not conceived of as prior to scientific methods in any sense. (Boyd, 1985a, p. 33)

According to *scientific realism*, we have good reason to believe in the approximate truth of the claims made by our currently accepted scientific theories about observable and unobservable (theoretical) phenomena.²⁵ Richard Boyd (1972, 1973, 1982, 1983, 1985a, 1985c, 1989, 1990a, 1990b, 1991b, 1992, 1993, 1999a, 1999b, 2001a, 2001b, 2010b, 2013) has developed a version of scientific realism known as *accommodationism*. There are four central theses of Boyd's version of realism:

1. 'Theoretical terms' in scientific theories (i.e., non-observational terms) should be thought of as putatively referring expressions; scientific theories should be interpreted 'realistically'.
2. Scientific theories, interpreted realistically, are confirmable *and in fact often confirmed* as approximately true by ordinary scientific evidence interpreted in accordance with ordinary methodological standards.
3. The historical progress of mature sciences is largely a matter of successively more accurate approximations to the truth about both observable and unobservable phenomena. Later theories typically build upon the (observational and theoretical) knowledge embodied in previous theories.
4. The reality which scientific theories describe is largely independent of our thoughts or theoretical commitments. (Boyd, 1983, p. 45, emphasis in original)

These four theses assume a thoroughgoing naturalistic view of natural kinds, reference, causation, and epistemology. Thesis 1 assumes a naturalistic view of reference such that claims made using theoretical terms are not reducible to claims made by replacing the theoretical terms with

²⁵ In the debates between scientific realists and antirealists, the main point of contention is the metaphysical status of the unobservable phenomena our scientific theories postulate. (Because they are postulated by scientific theories, unobservable phenomena are also referred to as theoretical phenomena (see for example Maxwell (1962)). Examples of unobservable phenomena include electrons, causation, natural selection, and force. There are two main foils opposing the realist view of unobservables: Neo-Kantian constructivism and empiricism. According to Neo-Kantian philosophers, such as Kuhn (1996) and Hanson (1965), unobservables are mere constructs or conventions postulated by our scientific theories and do not have a metaphysically real existence. That is, on the constructivist view scientists invent unobservables within a paradigmatic framework. According to empiricists, such as Logical Empiricists (see Ayer (1959)) or Van Fraassen (1980), claims about unobservables are reducible to claims about observables, meaning that unobservables do not exist independently of reference to observables. That is, on the empiricist view whatever our scientific theories say about unobservables can be reduced to claims about observables.

observational terms. For example, claims about the natural selection of peppered moths are not reducible to the observable predatory patterns of the birds that eat them. Thesis 2 assumes a naturalistic epistemology such that both the theoretical and observable claims of our scientific theories are confirmed by observational evidence when that evidence is in their favor. For example, the observed predation rate of birds on peppered moths will confirm both the observational predictions about said predation entailed by our scientific theories as well as the theoretical claims involved in the entailment of those predictions. Thesis 3 assumes both a naturalistic view of reference as well as a naturalistic epistemology such that the contribution theoretical terms made to past theories are often retained in replacement theories. For example, although Lamarck's claims about evolution have been rejected, Darwin retained much of Lamarck's theoretical claims about adaptation when formulating his theory of natural selection. Thesis 4 assumes a naturalistic view of natural kinds such that there are phenomena in the world that exist independently of our claims about them. For example, the acceptance of the current Darwinian paradigm does not necessitate the actual occurrence of Darwinian evolution. That is, saying it is so does not make it so.

Boyd's version of scientific realism is intended to provide an abductive argument for the instrumental reliability of our scientific theories and methods. According to Boyd, our scientific *theories* are said to be instrumentally reliable insofar as they produce approximately true claims about observable phenomena, and our scientific *methods* are said to be instrumentally reliable insofar as their use contributes to the invention and acceptance of instrumentally reliable theories. According to what Boyd calls the *accommodation thesis* (see especially Boyd (1993, 1999a, 1999c, 2001b, 2010b, 2013)), the instrumental reliability of our scientific theories and methods can only be explained by the tendency of the conceptual features of our scientific disci-

plines, such as their theories, concepts, classificatory practices, inferential standards, and standards of experimental design to accommodate to naturally occurring causal phenomena.²⁶

My aim in this chapter is to describe the following important elements of accommodationism underlying the four theses described above and the role each plays in achieving accommodation: correspondence truth (§3.1.); the mind-independence of reality (§3.2.); a non-Humean account of causation (§3.3.); disciplinary matrices, inferential and social architectures, and accommodation demands (§3.4.); homeostatic property cluster natural kinds (§3.5.); a causal notion of reference (§3.6.); the theory-dependence of methods (§3.7.); and approximate truth (§3.8). Note that there are ongoing debates about each of these components, and Boyd's views are no less controversial than the rest. However, due to space constraints I will not be able to fully flesh out Boyd's arguments in favor of his positions or detail the criticisms he gives against alternative positions. Rather, my purpose in this chapter is merely to explicate the main ideas of Boyd's version of scientific realism in order to evaluate their appropriateness for the realistic view of scientific models I argue for in Chapter 4.

3.1. Correspondence truth

Boyd argues that the only way to explain the inductive and explanatory successes of our scientific practices is to adopt a *correspondence theory of truth* (à la Tarski (1935)) rather than a theory of socially constructed truth. According to the correspondence theory of truth, the truth-value of a belief (statement, sentence, proposition, etc.) is determined by whether or not there exists a fact *corresponding* to it. For example, the claim "Snow is white" is true if and only if snow is in fact white. How this correspondence is carried out and how we determine the approx-

²⁶ I use the terms 'causal phenomena' to refer to a class of phenomena, which include (but is not limited to) casual powers, mechanisms, processes, entities, structures, regularities, and relations.

imate truth of the claims made by our scientific theories according to Boyd will be discussed below. Here, it is enough to keep the following claim by Boyd in mind:

We are animals. Like other animals, we have non-linguistic representational capacities—perceptual and memorial ones, for example.... Explaining the representational semantics of these capacities requires positing some sort of *correspondence* between internal representational structures and features of the empirical world. Since our linguistic representational capacities are evolutionarily continuous with the non-linguistic representational capacities of our ancestors, and since our linguistic representations are psychologically integrated with many of our non-linguistic representations, it is to be expected that, at least in the empirical domain, our linguistic representations too should have a correspondence semantics. They do. (Boyd, 2001b, p. 57)

3.2. The mind-independence of reality

Boyd argues that “reality is prior to thought” (Boyd, 1982, p. 613) in the sense that reality is *theory-independent*.²⁷ According to Boyd and mainstream scientific realists (see for example Psillos (1999, 2000); Smart (1963)), reality is theory-independent in the sense that our linguistic conventions, social practices, as well as the claims of our scientific laws, theories, and methods do not *a priori* dictate the truth-value of our metaphysical propositions. Rather, the laws, theories, and methods of our sciences are developed *a posteriori* as a result of the ongoing struggle of our scientific language and practices in their accommodation to independently existing observable and unobservable phenomena. And this struggle is only possible because our scientific conventions and practices themselves are causal phenomena, and they are capable of causally interacting with causal phenomena in the world. On Boyd’s view, humans and their political, social, cultural, and linguistic practices are nothing above ordinary natural phenomena.

According to what Boyd calls the *metaphysical innocence thesis*, human social practices are *metaphysically innocent* such that the adoption of a particular social convention or practice will have no effect on the truth-value of our metaphysical claims (Boyd, 1988, 1990b, 1992, 1999b, 2001b). However, given the ability of our scientific practices to causally interact with causal

²⁷ Neo-Kantian antirealists, such as Kuhn (1996) and Hanson (1965), argue that reality and truth are the products of social constructions. They hold that our claims about the states of affairs in the world determine *a priori* their metaphysical actuality; for Neo-Kantians, the world is structured in a *theory-dependent* way.

phenomena, our scientific practices *do* make a contribution to the determination of the truth-values of our causal *claims* through a process of accommodation. According to Boyd,

Reality is prior to thought not only in that its structure is largely independent of what we believe, but also in that the very machinery of thought (or, at any rate, of the public expression of thought) undergoes continuous accommodation to the structure of a largely independent causal reality.... Reality is prior to thought not only with respect to the correctness of theories and the appropriateness of the language in which they are expressed, but also with respect to the standards by which the rationality of thought is to be assessed. (Boyd, 1982, p. 614)

Boyd calls this version of the metaphysical innocence thesis the *No Non-Causal Contribution Thesis* (2N2C) (Boyd, 1988, 1990b, 1999b, 2001b). According to 2N2C, “the adoption of theories, frameworks, paradigms, projects, intellectual or practical interests, and so forth, makes no non-causal contribution to the causal structure of the world scientists study” (Boyd, 1990b, p. 183).²⁸

3.3. A non-Humean account of causation

Boyd’s accommodationist version of scientific realism relies on a *non-reductive* account of causation. According to Boyd (especially Boyd (1985c)), causation, that is, unobservable causal powers, mechanisms, processes, relations, forces, etc., is a theory-independent phenomenon in the sense that its metaphysical existence is not dependent upon our theoretical claims. Boyd’s view opposes the Humean account of causation, which takes causation as a concept *reducible* to observable patterns of events.²⁹ Against the Humean view, Boyd argues in favor of a non-

²⁸ 2N2C does not, however, guarantee that the causal interaction of our scientific practices with causal phenomena will always *positively* contribute to determining the truth-value of our claims. In fact, Boyd (2001a) argues that the linguistic and methodological practices of evolutionary psychology (also known as sociobiology in the sense of Wilson (1975)) have so far *negatively* contributed to determining the truth-values of their claims.

²⁹ David Hume defined a cause as “an object precedent and contiguous to another, and where all the objects resembling the former are placed in like relations of precedency and contiguity to those objects, that resemble the latter” (Hume, 2005, p. 114). According to the Humean account, causation is not an independently existing phenomenon scientists can discover and explain. For the Humean, causation is nothing more than a conceptual manifestation of the attempts by our scientific practices to explain detectable *patterns* of observable phenomena. Furthermore, the Humean holds that the cognitive content of our causal statements is linked to the cognitive content of our observational statements such that our causal notions can always be reduced to non-causal observational notions that do not invoke reference to “secret powers” or unobservable “inner constitutions” (see Hume (2007) Section IV).

reductive notion of causation.³⁰ According to Boyd, if the Humean notion of causation is correct “it would have to be the case that...causal relations could be discerned in nature without recourse to knowledge of unobservable ‘theoretical entities’ and their causal powers” (Boyd, 1985c, p. 68). However, because the derivation of observational consequences from our background theories depends just as much on the theoretical claims they make about unobservable causal powers as on the observational claims they make about phenomena, Boyd argues that we do have good reason to believe that we cannot (and should not) ignore what our theories say about unobservable causal powers. In fact, the reliability of our scientific practices depends on the theoretical claims our theories make about causation. That is, claims about causation cannot be reduced to observational patterns.

According to Boyd, the theoretical claims our theories make about causal powers cannot be *a priori* established due to the metaphysical restrictions imposed by 2N2C. Rather, the notion of causation scientists employ will be the result of a theoretical inquiry into the causal powers of natural phenomena and not a conceptual analysis about deductive procedures or linguistic conventions. Because our notion of causation plays a reliable role in the inductive and explanatory successes of our scientific theories, then, according to Boyd, it must be accurately reflecting *a posteriori* facts about theory-independent phenomena. That is, a *naturalistic notion of causation* is partially dependent upon independently existing, unobservable causal powers and partially dependent upon the inductive and explanatory successes of our scientific practices. In other words, our notion of causation will depend on the inferential role it plays in the inductive and explanatory successes of our scientific practices in the process of their accommodation to inde-

³⁰ The Humean notion of causality has played an integral part in the philosophy of science, especially for the first half of the 20th century, and has served to underwrite the once widely adhered to deductive-nomological (D-N) account of explanation. Enthusiasm for this form of explanation can still be found in theoretical discussions of morphological explanation (see for example Bock (2000, 2004, 2007, 2010)).

pendently existing phenomena in the world. According to Boyd, “what causation is and what causal interaction amounts to are theoretical questions about natural phenomena” (Boyd, 1985c, p. 79).

3.4. Disciplinary matrices, architectures, and accommodation demands

According to Boyd (1999a, 1999b, 2001b), our scientific practices are performed within contingently defined social and inferential systems known as *disciplinary matrices*. He defines a disciplinary matrix as “a family of inductive and explanatory aims and practices, together with the conceptual resources and vocabularies within which they are implemented” (Boyd, 1999b, p. 57). Disciplinary matrices can be divided into two parts: an inferential architecture and a social architecture. The *inferential* architecture is comprised of the inductive and explanatory aims and practices of a disciplinary matrix; the *social* architecture is comprised of the social, economic, political, and cultural factors of a disciplinary matrix, such as hiring practices, funding formulas, and publication expectations. The social architecture may or may not influence (positively or negatively) the inferential architecture.³¹ Disciplinary matrices do not necessarily correspond to our established academic disciplines and practices.

According to Boyd, each disciplinary matrix has to satisfy a set of *accommodation demands* in order to be considered successful. Accommodation demands consist of the required “fit” between the disciplinary matrix’s inferential architecture and the relevant causal phenomena that would be required in order for the characteristic inductive and explanatory aims of the disciplinary matrix to be achieved. It is a contingent question whether or not a given disciplinary matrix can satisfy all of its accommodation demands, and the answer depends on the presence of the

³¹ Occasionally, the social architecture of a disciplinary matrix can serve to suppress scientific knowledge and progress. According to Boyd, “the lone, underfunded, socially marginalized researcher who makes the relevant inductive inference from background theories to the correct answer to a scientific question, but whose research does not get published in established journals, does not thereby make a contribution to the reliability of scientific practice.” (Boyd, 1999b, p. 62)

relevant causal phenomena. For example, the disciplinary matrices dedicated to the explanation of phlogiston and the luminiferous ether failed because the relevant causal phenomena required for the satisfaction of their accommodation demands, namely phlogiston and luminiferous ether, did not exist. Nevertheless, even when there is no doubt as to the existence of the relevant causal phenomena, satisfying accommodations demands may be quite difficult. Our inability to treat the phenomena collectively referred to as ‘cancer’ is a case in point.

3.5. Homeostatic property cluster natural kinds

Boyd’s version of scientific realism advocates for a natural-kind structure of the world in the sense of Quine (1969). The traditional view of natural kinds has been shaped by the work of philosophers, such as Putnam (1972, 1973) and Kripke (1981), on reference and essence. According to the traditional view of natural kinds, kinds possess metaphysically real essences that are detectable and describable by our scientific practices. Naturalists have emphasized the central role played by natural kinds in the inductive and explanatory successes of our scientific practices. For example, Quine (1969) and Goodman (1983) argue that our ability to judge the plausibility of a scientific theory is only possible with reference to natural kinds, because only those theories that employ the natural kinds *embedded* within current scientific practices will be judged as most plausible. For example, theories in modern chemistry no longer refer to phlogiston because phlogiston is no longer embedded within our chemical practices.

Boyd’s account of natural kinds denies that natural kinds have typological or eternal essences.³² According to Boyd, natural kinds are 1) open textured, 2) historically situated, 3) relation-

³² The traditional view of natural kinds has been criticized by antirealist philosophers of science (see for example Hanson (1965) and Kuhn (1996)), who have argued that kinds are mere social constructions, and by philosophically astute scientists, predominantly biologists (see for example Mayr (1982, 1988)), who have argued against essentialistic or typological thinking about mutable phenomena such as biological species. As a result, the traditional notion of natural kinds has become *stereotypically* defined as 1) completely independent of human practices, and 2) as having eternal, unchanging, ahistorical, and intrinsic necessary and sufficient membership conditions that hold across all possible worlds (Boyd, 1999b). (*A possible world* can be quite similar to our own

ally and historically defined, and thus 4) non-eternal, 5) non-intrinsic social constructions (Boyd, 1999b). Nevertheless, Boyd maintains that natural kinds have real essences and play an integral role in the reliability of our scientific practices. These constructive and naturalistic properties of natural kinds are reconciled in the view developed by Boyd, which takes natural kinds as what he calls *homeostatic property clusters*. Homeostatic property cluster kinds (HPC kinds) are entities (observable and unobservable), processes, or relations comprised of unified families of co-occurring properties.³³ Families of properties are unified either because the presence of some properties favors the presence of others or because underlying homeostatic (in both a metaphorical and literal sense) causal mechanisms sustain their co-occurrence. According to Boyd, HPC kinds “possess *a posteriori* real essences or natural definitions, as opposed to nominal essences” (Boyd, 1999b, p. 54). These real essences are the causal powers, processes, mechanisms, and relations possessed by independently existing causal phenomena.

For example, the natural kind Water possesses various properties, such as a boiling point of 100° C, a density of 1 g/mL and a molecular structure comprised of two hydrogen atoms and an oxygen atom, that have a tendency to co-occur because of the underlying chemical and physical mechanisms (causal powers, mechanisms, processes, relations, etc.).³⁴ These chemical and physical mechanisms maintain a degree of homeostasis such that from day to day or year to year we have little doubt as to the physical properties and behavioral characteristics we can expect of Water.

actual world, such as imagining different climatological conditions to assess the evolutionary responses of current fauna to future climate change, or it can be as different as we can possibly imagine, such as a world where the laws of physics are drastically different from those of this world.) A classic example illustrating these stereotypes is the natural kind Water. According to the stereotypes, the presence of a particular molecular structure, H₂O, is a necessary and sufficient condition for membership in the natural kind Water in all possible worlds. That is, the molecular structure, H₂O, is an intrinsic, essential, and unchanging property such that if something has it, then it is a sample of the natural kind Water. Boyd’s view of natural kinds rejects these stereotypes.

³³ Boyd (1988) attributes his inspiration for this view to the property-cluster conception of definitions of “ordinary language” philosophy.

³⁴ When referring to a natural kind proper, I will use a capitalized word, and when referring to the natural kind term, I will place the term in quotations. For example, we refer to the natural kind Water by using the term ‘water’.

There are two important characteristics of HPC kinds. First, the presence of a given property in a family of properties defining a particular HPC kind and the presence of a particular underlying causal mechanism sustaining that family are a *contingent* matter. The family, F , of properties, P_1, \dots, P_n defining a given HPC kind, as well as the causal powers, mechanisms, and relations that homeostatically sustain the family, *are* the real essences of natural kinds. However, determining which properties have membership within a given family is *not* an *a priori* conceptual matter about necessary and sufficient conditions. Rather, membership in an HPC kind is a theoretical *a posteriori* matter that depends on what Boyd (1999b) calls its *causal role* in contributing to the inductive and explanatory successes of our scientific practices. According to Boyd, the definition of an HPC kind depends upon its role in helping our scientific practices meet their *accommodation demands*.

Second, it follows from this that HPC kinds are individuated historically rather than extensionally. Over time the properties and/or causal powers, mechanisms, and relations vary such that a profile of the properties in a family F at time t_1 will likely differ from a profile of F at t_2 , but F will nevertheless be the same family at both times. For example, the properties used to define *Homo sapiens* today will not include all of the properties used to define *Homo sapiens* as the species existed 30,000 years ago because species change over time. But, *Homo sapiens* remains the same *family* of properties today as it was then even though some of its properties have changed. Hence, the membership conditions are not only contingent upon theoretical judgments, but are apt to vary over time and space for the same family. Boyd argues, for example, that biological species are paradigmatic HPC kinds, because their membership conditions are dependent upon theoretical judgments and the inductive and explanatory successes of biological

classification. Furthermore, their constant evolution and/or migration means the membership conditions for any given species will depend upon the time and place it is studied.³⁵

In summary, according to Boyd's account of natural kinds, they are *always* interest and practice dependent social constructions, and they are *often* defined by open textured (neither necessary nor sufficient properties), historically situated, relationally and historically defined, and thus non-eternal and non-intrinsic properties, but they nevertheless possess real essences which can be studied and explained by our scientific practices (Boyd, 1999b, p. 68). It is the real essences of natural kinds, that is, causal powers, mechanisms, processes, relations, etc., that our scientific practices are constantly attempting to accommodate.

3.6. A causal notion of reference

Boyd argues that the inductive and explanatory successes of our scientific practices, their *epistemic achievements*, can only be explained by a naturalistic theory of reference for natural kind terms that refer to both observable and theoretical phenomena within the inferential architectures of our sciences.³⁶ According to Boyd, "what is to be explained is the ways in which the *accommodation* of classificatory and linguistic practices to causal factors in the world contributes to the reliability of those practices" (Boyd, 2010b, p. 214). In other words, if we want to understand why our sciences are so successful, we have to understand how reference works within the inferential architectures of disciplinary matrices.

According to Boyd, *natural kind terms* are deployed within the discourse central to the inferential architecture of a particular disciplinary matrix to track naturally occurring causal phenomena *via* the tracking of (homeostatically clustering) families of properties. Their role in tracking

³⁵ Splitting and lumping our classifications of species are exercises of denotational refinement in the sense of Field (1973), but more on this later.

³⁶ Henceforth, my use of 'natural kinds' will be in the sense of HPC natural kinds.

causal phenomena plays an *inferential role* within the disciplinary matrix deploying them such that the use of the natural kind terms within the inferential architecture of the disciplinary matrix contributes to the satisfaction of its accommodation demands. For example, the natural kind term ‘trachea’ plays an inferential role in the inferential architecture of respiratory biology such that its use contributes to the epistemic achievements of respiratory biology, and it plays this role because it successfully tracks naturally occurring families of properties. That is, the word ‘trachea’ is associated with a family of properties identifiable by practitioners of the discipline such that use of the word ‘trachea’ has a tendency to pick out the same family of properties over and over again.

The deployment of a natural kind term within a disciplinary matrix to track a naturally occurring family of properties to satisfy the accommodation demands of the disciplinary matrix *establishes* a natural kind. That is, a natural kind is defined by the family of properties tracked by the natural kind term. For example, the natural kind term ‘trachea’ is deployed in respiratory biology to track a naturally occurring family of properties. The natural kind Trachea is *defined* by the family of properties tracked by the deployment of the natural kind term ‘trachea’ within the inferential architecture of respiratory biology. Because natural kinds are defined by naturally occurring families of properties, natural kinds, according to Boyd, play a *causal role* in the accommodation of the inferential architecture of disciplinary matrices to naturally occurring families of properties. For example, the natural kind Trachea plays a causal role in the accommodation of the inferential architecture of respiratory biology such that reference to Trachea by the natural kind term ‘trachea’ contributes to the epistemic achievements of respiratory biology. Deployment of the natural kind term ‘trachea’ in the theories and explanations of respiratory

biology leads to claims about the natural kind Trachea, and the accuracy of these claims can be assessed through causal interaction with the family of properties that underlie the natural kind.

Boyd (1999a, 1999b, 2003, 2010b) describes reference to natural kinds more precisely as follows:

Let M be a disciplinary matrix and let t_1, \dots, t_n be the natural kind terms deployed within the discourse central to the inductive/explanatory successes of M . Then the families F_1, \dots, F_n of properties provide definitions of the kinds referred to by t_1, \dots, t_n , and determine their extensions, just in case:

1. (*Epistemic access condition*) There is a systematic, causally sustained, tendency—established by the causal relations between practices in M and causal phenomena in the world—for what is predicated of t_i within the practice of M to be approximately true of things which satisfy F_i , $i = 1, \dots, n$. In particular, there is a systematic tendency for things of which t_i is predicated to have (some or most of) the properties in F_i .
2. (*Accommodation condition*) This fact, together with the causal powers of things satisfying these explanatory definitions, causally explains how the use of t_1, \dots, t_n in M contributes to the accommodation of the inferential practices of M to relevant causal phenomena. It explains whatever tendency there is for participants in M to identify causally sustained generalizations, to obtain correct explanations, or to obtain successful solutions to practical problems.

This means that the definition of a natural kind term is determined (on the one hand) by its use within a disciplinary matrix and the inferential connections between it and other natural kind terms that are also used within a matrix, and (on the other hand) by the naturally occurring causal phenomena the term is tracking. The deployment of a natural kind term and its use in the epistemic achievements of a disciplinary matrix *alone* establishes a natural kind, and the natural kind's definition is nothing more than the naturally occurring causal phenomena being tracked by the natural kind term.

There are three noteworthy consequences of Boyd's account of reference that may appear to be potential weaknesses of his view: 1) "A natural kind is nothing (much) over and above a natural kind term together with its use in the satisfaction of accommodation demands" (Boyd, 2010b, p. 220), 2) natural kinds are discipline relative, and 3) natural kind terms may fail to refer due to the limiting approximation of natural kind definitions.

1) According to Boyd, natural kinds are theory-dependent social constructions that are employed to meet the needs of disciplinary matrices in satisfying their accommodation demands.³⁷ He further claims that “natural kinds are features, *not of the world outside our practice*, but of the ways in which that practice engages with the rest of the world” (Boyd, 1999b, p. 66, emphasis is mine). However, Boyd argues that while natural kinds are indeed social artifacts, that is, they do not exist independently of our scientific practices, their definitions are the result of a *bicameral* legislative process between the linguistic and inferential practices of our sciences that establish natural kinds and that detect naturally occurring causal phenomena in the world. Our disciplinary matrices “get on” to the real essences of the natural kinds they employ within their inferential architectures, because the real essences just are the causal powers in the world that causally interact with the inferential practices of our sciences in the process of accommodation. So, when we say that the natural kind Water *is* H₂O, we are saying nothing more than that most of the liquids we call ‘water’ are mainly made of H₂O molecules and that this explains the epistemic achievements of the use of the term ‘water’ in our theories, explanations, and scientific discourse. Therefore, “if questions about the systematic relations of our epistemic practices to causal structures are not metaphysical questions, then it’s not a metaphysical fact that water is H₂O” (Boyd, 2010b, p. 222). The claims our scientific theories make about natural kinds are just as much about the natural kinds we invent as the causal phenomena that underlie them.

2) Because natural kinds and natural kind terms are employed in the inferential practices of disciplinary matrices pursuing the satisfaction of their accommodation demands, and because the definitions of natural kinds and natural kind terms are dependent upon the causal phenomena relevant to the satisfaction of a given disciplinary matrix’s accommodation demands, Boyd

³⁷ This idea is borrowed from Locke. Locke claims that “such a manner of sorting of things is the workmanship of men” (Locke, 1996, Book III, Chap. 6, Sec. 37)

argues that natural kinds and the terms referring to them are *discipline relative*. This initially seems obvious. For example, the natural kind Trachea will not be a natural kind employed in the inferential architecture of particle physics, but it will be a causally important natural kind in respiratory biology. If the natural kind Trachea was only employed in respiratory biology, and if for some reason this discipline ceased to exist, then as far as scientists working in particle physics are concerned, the natural kind Trachea does not exist. But this raises an interesting and potentially problematic question for Boyd's account: if natural kinds are discipline relative, then can there be instances in which a natural kind with a seemingly indubitable definition, such as the natural kind Water being H_2O , nevertheless be contingently defined? Even if Boyd grants that the well-confirmed definition of Water is shared by all disciplinary matrices here in the actual world, the skeptic can still inquire about Water's definition on other possible worlds.

Boyd's reply is relatively straightforward: if what is being asked is whether the natural kind Water as defined in the disciplinary matrices of the actual world would also be defined by the molecular structure H_2O in other possible worlds if we exported our disciplinary matrices, then the answer will depend on whether or not the relevant causal phenomena are present on the other possible worlds such that the exported disciplinary matrices are still able to satisfy their accommodation demands by reference to Water. However, if what is instead being asked is whether the natural kind Water as defined in this world would be defined by the molecular structure H_2O in other possible worlds if a new disciplinary matrix was established in that world, then the answer is most likely "no" since our definition of Water as H_2O is dependent upon the accommodation demands of our disciplinary matrices, which are unlikely to be the same as a disciplinary matrix established on another world. On Boyd's account, natural kinds are not eternally defined by necessary and sufficient conditions.

3) Because the definition of a natural kind is contingent on the epistemic achievements of a particular disciplinary matrix, it may occasionally be the case that a natural kind term either fails to refer or partially denotes two or more natural kinds. Any history buff of science can produce a plethora of examples where natural kind terms have failed to refer, which has been the case with the terms ‘phlogiston’, ‘luminiferous ether’, ‘vital forces’, and ‘Brontosaurus’. These terms have failed to refer because the relevant causal phenomena that were thought to be the referents of these words turned out to be non-existent. So, at some point these natural kind terms no longer contributed to the epistemic achievements of the disciplinary matrices in which they were deployed and were subsequently dropped.

Sometimes natural kind terms may refer to two or more natural kinds, which can impede the satisfaction of accommodation demands, since one term is tracking two or more kinds. This is known as partial denotation.³⁸ Partial denotation can be remedied through *denotational refinement* in the sense of Field (1973). Boyd provides a nice example of how denotational refinement works within his accommodationist view of scientific practices:

Roughly, a term t partially denotes different kinds k_1 and k_2 in a disciplinary matrix M when the epistemic connection between the uses of t in M and k_1 explains very nearly the same achievements in M as does the connection between t and k_2 . In practice, practitioners in M do not distinguish between k_1 and k_2 ; their use of t corresponds, in a sense, to something like the union of k_1 and k_2 . Nevertheless, the reliability of their practice is compromised by this feature of their conceptual and linguistic practices. An improvement in reliability could be achieved by drawing the k_1 - k_2 distinction and by replacing the existing use of t with the use of two terms (one of which might, but need not, be t), one referring to k_1 and the other to k_2 . (Boyd, 2010b, p. 216)

3.7. The theory-dependence of scientific methods

Boyd argues that scientific methods are profoundly theory-dependent because of the central role contingently defined natural kinds play in the epistemic achievements of scientific theories, and because of the reliance on nonexperimental criteria, that is, standards given by background theo-

³⁸ Partial denotation should be quite interesting for biologists, since they are often confronted with having to decide whether to split or lump species.

ries, for theory invention and testing. According to Boyd, our scientific laws and methods are socially and historically situated, irreducibly political, and non-foundational.

Boyd's view on the theory-dependence of scientific laws can be best illustrated by reference to his account of natural kinds.³⁹ As discussed above, the definitions of natural kinds are the result of an ongoing process of accommodation between the inferential and social architectures of our scientific practices and naturally occurring causal phenomena in the world. Furthermore, the definitions of natural kinds are contingent upon the satisfaction of accommodation demands specific to a disciplinary matrix. If our scientific laws are going to quantify over natural kinds with contingent definitions, whose use in epistemic achievements is discipline relative, then our scientific laws cannot be fundamental, exceptionless, eternal, and ahistorical. That is, if our scientific laws are going to apply to contingently defined natural kinds, then the laws too must be contingent or else they will fail to accurately reflect our current understanding of independently existing causal phenomena. Because natural kinds “are *always* interest and practice dependent social constructions, and *often* defined by open textured (neither necessary nor sufficient), historically situated, relationally and historically defined, and thus non-eternal and non-intrinsic properties,” (Boyd, 1999b, p. 69) our scientific “laws” must also be interest and practice dependent social constructions, whose statements are contingent upon an ongoing process of accommodation. According to Boyd, “the notion of a *causally sustained regularity* should replace that of a LAW, as that notion was (and is) understood in much of analytic philosophy” (Boyd, 1999b, pp. 59-60).

In addition to our scientific laws, Boyd argues that our scientific methods are profoundly theory-dependent because they rely on nonexperimental criteria in theory invention and confir-

³⁹ Remember, these are natural kinds in the sense of homeostatic property clusters.

mation. Nonexperimental criteria are standards, rules, or principles of experimental design, evidential assessment, theory testing, etc., that are not reducible to observational patterns. That is, nonexperimental criteria are the result of theoretical judgments. This theory-dependence of our scientific methods is a consequence of the accommodation thesis.⁴⁰ Boyd identifies four important contexts in which our scientific methods are dependent upon non-experimental criteria, that is, theoretical judgments: 1) judgments of projectibility, 2) the critique, modification, or extension of measurement and detection procedures for theoretical phenomena, 3) theory testing, and 4) theory invention.

1) Goodman's (1983) notion of projectibility plays an important role in Boyd's theory of accommodation. Goodman sought to address a problem scientists confront on a daily basis, namely the justification for the acceptance of some theories (inductive predictions or generalized explanations) rather than others from an infinite number of available alternatives. According to Goodman (1983), scientists should only adopt *projectible* theories. A theory is *projectible* if and only if it is supported by favorable evidence, it is unviolated by unfavorable evidence, it is testable, and it is better supported by background theories than other conflicting theories. Otherwise, a theory is not projectible.

We can now modify Goodman's notion of projectibility using Boyd's account of natural kind terms. On Boyd's account, a theory is *projectible* within a given disciplinary matrix if and only if it a) makes an appropriate use of natural kind terms deployed in the discourse of the disciplinary matrix and b) makes appropriate claims about natural kinds embedded within the disciplinary matrix as indicated by *currently accepted background theories*. That is, out of the infinite number of possible theories that can be invented or adopted within a disciplinary matrix, only

⁴⁰ Remember, the accommodation thesis states that the instrumental reliability of our scientific theories and methods can only be explained by the tendency of the conceptual features of our scientific disciplines, such as their theories, concepts, classificatory practices, inferential standards, and standards of experimental design to accommodate to naturally occurring causal phenomena.

those that are relatively similar to our currently accepted background theories will be taken seriously.

For example, the traditional theory explaining our observations of the fossil record is known as *gradualism*, which claims that the fossil record is evidence that organisms have incrementally changed over the past 500 million years. Let's say scientists want to know which of the following two theories is a projectible alternative to gradualism: creationism or punctuated equilibrium. According to *creationism*, the fossil record is evidence for a worldwide flood, such as the Flood of Noah, and is consistent with geological catastrophism (Ham, 1982). According to *punctuated equilibrium*, the fossil record is evidence that organisms undergo periods of sudden or accelerated change, which results in the apparent imperfect preservation of "gradually" changing organisms in the fossil record (Eldredge & Gould, 1972). In assessing creationism and punctuated equilibrium in light of the currently accepted background theories about geology, evolution, physics, etc., scientists will clearly favor punctuated equilibrium over creationism because the former makes appropriate use of natural kind terms and natural kinds embedded within paleontology, whereas the latter rejects currently accepted theories about evolution, geological processes (uniformitarianism), physics (dating), etc. In this example, punctuated equilibrium is the projectible alternative to gradualism because when compared to currently accepted background theories, punctuated equilibrium will be judged *theoretically plausible*.⁴¹

Because judgments of theoretical plausibility are dependent upon currently accepted background theories, and because the acceptance of our current background theories is dependent upon their role in the epistemic achievements of our scientific practices engaged in accommodation with the world, the criteria for judgments of theoretical plausibility are also contingent upon

⁴¹ Boyd (1982, 1985c) also argues that judgments of simplicity and parsimony are actually types of judgments of theoretical plausibility.

their role in satisfying the accommodation demands of the disciplinary matrix employing them. In other words, the ongoing process by which scientists choose which theories to take seriously is itself the result of an ongoing process of accommodation.

2) The second way our scientific methods are theory-dependent according to Boyd involves the critique, modification, or extension of measurement and detection procedures for unobservable or theoretical phenomena. Boyd illustrates the theory-dependence of our measurement and detection procedures in the following example:

Suppose that $T_1(t)$ is a well confirmed theory containing the theoretical term t , and supported by observations in some class D . Suppose that ‘measurements’ of t have thus far been possible only using measurement procedures m_1, \dots, m_r whose reliability is asserted by ‘mini-theories’ M_1, \dots, M_r . Now imagine that some new theory $T_2(t)$, only distantly related to T_1 , is confirmed by entirely different observations. T_2 has the happy consequence, M_{r+1} , that a new procedure m_{r+1} is suitable for the ‘measurement’ of t under circumstances well outside the range of application of m_1, \dots, m_r . Under these circumstances, if T_1 and T_2 are each quite well supported by experimental evidence, we shall confidently expect $T_1(t)$ to yield true (or approximately true) predictions when employed with the new measurement procedure m_{r+1} . (Boyd, 1982, pp. 619-620)

This example shows that the use of measurement and detection procedures within one theory often depends on the theoretical claims made by another, indirectly related theory. For example, the ability to use electron microscopes to detect microorganisms or micro-structures in biology is dependent on the physical and chemical theoretical claims about theoretical entities such as electrons and electromagnetic forces.

As Boyd’s illustration implies, the critique, modification, or extension of measurement and detection procedures of theoretical phenomena is only possible if the theoretical term t is *univocal* in the conjunction of T_1 and T_2 . That is, the definition of the theoretical term t is the same in both T_1 and T_2 such that the deployment of t in both theories has a tendency to track the same family of properties. Univocality underwrites the “unity of science” principle, which states that the observational consequences of two or more theories can be combined such that new or more complex observational consequences can be derived because the terms used in those theories

share the same definitions. The derivation of observational consequences from two or more theories is only possible if their theoretical terms are univocal, and since univocality is the only explanation for the successful use of theory-dependent methods for measurement and detection, we have good reason to believe in the “unity of science” principle. (Remember, theoretical terms are types of natural kind terms, so their definitions are determined by their inferential role in the epistemic achievements of the matrices deploying them.) Furthermore, because all theories are apt to change as a result of the accommodating process taking place between them and causal phenomena, the reliability of our current measurement and detection procedures is dependent upon their role in the epistemic achievements of our scientific practices.

3) The third important way in which our scientific methods are theory dependent according to Boyd concerns theory testing. We have already seen that scientists prefer theoretically plausible (projectible) theories (gradualism and punctuated equilibrium) over those that are not theoretically plausible (creationism). Because judgments of theoretical plausibility rely on currently accepted background theories, and because background theories are currently accepted due to their reliable role in satisfying accommodation demands, a judgment of theoretical plausibility for a theory is (at least) directly confirming. So how do we determine if a given theoretically plausible theory is the best one?

Boyd identifies three rules scientists seem to follow in determining whether or not a given theoretically plausible theory *T* is the (contingently) best one:

Rule 1: Basic methodological rule of science: Carefully choose from among relevant alternatives (that is, theories recommended by best current theories), controlling for artifacts suggested by best current theories.

Rule 2: Basic ‘falsificationist’ rule for objective testing: Try to falsify *T*. Test *T* where it’s most vulnerable, *i.e.*, under circumstances where it’s observational consequences are most likely to go wrong if they can go wrong.

Rule 3: Rule for identifying such circumstances: Identify the most plausible alternatives to *T*: its projectible rivals. Similarly for identifying experimental artifacts. Use these rivals in devising tests for carrying out Rule 2.⁴²

According to Rule 1, only (but not all) those theories that are theoretically plausible as indicated by the currently accepted background theories as well as the contingent beliefs and concepts making up the inferential architecture of a disciplinary matrix should be adopted. According to Rule 2, theories should be tested in ways most likely to lead to their “falsification” if they are falsifiable.⁴³ And according to Rule 3, theories should be tested by comparing their observational consequences to those of their most plausible rivals. Once theories are tested against their theoretically plausible rivals, scientists evaluate the gathered evidence according to currently accepted standards of evidence to determine whether or not their chosen theory *T* is confirmed or disconfirmed. According to Boyd, a chosen theory *T* will be confirmed by evidence *E* if and only if *T* is theoretically plausible, *E* favors *T* over its theoretically plausible rivals, and *E* was gathered to control for experimental or observational artifacts in the ways indicated by currently accepted background theories about experimental design and data collection.⁴⁴

Each time a scientific theory is directly confirmed (or disconfirmed) by a given class of evidence, the methodology and background theories used to invent, test, and evaluate that theory will also be confirmed (or disconfirmed). In other words, the nonexperimental criteria “turn out to be nonexperimental only in the sense that they do not reflect the assessment of *direct* experimental evidence” (Boyd, 1985c, p. 374). Rather, nonexperimental criteria reflect the assessment of *indirect* experimental evidence due to the role they play in the accommodation process. Our scientific practices can be characterized as a dialectic interaction between scientific methodology and scientific theories shaped by their ongoing accommodation to causal phenomena in the

⁴² This formulation of the three rules is taken from Boyd (2010b, p. 213) with slight alterations.

⁴³ I do not intend for my use of ‘falsification’ to be in the sense of Popper (1963) and his philosophy of falsificationism.

⁴⁴ “Data are not ‘raw’; there are no ‘brute facts’.” (Shapere, 1969, p. 119)

world; theory modification informs method modification, which informs theory modification, and so on.

The reliability of this “theory of theory confirmation” is itself dependent upon its contribution to the epistemic achievements of the disciplinary matrix that employs it (Boyd, 1999b, p. 61). In other words, there is no *a priori* “scientific method”. According to Boyd, the scientific method just “is a paradigm dependent paradigm-modification strategy: a strategy for modifying or amending our existing theories in the light of further research, which is such that its methodological principles at any given time will themselves depend upon the theoretical picture provided by the currently accepted theories” (Boyd, 1989, p. 9).

4) The theory-dependence of our scientific methods is seldom more obvious than in the case of theory invention. Theory invention is the process in which scientists create generalizations about phenomena. As we have seen up to now, theory invention within a disciplinary matrix involves all the theory-dependent ingredients contained within the inferential architecture, such as natural kinds, natural kind terms, experimental and evidential standards, etc. However, theory invention also involves everything associated with the *social architecture* of our scientific practices. Boyd summarizes the influence of the social architecture on theory invention in the following passage:

It is not sufficient that accepted background theories be such as to, in fact, inductively support answers to scientific questions which (often enough) include good approximations to the truth. What is required is that the availability of relevant information, the structure of inductive practices and cognitive resources within the relevant scientific communities, and the social practices of communication and evaluation within them, be such that *suitably articulated* formulations of good approximations actually *get formulated* and that their predictive and explanatory resources be *sufficiently widely* appreciated. The lone, underfunded, socially marginalized researcher who makes the relevant inference from background theories to the correct answer to a scientific question, but whose research does not get published in established journals, does not thereby make a contribution to the reliability of scientific practice. (Boyd, 1999b, pp. 61-62)

Not only do scientists have to be sufficiently imaginative to produce projectible theories,⁴⁵ but they have to successfully communicate and disseminate their hypotheses in order to contribute to the epistemic achievements of our scientific practices. Although scientific communities are no less political than any of our other institutions, the politics of science play a substantial role in the epistemic successes of our scientific practices. “*The epistemology of science is necessarily a (partly) political discipline*” (Boyd, 1999b, p. 85, emphasis in original).

3.8. Approximate truth

That our scientific methods and theories are instrumentally reliable is much less controversial than the explanation for why this is the case. As discussed above, Boyd maintains that the best explanation for the reliability of our scientific practices is a realist one that is characterized by a dialectic interaction between theories and methods as they undergo a process of accommodation to the causal phenomena in the world. Furthermore, he maintains that the epistemic achievements that result from a process of accommodation are only possible if scientists do in fact possess knowledge of observables and unobservables alike. On Boyd’s account, knowledge of phenomena is not socially constructed, but rather reflects something approximately *true* of the world. However, this suggests that realists require a well-defined notion of approximate truth in order to explain the reliability of our scientific practices from a realist viewpoint.

Tarski (1935) and subsequent workers have produced a formalized, uniform correspondence theory of *exact* truth (although this is controversial). However, a theory of exact truth is inadequate for the assessment of most of our scientific achievements, because the epistemic achievements of our sciences depend on historically dependent criteria for theory confirmation. For example, it is not clear how a theory of exact truth can help us arbitrate between the Lamarckian

⁴⁵ According to Boyd, “failures of theoretical imagination can thus render the methodological practices we are discussing epistemologically unreliable in particular cases” (Boyd, 1985c, p. 90).

and Darwinian theories of evolution since neither seems to be totally true or false and both were once considered the best explanation of their day. Rather, scientific realism requires a notion of approximate truth in order to explain what the realist views as the successive approximations to truth our sciences have made over the last five hundred years. Boyd (1990a) argues for a notion of approximate truth based on his views of scientific methodology and theory confirmation, which is thin enough to account for the numerous times in the history of science that scientific theories have got it wrong, but thick enough to avoid being so trivial that *any* consistent theory or group of theories can be approximately truth.

Boyd claims that knowledge of theory-independent phenomena (both observables and unobservables) is possible and often *actual* (Boyd, 1982, 1989, 1990a). His account of approximate truth just is an account of how scientific knowledge of theory-independent phenomena is possible. According to Boyd,

truth is definable from ‘primitive denotation’ (Field, 1974), and denotation, on the realist’s account, is an epistemic and thus a causal matter; truth is correspondence truth and correspondence is a matter of complex causal interactions. Similarly, to talk of respects of approximation to the truth is to talk of respects of similarity and difference between actual causal situations and certain possible ones. (Boyd, 1990a, p. 376)

Boyd argues that scientists acquire knowledge of theory-independent phenomena as the result of an ongoing process of accommodation between their methodological and theoretical practices and independently existing causal phenomena. Acquiring knowledge through accommodation is only possible if the important relevant features of our scientific practices are approximately true. And what features must be approximately true in order to explain the instrumental reliability of our scientific practices?

For starters, the background theories in mature scientific practices must be approximately true if we are to explain the contribution judgments of theoretical plausibility make to the epistemic achievements of those practices. The current best available theory will be amongst the

current projectible theories, because the currently accepted background theories from which projectibility judgments are derived *are* currently accepted because of the role they played in the epistemic achievements of the past. This is not to say that once accepted, background theories cannot be amended or discarded. Their current approximate truth is indicated by their current contribution in satisfying accommodation demands. As these demands change and as the accommodation process proceeds, the worth of currently accepted background theories is contingent on their future contributions.

For example, in his *Zoological Philosophy* Lamarck described the adaptation and evolution of organisms based on a mechanism of inheritance of acquired characteristics. Lamarck got it approximately right, and there was a time when Lamarckian evolution *did* contribute to the epistemic achievements of its day (and even later according to Spencer (1899)). However, as the accommodation demands changed during the 19th century, the contribution Lamarckian theories made to the success of scientific practices precipitously declined. In his *Origin of Species*, Darwin provided an alternative to Lamarckian evolution in his formulation of the theory of natural selection. Yet, Darwinian evolution only succeeded Lamarckian evolution because the former was considered a theoretically plausible alternative to the latter. Since the 1860s, Darwinian evolution has surpassed the epistemic achievements of Lamarckian evolution because Darwinian evolution gets it approximately right to a greater degree.

Background theories are approximately true and contribute to the reliability of scientific practices because the natural kind terms they deploy get it right in tracking the causal phenomena in the world. Denotational refinement (in the sense of Field (1973)) can lead to an increase in epistemic achievements and is only possible if natural kind terms reflect an approximate knowledge of independently existing causal structures. Similarly, the definitions of natural kinds

must be approximately correct when their causal role contributes to the successful accommodation of our theories and methods to the world. Furthermore, the inductive methods that guide the process of theory invention, the application of nonexperimental criteria such as background theories, and theory confirmation must be approximately correct when they contribute to the instrumental reliability of scientific theories. According to Boyd, “scientific methodology, dictated by currently accepted theories, is reliable at producing further knowledge *precisely because, and to the extent that, currently accepted theories are relevantly approximately true*” (Boyd, 1990a, p. 362, emphasis in original).

In summary, Boyd argues that the best explanation for the instrumental reliability of our scientific theories and methods is that the theoretical claims our theories make are approximately true and the theoretical judgments our methods require are approximately correct. Such reliability is the result of an ongoing process of accommodation between our scientific theories, methods, linguistic practices, including the deployment of contingently defined natural kind terms and the employment of contingently defined natural kinds, and naturally occurring causal phenomena in the world. It follows from this that whatever methodologies scientists employ in achieving their inductive and explanatory successes will be taken as approximately correct or adequate tools for investigating causal phenomena. As I argue in the following chapter, scientific models are an integrally reliable method in morphology and serve as an important and indispensable tool for establishing reliable morphological theories.

4. A REALIST VIEW OF MORPHOLOGICAL MODELS

A priori model building approaches have not been very successful in functional morphology, due in part to the enormous difficulty in building a model that reflects biological reality from first principles... (Lauder, 1995, p. 5)

I contend that 1) morphological models contribute to the instrumental reliability of morphological methods and to the epistemic achievements of morphological theories produced by those methods, because 2) the process of morphological modeling just is the process of accommodation as described by Boyd. I take it as uncontroversial that morphological models *contribute* to the instrumental reliability of morphological methods and to the epistemic achievements of morphological theories. Morphological modeling is a widely used method in morphological studies, and its methodological prevalence is best explained by its instrumental reliability.⁴⁶

My aim here is to argue that the best explanation for the instrumental reliability of morphological modeling is that it is a process of accommodation occurring between (on the one hand) the models constructed and developed by morphologists and (on the other hand) independently existing organismal phenomena. That is, morphological models are not mere heuristic devices adopted by convention, and their construction and deployment are not based on *a priori* rules and methods. I claim instead that morphological models are constructed from theory-dependent methods and are deployed in theory-dependent ways to aid morphologists in their quest to explain independently existing causal phenomena.

⁴⁶ A search for the word ‘modeling’ in the publication titles of the top ten major journals in morphology (ranked by impact factor from 1981-2007) returned 17,016 results. This total does not include publications that have incorporated modeling into their materials and methods, but did not mention ‘modeling’ in the publication title.

4.1. Morphological modeling: A process of accommodation

4.1.1. The theory-independence of organismal phenomena

Morphology is the study of organismal phenomena or phenomena involving biological organisms. We can think of the terms ‘organismal phenomena’ as referring to a natural kind, Organismal Phenomena. As such, Organismal Phenomena are homeostatic property cluster kinds, whose definition consists of whatever causal phenomena, such as causal structures, powers, mechanisms, processes, relations, etc., underwrite the epistemic achievements associated with the use of the natural kind terms, ‘organismal phenomena’, within morphological practices. The causal phenomena that define Organismal Phenomena are theory-independent, and claims about their metaphysical existence are subject to the restrictions imposed by the *No Non-Causal Contribution Thesis (2N2C)*. However, the properties and underlying causal powers, mechanisms, processes, relations, etc., underwriting the definition of Organismal Phenomena are determined by theoretical judgments that are based on the epistemic achievements of morphological practices. That is, the definition of Organismal Phenomena is contingent upon its contribution to the epistemic achievements of morphology. So, the members of the natural kind Organismal Phenomena just are the causal phenomena morphologists seek to explain, and the accuracy of morphological theories will be determined by how well they correspond to the phenomena they purport to describe.

4.1.2. Is morphology a disciplinary matrix?

Boyd defines a disciplinary matrix as “a family of inductive and explanatory aims and practices, together with the conceptual resources and vocabularies within which they are implemented” (Boyd, 1999b, p. 57). So, is morphology a disciplinary matrix? Suppose we take the following textbook description of morphology given by Kardong:

Morphology embraces the study of form and function, of how a structure and its function become an integrated part of an interconnected design (the organism), and of how this design itself becomes a factor in the evolution of new forms. The term **morphology** is not just a synonym for the word **anatomy**. It has always meant much more; for Cuvier, it meant the study of structure and function; for Owen, it meant the study of archetypes behind the structure; and for Huxley, it meant a study of structural change over time (evolution). Today, diverse schools of morphology in North America, Europe, and Asia all generally share an interest in the structural integration of parts, the significance of this for the functioning of the organism, and the resulting limitations and possibilities for evolutionary processes.... Morphological analysis focuses on higher levels of biological organization—at the level of the organism, its parts, and its position within the ecological community. (Kardong, 2009, p. 14, emphasis in original)

It is clear from this description that morphology is indeed an (historically) established academic discipline, whose domain of research is “the study of form and function” of organisms. However, this is a broad description, and it is not clear whether it encompasses a single disciplinary matrix. In fact, Kardong mentions “schools of morphology”, which implies that morphology is comprised of multiple disciplinary matrices. It is possible that these schools do share the same inductive and explanatory aims, utilize the same conceptual resources and vocabularies, and are demarcated solely upon their geographic location. But I think this is unlikely. For example, Asian schools of morphology probably differ to a large extent in their conceptual resources and vocabularies from European schools due to the differing cultural, political, and social factors. Therefore, I think it is safe to say that morphology broadly defined *cannot* be a disciplinary matrix.

Furthermore, morphologists often distinguish between various morphological sub-disciplines such as comparative morphology (or comparative anatomy), functional morphology (or functional anatomy), structural morphology, developmental morphology, paleomorphology, vertebrate morphology, invertebrate morphology, human morphology, veterinarian morphology, etc. These sub-disciplines differ in their explanatory aims and practices, and they employ vocabularies and concepts that differ from one another to various degrees. For example, the word ‘muscle’

has different definitions and is used in different contexts in vertebrate, invertebrate, and human morphology.

These academic sub-disciplines of morphology are not obvious disciplinary matrices either. For example, Homberger (1988) claims that studies within functional morphology vary to such an extent in the types of organismal phenomena they study, in the methods they use, and in the environmental context in which their studies are performed that they cannot be concisely described. Yet, according to Homberger (1988) studies in functional morphology share three things in common: they study the relationships between functional and structural aspects of biological systems, they utilize modeling as a primary method for investigating these relationships, and *they usually commence with an initial question*. This initial question may be about either proximal interests, such as “How does this organismal phenomenon work?”, or ultimate interests, such as “Why does *this* organismal phenomenon differ in different animals?” (Homberger, 1988). Nevertheless, having these three common elements does not necessarily establish functional morphology as a distinct disciplinary matrix.

Similar questions like those proposed in functional morphology can be posed in the context of the other morphological sub-disciplines. As Homberger (1988) argues, not only do initial questions about organismal phenomena mark the “start” of morphological investigations, but they also serve to guide the research methodology. Because there is nothing in this analysis peculiar to functional morphology, I think the description by Homberger (1988) can be expanded to include all morphological sub-disciplines. That is, all morphological sub-disciplines study organismal phenomena, deploy models to aid their studies, and begin their studies with a proximate or ultimate question about a particular organismal phenomenon.

Given the role initial questions play in guiding research methodology, it is reasonable to attribute the establishment of disciplinary matrices to the formulation of initial questions. That is, these questions “establish” the inductive and explanatory aims of the studies pursuing their answers and inform the adoption of the conceptual schemes and vocabularies those studies should utilize. Therefore, the morphological studies pursuing the answers to direct proximal or ultimate questions *are* the disciplinary matrices of morphology. In other words, initial questions generate accommodation demands for the disciplinary matrices that attempt to answer them. This means that each sub-discipline may contain numerous disciplinary matrices and that some research labs may even be characterized by multiple disciplinary matrices.⁴⁷

4.1.3. Organismal phenomena, features, and research roles

Morphologists in all disciplinary matrices study organismal phenomena, which are homeostatic property cluster kinds. This means that the natural kind Organismal Phenomena has the same definition in all disciplinary matrices. In other words, the definition of Organismal Phenomena is not discipline specific. Rather, the natural kind Organismal Phenomena is contingently defined by the union of the sets of causal phenomena that underwrite the accommodation demands in each morphological disciplinary matrix.⁴⁸ Organismal phenomena are the causal phenomena studied by morphologists, with each morphological sub-discipline focusing on a contingent subset of causal phenomena. For example, the set of causal phenomena vertebrate morphologists study is not the same set of causal phenomena invertebrate morphologists study, but together the

⁴⁷ By this point, the importance of posing the initial question about an organismal feature for the modeling and investigative process should be clear. To be undoubtedly clear I defer to Kant: “It is already a great and necessary proof of cleverness or insight to know what one should reasonably ask. For if the question is absurd in itself and demands unnecessary answers, then, besides the embarrassment of the one who proposes it, it also has the disadvantage of misleading the incautious listener into absurd answers, and presenting the ridiculous sight of one person milking a billy-goat while the other holds a sieve underneath” (Kant, 2009, p. 197).

⁴⁸ The natural kind Organismal Phenomena can be thought of as a higher-order natural kind, whose definition is dependent upon its use in multiple lower-level disciplinary matrices within the academic discipline of biological morphology.

two sets contribute to the set of organismal phenomena. So, what are the causal phenomena specific to each disciplinary matrix called?

As described in Chapter 1, a particular organismal phenomenon made the focus of an initial question, that is, studied within a particular disciplinary matrix, is called a *feature* (Bock & von Wahlert, 1965). Within each disciplinary matrix, morphologists “pick out” for study instantiations of the natural kind Feature. I shall refer to an instantiation of a natural kind Feature within a given disciplinary matrix, D , as Feature_D . Features_D , like organismal phenomena, are homeostatic property cluster kinds, and their definitions, *unlike* organismal phenomena, are discipline dependent. The natural kind Feature is partially defined by the collective definitions of Features_D employed within a range of disciplinary matrices, which are accommodating to the relevant causal phenomena that underwrite the accommodation demands for each disciplinary matrix. So, we can distinguish the natural kind Feature_D from the natural kinds Feature and Organismal Phenomena as follows: The natural kind Feature_D is directly defined by the causal phenomena studied within disciplinary matrices; the natural kinds Feature and Organismal Phenomena are indirectly defined by the causal roles the natural kind Feature_D plays within a range of disciplinary matrices such that they are indirectly defined by the causal phenomena directly defining each Feature_D . Therefore, in order for a given causal phenomenon to have membership in the natural kinds Feature and Organismal Phenomena, it must be associated with the definition of a Feature_D , whose use within a disciplinary matrix has contributed to that disciplinary matrix’s epistemic achievements.

The distinction between organismal phenomena and features is important for understanding the former’s role in “defining” morphology as an academic discipline and the latter’s role in establishing disciplinary matrices. Reference to features plays what Boyd (2010a) describes as a

“research guiding role” for disciplinary matrices. Here is an illustration of how reference to a feature might play a research guiding role in morphology: When a morphologist poses a question about an observable phenomenon related to an organism, such as “How does this phenomenon work?” or “What is the explanatory significance of this phenomenon versus that phenomenon?”, the phenomenon under inquiry becomes a feature of interest, F , for the morphologist. There are two possible ways in which a morphologist can follow-up on her initial question, each answering to one of the following two cases:

Case 1: If the morphologist can determine that F has the relevant properties constituting the definition of a currently accepted natural kind term, t , then the morphologist can use t to refer to F . She can now rephrase her question to ask “How does t work?” Now, the inferential role played by t that refers to F will determine the inferential architecture the morphologist will adopt for her study of F . That is, reference to F using t engages the theoretical resources associated with the definition of t in the disciplinary matrix deploying it to bear on F .

Case 2: If the morphologist cannot determine whether F has the relevant properties constituting the definition of a currently accepted natural kind term (which might occur if the causal phenomena initially defining F have never been studied), then the morphologist will either adopt an inferential architecture deemed appropriate for the study of F as the result of a theoretical judgment, or the morphologist will develop a new inferential architecture for the study of F using relevant background theories as her guide. That is, the similarities of the properties initially defining F to the properties of other natural kinds embedded within her disciplinary matrix, will “direct” the morphologist to adopt an appropriately relevant inferential architecture or to develop an appropriately structured inferential architecture.⁴⁹

⁴⁹ For example, an inferential architecture developed for and by the study of “aliens” is not currently available (as far as I know). However, if E.T. were to crash-land on Earth, morphologists would assemble a new inferential architecture that draws upon the

So, reference to F largely determines how F will be studied.⁵⁰ However, no matter what inferential architecture is initially adopted, its continued use is contingent upon its contribution to satisfying accommodation demands.

4.1.4. Forms, functions, faculties, features and HPC kinds

Within a disciplinary matrix, Feature_D are defined by the independently existing causal phenomena that serve to underwrite the disciplinary matrix's accommodation demands. The definition of the natural kind Feature_D is dependent upon the causal role reference to it plays in the epistemic achievements of a *particular* disciplinary matrix. Like all homeostatic property cluster kinds, the natural kind Feature_D is defined by naturally occurring families of properties (causal phenomena). Let's call the family of properties defining a given Feature_D within a disciplinary matrix the *feature-family* of properties. In order for a property to have membership within the natural kind Feature_D , it must be part of the feature-family of properties that contributes to the causal role that reference to the natural kind Feature_D plays within a particular disciplinary matrix's inferential architecture.

Because a feature-family of properties can contain an extraordinary number of properties and because the underlying causal powers, mechanisms, relations, etc., sustaining the clustering of those properties often form complex causal systems, morphologists subdivide feature-family properties into subfamilies of properties called *form-families*, *function-families*, and *faculty-families* of properties to better organize organismal complexity. Recall that Bock and von

theoretical resources of currently established inferential architectures. In other words, we could custom build an inferential architecture for E.T. without having to reinvent morphology (although this may happen as we study E.T. over time and learn more about astrobiology).

⁵⁰ Of course, the social architecture is also in play. For example, the morphologist may be forced to adopt the inferential architecture that's most likely to bring funding into her scientific community, or she may be forced to adopt the one that will make a greater contribution to her efforts in achieving tenure, although neither may be explanatorily the best.

Wahlert (1965) define a *form* as a temporally specific appearance, shape, configuration, or material composition of an organismal feature. Because these properties do not describe a biological process or mechanism, let's call them *non-contributing* properties, that is, they do not consist of the underlying causal powers, processes, mechanisms, relations, etc., that sustain their clustering. So, a *form-family* of properties will be comprised of those non-contributing properties within the feature-family of properties that correspond to the appearance, shape, configuration, or material composition of a feature. The form-families of properties contingently define the Forms_D of a Feature_D .

Recall that Bock and von Wahlert (1965) define a *function* as a physical or chemical property or process associated with a form of an organismal feature. Because these properties describe biological processes, mechanisms, and relations, let's call them *contributing* properties, that is, they consist of the underlying causal powers, processes, mechanisms, relations, etc. that sustain the clustering of a form-family of properties. So, a *function-family* of properties will be comprised of those *contributing* properties that sustain a form-family. That is, a function-family is comprised of those phenomena that hold a form-family together. Think of a form-family as a cluster of relevant properties, and think of a function-family as the “glue” holding those properties together.⁵¹ The function-families of properties contingently define the Functions_D of a Feature_D .

Recall that Bock and von Wahlert (1965) define a *faculty* as the role a function (or functions) associated with a form (or forms) plays in the life of an organism. Also recall that faculties are known as *form-function complexes* because the roles faculties play are determined by the relationship between forms and their requisite functions. So, a *faculty-family* of properties will be

⁵¹ As described in Chapter 1, this glue can have static or dynamic properties.

comprised of both the non-causal form-family properties and the causal function-family properties that contingently define a group of Forms_D and Functions_D within a form-function complex. The faculty-families of properties contingently define the Faculties_D of a Feature_D .

4.1.5. A causal notion of model deployment

Morphologists deploy models to produce interpretations and representations from observational data collected from independently existing causal phenomena. The interpretations and representations produced by morphological models are incorporated into the definitions of the natural kinds Feature_D , Faculty_D , Function_D , and Form_D , that are embedded within disciplinary matrices. Reference to these natural kinds contributes to the inductive and explanatory successes within disciplinary matrices, and, as a result, morphological models, like natural kind terms, have a tendency to track the causal phenomena they interpret and represent. That is, morphological models contribute to the accommodation of the inferential architectures of disciplinary matrices to causal phenomena in the world by underwriting the definitions of the natural kinds used within the inductive and explanatory claims of disciplinary matrices. Therefore, morphological modeling is an important methodology because it contributes to the instrumental reliability of morphological theories. So, how do morphological models contribute to the definitions of the natural kinds Feature_D , Faculty_D , Function_D , and Form_D ?

I argue that feature-models help determine the definition of the natural kind Feature_D within a given disciplinary matrix in the following way: Recall from Chapter 1 that morphologists deploy *feature-models* to interpret and represent organismal features. A morphologist deploys a feature-model to interpret and represent a collection of observational data about particular causal phenomena that are theoretically judged by the morphologist to have membership within a natural

kind Feature_D , F_D . The interpretations and representations produced by the feature-model provide new information about F_D , and, therefore, begin or extend the definition of F_D . The new or extended definition of F_D becomes incorporated into the inferential architecture of the disciplinary matrix and underwrites the natural kind term t that refers to F_D .

The natural kind term t plays an inferential role within the disciplinary matrix, which is determined by how it is used to produce new inductive and explanatory claims. If the use of t contributes to the epistemic achievements of the disciplinary matrix, then the definition of F_D will be confirmed. If the definition of F_D is confirmed, then the interpretations and representations produced by the feature-model will also be confirmed. So, the use of t to produce instrumentally reliable theories indicates that the methodology used to produce the feature-model's interpretations and representations, which contribute to the definition of t , is also instrumentally reliable.

The inductive and explanatory successes attained when referring to the natural kind F_D indicate that the definition of F_D is approximately correct, which also indicates that the interpretations and representations of the feature-model underwriting F_D are approximately correct as well. The best explanation for the contribution reference to F_D makes to the epistemic achievements of the disciplinary matrix is that the feature-model is approximately, yet successfully, tracking a feature-family of properties. Because membership within the feature-family of properties is dependent upon the causal role F_D plays within the inferential architecture of the disciplinary matrix and because the causal role F_D plays is largely a consequence of the interpretations and representations produced by the feature-model, the feature-model helps to determine which properties have membership within feature-families. Furthermore, because the interpretations

and representations produced by the feature-model are subject to the various model tests described in Chapter 1, the membership conditions prescribed by the feature-model are contingent upon the contribution its interpretations and representations make to the epistemic achievements of the disciplinary matrix. Therefore, the definition of F_D is contingent upon the role feature-model interpretations and representations play in the epistemic achievements of the disciplinary matrix.

What I have just described is how I think feature-models contribute to the definitions of $Features_D$. But, this does not address how I think the definitions for the natural kinds $Forms_D$, $Functions_D$, and $Faculties_D$ are determined. I think their definitions are determined and are related to the definition of their Feature in the following ways: Recall from Chapter 1 that the representations and interpretations of feature-models are dependent upon the interpretations and representations of their hierarchically related sub-models: form-models, function-models, and faculty-models. The deployment of these sub-models follows the same methodology as the deployment of the feature-model, because, like the feature-model, the sub-models are deployed to interpret and represent a set of observational data about particular organismal phenomena. The key difference between the feature-model and its sub-models is that the former interprets and represents the *entire* feature-family of properties while the latter interprets and represents *subsets* of properties within the feature-family of properties.

Form-models are the first of the sub-models to be deployed, and they interpret and represent the forms of an organismal feature as determined by a morphologist. That is, form-models interpret and represent a subset of (non-contributing) properties within the feature-family of properties that are theoretically judged by a morphologist to have membership within the natural kind $Form_D$. The relationship between a form-model and the definition of the natural kind $Form_D$ and

the relationship between the natural kind Form_D and the natural kind term referring to it are the same as the relationships between feature-models, natural kinds, and natural kind terms. And the best explanation for the contribution made by form-models, kinds, and terms to the epistemic achievements of the disciplinary matrix is that form-models are approximately, albeit successfully tracking form-families of properties. Furthermore, the definition of the natural kind Form_D is contingent upon both its causal role within a disciplinary matrix as well as the results of the model tests described in Chapter 1.

The deployment of a form-model is the first step to the deployment of a function-model. Recall from Chapter 1 that a *function-model* is deployed to interpret and represent the functions of an organismal feature. Also recall that a function is a physical or chemical property or process associated with a form. The functions of a feature are related to a particular form or group of forms as described in Chapter 1. Therefore, the interpretations and representations of a function-model will be dependent upon those of a least one form-model. In other words, function-models are deployed to interpret and represent a subset of causal properties with a feature-family of properties that are theoretically judged by a morphologist to be associated with one or more form-families of properties. Because the morphologist's judgment is based on the form-families of properties and, therefore, the interpretations and representations form-models produce, form-models play a sort of research guiding role for the deployment of function-models.

The same relationships hold between function-models, natural kinds, and natural kind terms as they do for form-models, natural kinds, and natural kind terms. Similarly, the best explanation for the contribution made by function-models, natural kinds, and natural kind terms to the epistemic achievements of a disciplinary matrix is that function-models are approximately, yet successfully tracking function-families of properties. And, as with the definition of the natural

kind Form_D , the definition of the natural kind Function_D is contingent upon both its causal role within the disciplinary matrix as well as the results of the model tests described in Chapter 1.

Like function-models, the interpretations and representations produced by faculty-models are dependent upon those produced by lower-level models. Recall from Chapter 1 that *faculty-models* are deployed to interpret and represent faculties of organismal features. Also recall that faculties are also known as form-function complexes. Therefore, the interpretations and representations of faculty-models are dependent upon those produced by associated form- and function-models. In other words, faculty-models are deployed to interpret and represent a subset of properties within a feature-family of properties that are theoretically judged by a morphologist to be comprised of a union of at least one form-family of properties with its associated function-family of properties. Similarly, because the morphologist's judgment is based on the combination of form-families of properties and function-families of properties, and, therefore, the interpretations and representations both types of models produce, form- and function-models play a sort of research guiding role for the deployment of faculty-models.

Likewise, the same relationships hold between faculty-models, kinds, and terms as they do for form- and function-models, natural kinds, and natural kind terms. Similarly, the best explanation for the contribution made by faculty-models, natural kinds, and natural kind terms to the epistemic achievements of a disciplinary matrix is that faculty-models are approximately, yet successfully tracking faculty-families of properties. And, as with the definition of the natural kind Form_D and Function_D , the definition of the natural kind Faculty_D is contingent upon both its causal role within the disciplinary matrix as well as the results of the model tests described in Chapter 1.

In summary, the interpretations and representations produced by feature-models are dependent upon those produced by its form-, function-, and faculty-models. Because the interpretations and representations of each type of model underwrite the definition of their corresponding natural kind, and because the interpretations and representations produced by the feature-models are dependent upon those of its sub-models, the definition of the natural kind Feature_D and the inferential role played by the natural kind term that refers to it will be dependent upon the definitions of the natural kinds Form_D , Function_D , and Faculty_D and the inferential roles played by the natural kind terms that refer to them. And the definitions of these natural kinds and the inferential roles of their natural kind terms are contingent upon the role they play in the inductive and explanatory successes of the disciplinary matrix deploying them.

We can now describe the general role morphological models have within Boyd's account of reference described in Chapter 3. As described in Chapter 3, the deployment and successful use of a natural kind term establishes a natural kind, and when used in successful inductive and explanatory claims, natural kind terms have a tendency to track independently existing causal phenomena. In turn, the definition of the natural kind being referred to by the natural kind term is dependent upon the causal role the natural kind plays and the inferential role the natural kind term plays within the inferential architecture of a disciplinary matrix. Models, like natural kind terms, also track independently existing causal phenomena, and as a result models produce interpretations and representations. Model interpretations and representations produce new information about the causal phenomena they are tracking, and, therefore, provide new information about the natural kinds defined by the same causal phenomena. Hence, the definition of natural kinds is partially attributed to the causal role they play within a disciplinary matrix and partially attributed to the interpretations and representations produced by models. But because the defini-

tion of natural kinds is largely dependent upon the interpretations and representations produced by models, the causal role they play is also dependent upon the interpretations and representations of models. So, *models contribute to the satisfaction of accommodation demands for a disciplinary matrix by facilitating the accommodation of natural kind definitions to independently existing phenomena.*

4.2. The theory-dependence of model interpretation and representation

The account of model deployment I describe above is consistent with Boyd's version of reference, but my account is not obviously realistic. To see how morphological models and their deployment can be viewed through Boyd's version of realism, we need to take a closer look at the role theoretical judgments play in modeling and in the production of interpretations and representations. In this section, I argue that the interpretations and representations models produce are highly theory-dependent and that the best explanation for the reliability of the interpretations and representations models produce for defining natural kinds is that the interpretations and representations produced by morphological models must get it approximately correct, that is, they are approximately true. There are two important ways that morphological modeling is theory-dependent: 1) Theoretical judgments are a large component in the production of the interpretations and representations from models, and 2) theoretical judgments are necessary for establishing the representational relationships shared between multiperspectival models as described in Chapter 2.

1) The production of interpretations and representations from morphological models results from a series of theoretical judgments a morphologist makes in accordance with nonexperimental criteria. There are three important theoretical judgments morphologists as model users must

make: judgments about model function and selection, judgments about model synthesis, and judgments about model type and expression.⁵²

First, morphologists must determine the function and selection of their models. Recall from Chapter 2 that a model's *function* is roughly its purpose for deployment. In morphological modeling, form-, function-, and faculty-models have the functions (or purpose) of representing their respective aspects (forms, functions, and faculties) of an organismal feature. Because organismal features and the aspects that comprise them are contingently defined natural kinds, determining the function of a morphological model is an *a posteriori* theoretical exercise performed by a morphologist within the inferential architecture of her disciplinary matrix. In other words, the functions of morphological models reflect the *intentions* of the morphologists that use them, and these intentions are purely theoretical. Furthermore, the processes morphologists use to determine model functions is dependent upon and directly reflects the contributions they make to the epistemic achievements of the disciplinary matrix in which they are deployed.

Directly related to the determination of a model's function is the determination of a model's *selection*. Recall from Chapter 2 that to determine the selection of a model is to determine the particular aspect of a given phenomenon the model shall represent. By selecting a particular aspect of a phenomenon for a model to represent, a morphologist implicitly selects the subset of data from the total available dataset about the phenomenon to interpret and represent. And, this selection procedure is guided solely by nonexperimental criteria such as morphological theories, background knowledge, and other accepted practices and standards within the morphologist's disciplinary matrix. Unfortunately, datasets are not produced by nature with ready-made subdivisions that precisely indicate which data fall into which subsets. So, if a morphologist is to

⁵² These judgments are made for each theoretical model as well as their underlying data and data production models. Due to space constraints, I shall only discuss how these judgments are made to produce theoretical models and how such judgments directly affect the representations of organismal phenomena they provide.

deploy a model to interpret and represent a particular subset of data about an aspect, she must first determine which data belong in that subset. This too is an *a posteriori* theoretical exercise, and the process the morphologist will use to carry it out will be dependent upon the contribution it makes to the epistemic achievements of the disciplinary matrix in which it is deployed.

Second, morphologists must *synthesize* their models. Recall from Chapter 1 that form-, function-, faculty-, and feature-models are made by synthesizing (on the one hand) observational data (or at least the claims made by data models) with (on the other hand) biomechanical and background knowledge by using biomechanical principles and morphological theories. Also recall that I did not provide a detailed account of the synthesis process. This is because the notion of synthesis will be dependent upon the modeling practices within the disciplinary matrix in which models are deployed. That is, synthesis is a process guided by the model building standards and practices morphologists adopt as they work within a disciplinary matrix. There is no *a priori* method of model building. Model building is a purely *a posteriori* and largely theoretical endeavor. This means, as we have already seen in the contexts of function and selection, that the representations provided by models are largely the result of theoretical judgments that are made by reference to the background knowledge, morphological theories, biomechanical principles, and other accepted practices morphologists utilize. Hence, a notion of synthesis will be dependent upon the contribution it makes to the epistemic achievements of the disciplinary matrix in which it is deployed.

Finally, morphologists must determine the model *type* and *expression* of their models for the production of interpretations and representations. Recall from Chapter 1 that a model's type is the kind of model it is (conceptual, visual, or mathematical), and a model's expression is its medium of description or explanation, that is, its form of representation (diagrammatic, graph-

ical, propositional, and virtual). Also recall from Chapter 1 that each model is characterized by only one model type and one model expression; to change either is to change to a different model. The determination of a model's type and expression is crucially important to the representation it will produce and the information it will provide about its selected aspect. If a morphologist is a skilled modeler, then she will be able to produce a representation "worth a 1,000 words." However, if she is a less than able modeler, her representation will be less able to produce the information needed for explanation of the aspect being modeled. A good representation will provide a morphologist with a bounty of information about a phenomenal aspect, which can then be incorporated into better explanations and theories about her phenomenon of interest. However, the production of a good representation is dependent upon her modeling know-how.

Choosing the "right" model type and expression is an *a posteriori* theoretical exercise. That is, there are no *a priori* rules for choosing the right model type and expression in order to ensure the best representational output possible. Rather, morphologists must depend on their background knowledge, their understanding of morphology, and their understanding of modeling to produce a theoretical judgment as to which model type and expression are best for representing a selected aspect. The "correctness" of their choices will be dependent on the contribution they make to the epistemic achievements of the disciplinary matrix morphologists are working within.

2) Theoretical judgments also play an important role in the ability of morphologists to establish or identify the relationships that exist between multiperspectival models. Recall that multiperspectival models are types of over-all models, that is, a group of sub-models embedded within an amalgamated model, that possess the following three relations: focused-based-relation, combinatorial-based-relation, and hierarchical-based-relation. Determining which sub-models share which of these three relations within the over-all model is a purely theoretical exercise.

This means that establishing the coherence of sub-models within an over-all model is dependent upon the ability of a morphologist to detect the appropriate relations between models and upon her ability to rectify inconsistencies between related models.

The imaginative capacity and theoretical knowledge of a morphologist are especially important for establishing multiperspectival models and ensuring their coherence. The relations between currently available models may not be obvious and there may be more than one way to link them together. This has the consequence that the total representational output of the multiperspectival model will have multiply realizable forms. The determination of which representational output is satisfactory for each individual model and for the total multiperspectival model will be that which is theoretically plausible. The representations of the multiperspectival form-, function-, faculty-, and feature-models will be dependent upon judgments of theoretical plausibility by morphologists by reference to their background knowledge, morphological theories, and other accepted nonexperimental criteria.⁵³ And, the theoretical plausibility of a morphologist's judgment is dependent upon the contribution it makes to the epistemic achievements of her disciplinary matrix.

In summary, I have discussed two ways in which morphological models contribute to the instrumental reliability of morphological practices. First, morphological models play a central role in helping morphologists determine the definitions of the natural kinds used within their theories and explanations. The best explanation for why morphological models are able to contribute to the inductive and explanatory successes of morphological theories achieved through the use of natural kinds within disciplinary matrices is that their representational output is ap-

⁵³ This is not to suggest that the knowledge obtained from the deployment of a multiperspectival model will be *perspectival* knowledge. I discussed in Chapter 2 the methods model users employ to cull unnecessary or biased or idealized representational content from the total available representational content. For additional reasons why multiperspectival modeling avoids perspectival knowledge see Chakravartty (2010).

proximately true. Second, the theoretical judgments required for the production of morphological models are based on theoretical beliefs of morphologists about the best way to build models. Therefore, if morphological models contribute to the inductive and explanatory successes of morphology and if morphological models are as theory-laden as I have argued in the sections above, then the best explanation for why morphological modeling is a reliable methodology is that the method of morphological modeling is approximately correct for investigating organismal phenomena. Morphologists get something right when they deploy and employ morphological models within their disciplinary matrices and morphological explanations, respectively, as reflected by the contributions morphological models make to the instrumental reliability of morphological methods. A diagrammatic representation of the accommodation process involving multiperspectival models is presented in Figure 6.

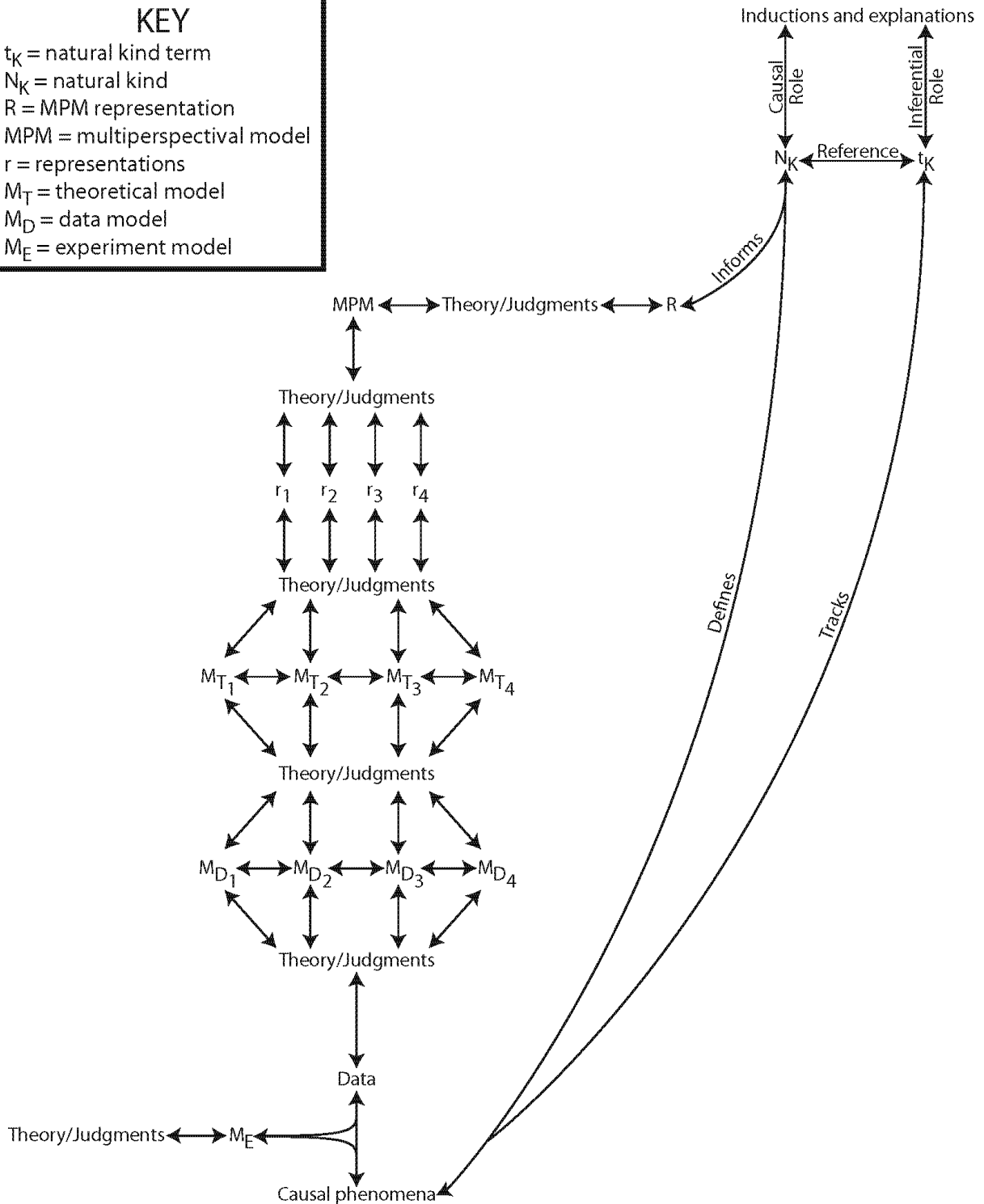
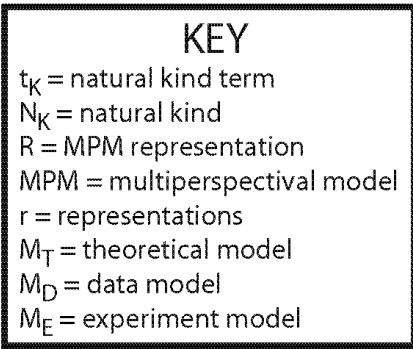


Figure 6: Flowchart combining multiperspectival modeling (MPM) with Boyd's view of reference. MPM tracks causal phenomena to produce the representation, R , which informs the definition of the natural kind, N_K , that is being referred to by the natural kind term, t_K , which is also tracking the same causal phenomena underwriting the definition of N_K and the content of R .

5. OBJECTIONS TO A REALIST VIEW OF MODELING

Good philosophical method dictates that what one says about, for example, scientific methods or scientific language generally should be consistent with what one says about the methods or language of particular sciences, and vice versa. (Boyd, 1991a, p. 3)

In this chapter, I address three objections that could be raised against a scientific realistic view of morphological modeling. The first objection I discuss comes from the empiricist tradition in the philosophy of science. I suggest that empiricists could argue against a realist view of morphological models by adapting their claims about the underdetermination of scientific theories to the underdetermination of morphological models. I shall refer to this objection as the *underdetermination of models argument* (§5.1.). The second objection I discuss comes from the constructivist tradition. I suggest that constructivists could argue against a realist view of models by appealing to paradigm dependent standards of model building and model assessment to support their claim that morphological models are a mere methodological convention deployed to “investigate” a paradigm dependent world. I shall refer to this objection as the *conventionality of models argument* (§5.2.). Lastly, I discuss a worry Barbara Horan (1988) raises against a realist view of biological models based on what she describes as an inverse relationship between the explanatory power and the predictive success of models that results from modeling complex biological phenomena. I shall refer to this objection as the *complexity problem* (§5.3.).

The first two objections are rooted in historical disagreements between realists and antirealists in the philosophy of science. Nevertheless, a realist view of morphological modeling must be able to answer empiricist and constructivist worries as they pertain to modeling if it is to be a viable account of scientific modeling. I shall rely on the contributions Boyd has made to the realist-antirealist debates in the philosophy of science (particularly Boyd (1983, 1985a, 1992)) in

formulating my response to the empiricist and constructivist objections. The third worry represents a constant concern for practicing biologists, especially morphologists, namely to what extent explanations about *specific* biological systems can be used as a source of predictions about a *general* and often unrelated class of biological phenomena. To address this worry, I shall rely on the philosophical work of Boyd as well as Bailer-Jones' notion of *prototype*.

5.1. The underdetermination of models argument

Following Boyd (1983), a simple, yet powerful empiricist argument against a realistic view of morphological modeling can be sketched as follows:

Suppose that M is a proposed multiperspectival model (an over-all model with requisite sub-models) that makes theoretical and observational claims about an organismal feature F , which can be subjected to experimental testing. A multiperspectival model is said to be empirically equivalent to M just in case it makes the same observational predictions about F that M does. Now it is always possible, given M , to construct arbitrarily many alternative multiperspectival models which are empirically equivalent to M but which offer contradictory theoretical accounts of the nature of F . Since scientific evidence for or against a multiperspectival model consists in the confirmation or disconfirmation of one of its observational predictions, M and each of the multiperspectival models empirically equivalent to it will be equally well confirmed or disconfirmed by any possible observational evidence. Therefore, no scientific evidence can bear on the question of which of these multiperspectival models provides the correct theoretical account of F . At best, it might be possible to confirm or disconfirm the claim that each of these multiperspectival models is a reliable instrument for the prediction of observable phenomena. Since this construction is possible for any multiperspectival model M , it follows that scientific evidence can never decide the question between multiperspectival models of organismal features. Hence, theoretical knowledge of the nature of organismal features is impossible.⁵⁴

According to an empiricist view of models, if the representations of two different multiperspectival models differ in theoretical content, yet produce the same observational consequences, then choosing one model over the other will be merely a *pragmatic* matter since empirical evidence will be unable to arbitrate, that is, determine, between the two models. That is, each model will be equally confirmed or disconfirmed by empirical evidence. This inability to empirically arbi-

⁵⁴ This sketch of an empiricist argument against a realist view of morphological models closely follows the sketch Boyd (1983, pp. 46-48) gives of an empiricist argument against a realist view of scientific theories. I have preserved the structure of Boyd's formulation in adapting his sketch of scientific theories to apply to morphological models.

trate between two multiperspectival models is the central thrust of the underdetermination of models argument (as applied to morphological models).

Before providing a realist rebuttal to the empiricist sketch above, I shall briefly note what the realist and empiricist share in common and where they disagree. This will help diagnose the problems with the claims made by the empiricist about the underdetermination of morphological models. First, both realists and empiricists accept the mind-independence of reality. That is, both realists and empiricists affirm the no non-causal contribution thesis (2N2C). Recall, 2N2C states that human social practices make no non-causal contribution to the causal structure of the world or to the properties of causal phenomena inhabiting the world (Boyd, 1992). Second, both realists and empiricists affirm the accommodation thesis, which states that our scientific methods are reliable insofar as they allow the accommodation of our scientific practices to independently existing causal phenomena under study and that the background theories of our sciences reliably govern our scientific methods by providing an approximately accurate account of the observable causal phenomena under study (Boyd, 1992). In other words, empiricists and realists agree that science explains a mind-independent world.

Realists and empiricists disagree on the role theoretical judgments (or claims) play in regards to claims about observational phenomena. Scientific realists (like Boyd) hold that judgments about projectibility, confirmation, experimental setup, and evidential standards are theory-dependent judgments such that they represent “inductive inferences from (partly or wholly) theoretical premises to (partly or wholly) theoretical conclusions” (Boyd, 1992, p. 29). In other words, the realist holds that theoretical judgments are approximately true inferences from approximately true background theories that inform how best to collect data about, explain, and predict observational phenomena. Therefore, theoretical judgments are evidential and play an

important role in the confirmation of models. Empiricists (like Van Fraassen (1980)) disagree. They claim that while background theories may *suggest* new alternatives that can be cashed out using theoretical judgments, there is no reason to believe that these theoretical judgments are approximately true. For the empiricist, theoretical judgments may serve a pragmatic role, but they do not serve an epistemic role. A theoretical judgment may help a morphologist choose between which of two empirically equivalent multiperspectival models to submit to experimental testing, but a theoretical judgment will not help the morphologist determine which model is approximately true. For the empiricist, theoretical judgments do not play a role in the confirmation of morphological models; they hold that that role can only be played by observational evidence.

A response to the empiricist can be given by following Boyd (1983, 1985a): The evidence for or against a multiperspectival model is not just a matter of the accuracy of its (tested) observational predictions. Rather, considerations of its theoretical plausibility in light of currently accepted morphological theories, biomechanical principles, and background knowledge, as well as the theoretical plausibility of alternative multiperspectival models, are just as evidential as confirmatory empirical evidence. Theoretical judgments play an integral role in modeling data production and data interpretation, which directly informs theoretical judgments involved in the construction of form-, function-, faculty-, and feature-models. The representations these models produce are largely based upon theoretical judgments made by the model user about appropriate model type, model expression, and model relations. Furthermore, the explanatory and predictive power of multiperspectival models directly depends on the relations between their sub-models, which can only be established by reference to the theoretical judgments of the model user. Morphologists only adopt those models that are most theoretically plausible. According to Boyd

When a theoretical proposal is theoretically plausible in light of the existing theoretical tradition, what that means is that it is supported by an inductive inference at the theoretical level from previously acquired theoretical knowledge.... The role of experimentation is to choose between the various theoretical proposals which pass this preliminary test for probable (approximate) truth. (Boyd, 1985a, p. 14)

The only way to make sense of the process of morphological modeling, which involves the synthesis of observational data with morphological theories, biomechanical principles, and relevant background knowledge, is to view the necessary involvement of theoretical judgments and claims as evidential; if a model is inadequately constructed, then its observational predictions will be equally inadequate.

Therefore, the underdetermination of models argument does not adequately reflect the evidential role of theoretical judgments as they are used in scientific practices. When presented with two multiperspectival models that are empirically equivalent, a morphologist can determine which model is more theoretically plausible, and therefore more likely to be approximately true, by reference to the model user's theoretical claims and by use of her theoretical judgments. The only way two multiperspectival models can be evidentially indistinguishable when their theoretical claims are taken into account is if they are identical to one another. (See Boyd (1972, 1973, 1976, 1982, 1983, 1985a, 1985b, 1985c, 1989, 1990a, 1991b, 1993, 2010b) for more detailed arguments against the empiricist view of science.)

5.2. The conventionality of models argument

Unlike empiricists, constructivists and scientific realists agree that theoretical judgments are indispensable when evaluating scientific theories or models.⁵⁵ Constructivists and realists also

⁵⁵ Boyd (1992) identifies three versions of constructivism: science-as-social-practice constructivism (SSP constructivism), debunking constructivism, and Neo-Kantian constructivism. SSP constructivism holds that science is fundamentally a social endeavor, that is, science can be politically, sociologically, literally, and anthropologically studied. Debunking constructivism holds that most of the content of scientific theories is determined by power struggles within the scientific community itself. Neo-Kantian constructivism (N-K constructivism) holds that the world scientists study is constructed by paradigm dependent theoretical claims (in the sense of Kuhn (1996) and Hanson (1965)). I am only concerned with N-K constructivism here.

agree that theoretical judgments play an important role in helping our scientific methods track causal phenomena in the world. However, the constructivists emphasize the profound dependence of our scientific methods on theoretical judgments such that they deny the *independent* existence of causal phenomena. For constructivists, the nature of causal phenomena is largely determined by paradigm dependent theoretical judgments or convention. In other words, constructivists reject 2N2C. By (again) following Boyd (1983), we can sketch the constructivist reasoning as applied to morphological modeling as follows:

According to constructivists, the actual methodology of morphological modeling is profoundly theory-dependent. What morphologists count as an acceptable model, what they count as an observation, which experiments they take to be well-designed, which measurement procedures they consider legitimate, what problems they seek to solve, what sorts of evidence they require before accepting a model, ...all of these features of modeling methodology are in practice determined by the theoretical tradition within which morphologists work. What sort of world must there be, the constructivist asks, in order for this sort of theory-dependent modeling methodology to constitute a vehicle for gaining knowledge? The answer, according to the constructivist, is that the biological world which morphologists study must be, in some robust sense, defined or constituted by, or 'constructed' from, the theoretical tradition in which the morphological community in question works. If the world which morphologists study were not partly constituted by their theoretical tradition then there would be no way of explaining why the theory-dependent modeling methods which morphologists use are a way of finding out what's true.⁵⁶

According to the constructivist, the theoretical judgments deployed by morphologists in the confirmation of models reflect currently accepted conventions about experimental testing, evidential standards, and data interpretation, which, while certainly functioning as (paradigm dependent) epistemic claims, do not reflect accommodation to independently existing causal phenomena in the world. For constructivists, the methods of morphological modeling are no less conventional than adopting the conceptual scheme of organismal forms, functions, faculties, and features. This position is what I refer to as the conventionality of models argument.

The constructivist maintains that the only way to make sense of the central role played by theoretical judgments within our morphological methods is to assume that the biological world is

⁵⁶ I have adapted this sketch of a constructivist argument against a realist view of models from a sketch made by Boyd (1983, p. 57) of a constructivist argument against a realist view of scientific theories and methods in general.

largely constructed by the paradigmatic morphological tradition which defines those methods. However, Boyd provides an alternative explanation: “the world might be one in which the laws and theories embodied in our actual theoretical tradition are approximately true” (Boyd, 1983, p. 65). There are two reasons to believe that Boyd’s answer is correct. First, if the constructivist denial of 2N2C is correct, then there should be evidence from the social practices in science to support their claims. That is, we should be able to find instances in science when acceptance of the best theories for theory or model confirmation relied on a rejection of 2N2C. But, this is not the case. According to Boyd,

if people live in worlds whose causal structure is determined noncausally by their local beliefs and practices in the ways contemplated by N-K constructivism, then the laws governing the relations between social practices and other conditions of human life are quite different from what they would be were 2N2C true.

We may reasonably ask, in the light of the best available scientific theories, whether or not, for example, the general causal structures of matter would be different if chemists and physicists engaged in different social practices. The answer is ‘no,’ and the answer would be ‘no’ for any of the alleged cases of social construction appropriate to N-K constructivist philosophical packages. *That* is evidence for 2N2C, or at least (what is enough) against those denials of 2N2C essential to the constructivist’s project. (Boyd, 1992, p. 176)

By denying 2N2C, constructivists reject what is in their own view paradigmatic evidence in favor of 2N2C’s acceptance! A large part of our scientific practices, including morphological modeling, are premised on the truth and acceptance of 2N2C, since to hold otherwise would be (scientifically) just as implausible as thinking “that social practices in gem-cutting noncausally contribute to the determination of crop yields in Missouri” (Boyd, 1992, p. 177). Rather, scientific practices seem to be *dialectically* accommodating to independent causal phenomena in the world in accordance with 2N2C. Only the scientific realist view can explain this process in a way coherent and consistent with actual scientific practices.

The second reason why Boyd’s explanation given above should be accepted is that constructivists are unable to adequately explain the instrumental reliability of morphological modeling.

Remember, Boyd's abductive argument for scientific realism is built upon its supremacy over empiricism and constructivism to explain the instrumental reliability of scientific theories and methods. If constructivism aims to be a serious alternative to scientific realism, then it must be able to offer an equally adequate explanation for the instrumental reliability of our scientific practices, including morphological modeling. However, Boyd maintains that such an explanation is not available to the constructivist. According to Boyd, "the instrumental reliability of particular scientific theories cannot be an artifact of the social construction of reality" because "it cannot be that the explanation for the fact that airplanes, whose design rests upon enormously sophisticated theory, do not often crash is that the paradigm *defines* the concept of an airplane in terms of crash-resistance" (Boyd, 1983, p. 66).

Any philosophical theory that aims to explain the instrumental reliability of scientific theories and methods must also explain why those theories and methods are sometimes unreliable. According to Boyd's view, the best explanation for why our theories and methods are unreliable is that they are imperfectly, yet dialectically, accommodating to independently existing causal phenomena in the world. Hence, the best explanation involves the acceptance of 2N2C. Constructivists cannot explain the instrumental reliability of our sciences because they cannot explain their occasional unreliability. Boyd points out that the *anomalous* observations which (sometimes) lead to "scientific revolutions" in the sense of Kuhn (1996) and the *resistance* that the world exerts on paradigms in the sense of Feyerabend (1989) can only be explained by accepting 2N2C. "Anomalies" and "resistance" are effects of the ongoing dialectical process of accommodation between our scientific practices, including morphological modeling, and independently existing causal phenomena, and it is this practice that best explains the instrumental reliability of our sciences and the occasional unreliability caused by Kuhnian anomalies and

Feyerabendian resistances. Therefore, the conventionality of models objection should be rejected in favor of a realistic view of morphological modeling. (See Boyd (1982, 1983, 1989, 1990b, 1992, 1999b, 1999c, 2001a, 2010b) for more detailed arguments against the constructivist view of science.)

5.3. The complexity problem

Barbara Horan (1988) raises an important worry for a realist view of morphological modeling. Her concern involves what she perceives as an unavoidable antagonism between the predictive success and the explanatory power of models. According to Horan, this antagonism is a consequence of competing aims to (on the one hand) produce a model that makes successful predictions about the class of phenomena in which its focus is a member and (on the other hand) to produce a model that can explain other less similar (and often unrelated) classes of phenomena. She argues that this is problematic for a realist view of models, writing:

The more robust a model becomes, that is, the more it can successfully predict what will be observed in different, more general cases, the less realistic it will be, for the trade-off of generality for detail is greater. That is, the greater the coincidence of results (the greater the predictive success of the model), the less likely it is that the model puts forward what is the actual explanation in each case. Similarly, supposing that explanatory power were the primary goal, the more realistic the model becomes (the more attention it pays to the details of a particular case in order to ascertain the actual causes of the result to be explained), the less robust the model will be. To the extent that the model is sufficiently complex to incorporate enough of the details of a particular case to make probable the identification of the actual causes of the result to be explained, the less able it will be to make predictions about other, different cases, simply because their details will differ. (Horan, 1988, p. 274)

Horan's worries concern morphological modeling for the following reasons: Organismal phenomena are complex and diverse. When modeling a particular focus, a morphologist must decide whether to construct her model to *realistically* represent its focus at the cost of its general applicability to other foci, or to construct her model less realistically to represent other foci at the cost of producing a realistic representation of its focus. In other words, the realism and the

general applicability of models are inversely related; emphasizing the former increases a model's representational adequacy of organismal complexity, whereas emphasizing the latter increases a model's explanatory scope.

There are two ways to respond to Horan's worries. The first way is by reference to the notion of *prototype* as described by Bailer-Jones (2009). According to Bailer-Jones, models are often intended to represent a class of phenomena, say a particular organismal feature shared by members of a species, of which many specimens can be found in the world. Most of these specimens are potential *prototypes*. A prototype, according to Bailer-Jones (2009), is

not any odd specimen of a phenomenon, but a typical one. Often this involves imagining the object of consideration as having 'average' or 'typical' properties, and this 'prototypical' object or phenomenon may not even exist in the real world. The point is that it could typically exist in just this way and that there probably exist many very much like it. So the prototype is selected or 'distilled' from a class of objects. The prototype has all the properties of the real phenomenon; it is merely that the properties are selected such that they do not deviate from a 'typical' case of the phenomenon. It is then this prototype that is addressed in the modeling effort. The assumption behind this process of prototype formation is nonetheless that the model is not only a model of the prototype, although there is a certain amount of deviation from the norm. (Bailer-Jones, 2009, p. 145)

Bailer-Jones maintains that prototypes are just as concrete as any individual specimen because none of the properties belonging to a phenomenon are abstracted away when a prototype is formed.⁵⁷ By modeling a prototypical organismal feature, morphologists avoid issues with the specificity of models to a particular specimen. Modeling prototypical features allows morphologists to successfully predict *and* explain relevantly similar members within a given natural kind Feature_D. The degree to which a given multiperspectival morphological model can apply to other natural kinds that may share some of the same properties is a concern of determining that

⁵⁷ This is not to suggest that a prototype is intended to represent an "ideal" specimen. Rather, there is a "typical" specimen relative to a given class of phenomena. I think Bailer-Jones' point is analogous to a practice found in nursing. Nurses are instructed not to record a patient's blood pressure as "normal" or "abnormal" since there is no such thing as normal human blood pressure. Rather, nurses are instructed record that their patient's blood pressure is either regular or irregular relative to a patient specific base-line. Analogously, prototypes are "typical" relative to a contingently clustering family of properties used to define their kind or class. (I thank Claiborne MacKnight for the information about nursing practices.)

model's explanatory power. And, according to Boyd, "assessments of explanatory power are just one species of assessment of indirect theory-mediated empirical evidence" (Boyd, 1985c, p. 87). Hence, at least for relatively similar phenomena, Horan's worries will be addressed by the theoretical judgments (in the sense of Boyd) of morphologists, who will determine (in the language of Bailer-Jones) what function their models shall fulfill, be it predictive, explanatory, or some combination of both.

Bailer-Jones' notion of prototype will not answer Horan's worries about the use of models that have a high degree of predictive success for one class of phenomena to explain a significantly different and unrelated class of phenomena. However, I think Horan's worries can be addressed by appealing to what Bailer-Jones (2002a, 2009) calls *metaphorical models*. According to Bailer-Jones, we can think of a *metaphor* as "a linguistic expression in which at least one part of the expression is transferred from one domain of application (source domain), where it is common, to another (target domain) in which it is unusual, or was probably unusual at an earlier time when it might have been new" (Bailer-Jones, 2009, p. 111). Scientific models can act as metaphors by organizing information in their source domain, that is, about their focus, to help make connections and to help organize information in a target domain that differs significantly from their focus. In fact, this is just how models are used when they are deployed to represent "model" organisms, which are organisms such as mice that are easy to study, yet provide a wealth of information about the biology of other organisms, such as humans (see Ankeny (2000); Ankeny and Leonelli (2011)). Models of model organisms are types of metaphorical models, and the extent to which they can be used to explain unrelated organismal phenomena will be dependent upon the theoretical judgments of morphologists.

Boyd (1993) offers an additional way models can be thought of as metaphorical when applied to unrelated phenomena. He claims that scientific metaphors help address the problem of *catachresis* by introducing theoretical terms in domains where none previously existed. Nevertheless, Boyd claims that “the use of metaphor is one of many devices available to the scientific community to accomplish the task of *accommodation of language to the causal structure of the world* (Boyd, 1993, p. 483). In similar fashion, metaphorical models introduce theoretical information into a target (and phenomenologically unrelated) domain where little to none existed previously. The appropriateness for the use of one model’s representation as a metaphorical representation of another phenomenon will be determined by the theoretical judgments of the model user. Like metaphorical language, metaphorical models are one of many devices scientists use to accommodate their practices to the world.

In summary, morphological models can be defended against the objections of empiricists and constructivists by appealing to the arguments in favor of scientific realism developed by Boyd. This has the consequence that the realist view of morphological models I have developed will be dependent upon the realist view of science Boyd has developed; if Boyd’s arguments for scientific realism fail, then my arguments for a realist view of morphological modeling also fail. Other objections to a realist view of models are available, but responding to them involves an analysis of the relationship between morphological models (and models similar to them) and the semantic view of scientific theories. Limited space prevents me from responding to those objections here. And, while Horan’s concerns are important for practicing biologists, her philosophical worries are easily addressed by reference to Boyd’s accommodation thesis: biological methods, whether predominantly predictive or explanatory, will be assessed by how well they con-

tribute to the satisfaction of the accommodation demands of the disciplinary matrices in which they are employed.

CONCLUSION

I have attempted to provide a positive account for a realist view of scientific modeling using the methods of morphological modeling. While I think my account can be used to address antirealist conceptions of scientific models, doing so would require a thorough analysis of the variation in antirealist notions of models, which range from the notion of models employed in the semantic view of scientific theories to the notion of models as autonomous agents. Like each version of antirealism in the philosophy of science, such as constructivism and empiricism, each antirealist notion of scientific models will demand a unique realist response, and therefore cannot be treated in full.

I have also restricted my treatment of scientific modeling as it is deployed in biological morphology, but I think my views of morphological modeling can be easily adapted to other scientific disciplines ranging from quantum mechanics to psychology. If I am right, then scientific modeling can be incorporated within a realist view of science across disciplines (although the details require a discipline specific analysis). This would be a significant achievement and would anticipate a realist interpretation of peripherally situated scientific disciplines that utilize modeling as a primary method for investigation and understanding such as architecture and history.

Within biological morphology, models play an important role not only in investigating and explaining extant organisms, but also in investigating and explaining their evolution. A likely next step in my investigation of morphological modeling will be to explore the use of multiperspectival models for the reconstruction of evolutionary history, which involves the amalgamation of models representing both extant and fossilized organismal features across taxa. The estab-

lishment of evolutionary relationships has been a principle endeavor of biology since Darwin, and I think multiperspectival modeling has played an integral role in this undertaking.

REFERENCES

- Achinstein, P. (1968). *Concepts of Science: A Philosophical Analysis*. Baltimore, MD: The Johns Hopkins Press.
- Ankeny, R. A. (2000). Fashioning descriptive models in biology: of worms and wiring diagrams. *Philosophy of Science*, 67, S260-S272.
- Ankeny, R. A., & Leonelli, S. (2011). What's so special about model organisms? *Studies in History and Philosophy of Science*, 42, 313-323.
- Ayer, A. J. (Ed.). (1959). *Logical Positivism*. New York: The Free Press.
- Bailer-Jones, D. M. (1999). Creative strategies employed in modelling : A case study. *Foundations of Science*, 4, 375-388.
- Bailer-Jones, D. M. (2000). Modelling extended extragalactic radio sources. *Studies in History and Philosophy of Modern Physics*, 31(1), 49-74.
- Bailer-Jones, D. M. (2002a). Models, metaphors and analogies. In P. Machamer & M. Silberstein (Eds.), *The Blackwell Guide to the Philosophy of Science* (pp. 108-127): Blackwell.
- Bailer-Jones, D. M. (2002b). Scientists' thoughts on scientific models. *Perspectives on Science*, 10(3), 275-301.
- Bailer-Jones, D. M. (2003). When scientific models represent. *International Studies in the Philosophy of Science*, 17(1), 59-74.
- Bailer-Jones, D. M. (2005). Models, theories and phenomena. In P. Hajek, L. Valdes-Villanueva & D. Westerstahl (Eds.), *Logic, Methodology and Philosophy of Science: Proceedings of the Twelfth International Congress [2003]* (pp. 243-255). London: King's College Publications.
- Bailer-Jones, D. M. (2009). *Scientific Models in Philosophy of Science*. Pittsburgh, P.A.: University of Pittsburgh Press.
- Bailer-Jones, D. M., & Bailer-Jones, C. A. L. (2002). Modelling data: Analogies in neural networks, simulated annealing and genetic algorithms. In L. Magnani & N. Nersessian (Eds.), *Model-Based Reasoning: Science, Technology, Values* (pp. 147-166). New York: Kluwer Academic/Plenum Publishers.
- Black, M. (1962). *Models and Metaphors*. Ithaca, New York: Cornell University Press.
- Bock, W. J. (1989). Organisms as functional machines: a connectivity explanation. *American Zoologist*, 29, 1119-1132.

- Bock, W. J. (2000). Explanations in a historical science. *Proceedings of the 7th International Senckenberg Conference, Organisms, Genes, and Evolution*(Evolutionary Theory at the Crossroads).
- Bock, W. J. (2004). Explanations in systematics. In D. M. Williams & P. L. Forey (Eds.), *Milestones in Systematics* (Vol. 67): CRC Press.
- Bock, W. J. (2007). Explanations in evolutionary theory. *Journal of Zoology, Systematics, and Evolution Research*, 45(2), 89-103.
- Bock, W. J. (2010). Multiple explanations in Darwinian evolutionary theory. *Acta Biotheor*, 58, 65-79.
- Bock, W. J., & von Wahlert, G. (1965). Adaptation and the form-function complex. *Evolution*, 19(3), 269-299.
- Bogen, J. (2011). 'Saving the phenomena' and saving the phenomena. *Synthese*, 182, 7-22.
- Bogen, J., & Woodward, J. (1988). Saving the phenomena. *The Philosophical Review*, 97(3), 303-352.
- Bogen, J., & Woodward, J. (1992). Observations, theories and the evolution of the human spirit. *Philosophy of Science*, 59, 590-611.
- Bogen, J., & Woodward, J. (2005). Evading the IRS. In M. R. Jones & N. Cartwright (Eds.), *Idealization XII: Correcting the Model. Idealization and Abstraction in the Sciences (Poznań Studies in the Philosophy of the Sciences and the Humanities)* (Vol. 86, pp. 233-267). Amsterdam/New York: Rodopi.
- Botha, M. E. (1986). Metaphorical models and scientific realism. *PSA*, 1, 374-383.
- Boyd, R. N. (1972). Determinism, laws, and predictability in principle. *Philosophy of Science*, 39(4), 431-450.
- Boyd, R. N. (1973). Realism, underdetermination, and a causal theory of evidence. *Nous*, 7(1), 1-12.
- Boyd, R. N. (1976). Approximate truth and natural necessity. *The Journal of Philosophy*, 73(18), 633-635. doi: 10.2307/2025819
- Boyd, R. N. (1982). Scientific realism and naturalistic epistemology. *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association, 1980*, 613-662. doi: 10.2307/192615
- Boyd, R. N. (1983). On the current status of the issue of scientific realism. *Erkenntnis*, 19, 45-90.
- Boyd, R. N. (1985a). Lex orendi est lex credendi. In P. M. Churchland & C. A. Hooker (Eds.), *Images of Science* (pp. 3-34). Chicago: University of Chicago Press.

- Boyd, R. N. (1985b). The logician's dilemma: deductive logic, inductive inference and logical empiricism. *Erkenntnis*, 22, 197-252.
- Boyd, R. N. (1985c). Observations, explanatory power, and simplicity. In P. Achinstein & O. Hannaway (Eds.), *Observation, Experiment, and Hypothesis in Modern Physical Science*. Cambridge, MA: MIT Press.
- Boyd, R. N. (1988). How to be a moral realist. In G. Sayre-McCord (Ed.), *Moral Realism* (pp. 297-356): Cornell University Press.
- Boyd, R. N. (1989). What realism implies and what it does not. *Dialectica*, 43(1-2), 5-29.
- Boyd, R. N. (1990a). Realism, approximate truth, and philosophical method. In W. Savage (Ed.), *Scientific Theories* (Vol. 14, pp. 355-391). Minneapolis: University of Minnesota Press.
- Boyd, R. N. (1990b). Realism, conventionality, and "realism about". In G. Boolos (Ed.), *Festschrift for Hilary Putnam* (pp. 171-195). Cambridge: Cambridge University Press.
- Boyd, R. N. (1991a). Introductory Essay to Section I: Confirmation, Semantics, and the Interpretation of Scientific Theories. In R. N. Boyd, P. Gasper & J. D. Trout (Eds.), *The Philosophy of Science* (pp. 3-36). Cambridge, MA: The MIT Press.
- Boyd, R. N. (1991b). Realism, anti-foundationalism and the enthusiasm for natural kinds. *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition*, 61(1), 127-148.
- Boyd, R. N. (1992). Constructivism, Realism, and Philosophical Method. In J. Earman (Ed.), *Inference, Explanation and Other Philosophical Frustrations*. Berkeley: University of California Press.
- Boyd, R. N. (1993). Metaphor and theory change: What is "metaphor" a metaphor for? In A. Ortony (Ed.), *Metaphor and Thought* (2 ed., pp. 481-532). Cambridge: Cambridge University Press.
- Boyd, R. N. (1999a). Homeostasis, species, and higher taxa. In R. A. Wilson (Ed.), *Species: New Interdisciplinary Essays* (pp. 141-186). Cambridge, MA: MIT Press.
- Boyd, R. N. (1999b). Kinds as the 'Workmanship of Men': Realism, Constructivism, and Natural Kinds (S. f. A. Philosophy, Trans.). In J. Nida-Rümelin (Ed.), *Rationalität, Realismus, Revision: Proceedings of the Third International Congress, Gesellschaft für Analytische Philosophie* (pp. 52-89). Berlin: de Gruyter.
- Boyd, R. N. (1999c). Kinds, complexity and multiple realization: comments on Millikan's "Historical kinds and the special sciences". *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition*, 95(1/2), 67-98.

- Boyd, R. N. (2001a). Reference, (in)commensurability and meanings. In P. Hoyningen-Huene & H. Sankey (Eds.), *Incommensurability and Related Matters* (pp. 1-63). Netherlands: Kluwer Academic Publishers.
- Boyd, R. N. (2001b). Truth through thick and thin. In R. Schantz (Ed.), *What Is Truth?* (pp. 38-59). Berlin and New York: Walter de Gruyter.
- Boyd, R. N. (2003). Finite beings, finite goods: the semantics, metaphysics and ethics of naturalist consequentialism, Part 1. *Philosophy and Phenomenological Research*, 66(3), 505-553.
- Boyd, R. N. (2010a). Homeostasis, higher taxa, and monophyly. *Philosophy of Science*, 77(5), 686-701.
- Boyd, R. N. (2010b). Realism, natural kinds, and philosophical methods. In H. BeeBee & N. Sabbarton-Leary (Eds.), *The Semantics and Metaphysics of Natural Kinds* (pp. 212-234): Routledge.
- Boyd, R. N. (2013). What of pragmatism with the world here? In M. Baghramian (Ed.), *Reading Putnam* (pp. 39-94). London: Routledge.
- Brainerd, E. L., & Owerkowicz, T. (2006). Functional morphology and evolution of aspiration breathing in tetrapods. *Respiratory Physiology and Neurobiology*, 154, 73-88.
- Cartwright, N., & Le Poidevin, R. (1991). Fables and models. *Proceedings of the Aristotelian Society, Supplementary Volumes*, 65, 55-82.
- Chakravartty, A. (2010). Perspectivism, inconsistent models, and contrastive explanation. *Studies in History and Philosophy of Science*, 41, 405-412.
- Chin, C. (2011). Models as interpreters (with a case study from pain science). *Studies in History and Philosophy of Science*, 42, 303-312.
- Darwin, C. (2004). *The Origin of Species by Means of Natural Selection*. New York: Barnes and Noble Books.
- De Regt, H. W. (2005). Scientific realism in action: molecular models and Boltzmann's Bildtheorie. *Erkenntnis*, 63, 205-230.
- Eldredge, N., & Gould, S. J. (1972). Punctuated equilibria: an alternative to phyletic gradualism. In T. J. M. Schopf (Ed.), *Models in Paleobiology* (pp. 82-115). San Francisco: Freeman, Cooper & Co.
- Feyerabend, P. (1989). Realism and the historicity of knowledge. *Journal of Philosophy*, 86(8), 393-406.
- Field, H. (1973). Theory change and the indeterminacy of reference. *Journal of Philosophy*, 70, 462-481.

- Field, H. (1974). Tarski's theory of truth. *Journal of Philosophy*, 69, 347-375.
- Fung, Y. C. (1990). *Biomechanics: Motion, Flow, Stress, and Growth*. New York: Springer-Verlag.
- Gans, C. (1974). *Biomechanics: An Approach to Vertebrate Biology*. Philadelphia: J.B. Lippincott Company.
- Gemci, T., Ponyavin, V., Chen, Y., Chen, H., & Collins, R. (2008). Computational model of airflow in upper 17 generations of human respiratory tract. *Journal of Biomechanics*, 41, 2047-2054.
- Giere, R. N. (1988). *Explaining Science: A Cognitive Approach*. Chicago: The University of Chicago Press.
- Goodman, N. (1983). *Fact, Fiction, and Forecast* (4 ed.). Cambridge, MA: Harvard University Press.
- Gould, S. J., & Lewontin, R. C. (1979). The spandrels of San Marco and the Panglossian paradigm: a critique of the adaptationist programme. *Proceedings of the Royal Society in London B*, 205, 581-598.
- Greaves, W. S. (1995). Functional predictions from theoretical models of the skull and jaws in reptiles and mammals. In J. J. Thomason (Ed.), *Functional Morphology in Vertebrate Paleontology* (pp. 99-115). Cambridge: Cambridge University Press.
- Gutmann, W. F. (1988). The hydraulic principle. *American Zoologist*, 28, 257-266.
- Hacking, I. (1983). *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*. Cambridge: Cambridge University Press.
- Ham, K. (1982). The Fossils Say What? *Creation Magazine*, 4, 33-38.
- Hanson, N. R. (1965). *Patterns of Discovery*. Cambridge: Cambridge University Press.
- Hardy, A. C. (1956). *The Open Sea: The World of Plankton*. Boston, M.A.: Houghton Mifflin Company.
- Harré, R. (1960). Metaphor, model and mechanism. *Proceedings of the Aristotelian Society*, 60, 101-122.
- Harré, R. (1970). *The Principles of Scientific Thinking*. London: Macmillan.
- Herring, S. W. (1988). Introduction: how to do functional morphology. *American Zoologist*, 28, 189-192.
- Hesse, M. B. (1953). Models in physics. *The British Journal for the Philosophy of Science*, 4(15), 198-214.

- Hesse, M. B. (1966). *Models and Analogies in Science*. Notre Dame, IN: University of Notre Dame Press.
- Homberger, D. G. (1986). *The lingual apparatus of the African Grey Parrot, Psittacus erithacus Linne (Aves: Psittacidae): Description and theoretical mechanical analysis*. Ornithological Monographs, (39).
- Homberger, D. G. (1988). Models and tests in functional morphology: the significance of description and integration. *American Zoologist*, 28, 217-229.
- Homberger, D. G. (2001). The Case of the Cockatoo Bill, Horse Hoof, Rhinoceros Horn, Whale Baleen, and Turkey Beard: The integument as a model system to explore the concepts of homology and non-homology. In H. M. Dutta & J. S. D. Munshi (Eds.), *Vertebrate Functional Morphology: Horizon of Research in teh 21st Century* (pp. 315-341). Enfield, NH: Science Publishers, Inc.
- Horan, B. L. (1988). Theoretical models, biological complexity and the semantic view of theories. *Philosophy of Science Association* 2, 265-277.
- Hume, D. (2005). *A Treatise of Human Nature*. Oxford: Oxford University Press.
- Hume, D. (2007). *An Enquiry Concerning Human Understanding and Other Writings*. Cambridge: Cambridge University Press.
- Hutten, E. H. (1954). The role of models in physics. *The British Journal for the Philosophy of Science*, 4(16), 284-301.
- Kant, I. (2009). *Critique of Pure Reason* (P. Guyer & A. W. Wood, Trans.). Cambridge: Cambridge University Press.
- Kardong, K. (2009). *Vertebrates: Comparative Anatomy, Function, Evolution* (5 ed.). Boston: McGraw Hill.
- Kripke, S. (1981). *Naming and Necessity*. Malden, MA: Blackwell Publishing.
- Kuhn, T. S. (1996). *The Structure of Scientific Revolutions* (3 ed.). Chicago: University of Chicago Press.
- Lamarck, J. (1963). *Zoological Philosophy: An Exposition with regard to the Natural History of Animals* (H. Elliot, Trans.). New York: Hafner Publishing Company.
- Lauder, G. V. (1995). On the inference of function from structure. In J. J. Thomason (Ed.), *Functional Morphology in Vertebrate Paleontology* (pp. 1-18). Cambridge: Cambridge University Press.
- Liem, K. F. (1988). Form and function of lungs: the evolution of air breathing mechanisms *American Zoologist*, 28, 739-759.

- Locke, J. (1996). *An Essay Concerning Human Understanding*. Indianapolis, IN: Hackett Publishing Company, Inc.
- Love, A. C. (2008). From philosophy to science (to natural philosophy): evolutionary developmental perspectives. *The Quarterly Review of Biology*, 83(1), 65-76.
- Love, A. C. (2010). Idealization in evolutionary developmental investigation: a tension between phenotypic plasticity and normal stages. *Philosophical Transactions of the Royal Society B*, 365, 679-690.
- Maxwell, G. (1962). The Ontological Status of Theoretical Entities. In H. Feigl & G. Maxwell (Eds.), *Scientific Explanation, Space, and Time* (Vol. 3, pp. 3-15). Minneapolis: Minnesota Press.
- Mayer, E. (1963). *Introduction to Dynamic Morphology*. New York: Academic Press.
- Mayr, E. (1982). *The Growth of Biological Thought: Diversity, Evolution, and Inheritance*. Cambridge, Mass.: The Belknap Press of Harvard University Press.
- Mayr, E. (1988). *Towards a New Philosophy of Biology: Observations of an Evolutionist*. Cambridge, MA: Harvard University Press.
- McAllister, J. W. (1997). Phenomena and patterns in data sets. *Erkenntnis*, 47, 217-228.
- McMullin, E. (1985). Galilean idealization. *Studies in History and Philosophy of Science*, 16, 247-273.
- Morrison, M. C. (1998). Modelling nature: between physics and the physical world. *Philosophia Naturalis*, 35, 65-85.
- Morrison, M. C. (1999). Models as autonomous agents. In M. S. Morgan & M. Morrison (Eds.), *Models as Mediators: Perspectives on Natural and Social Science* (pp. 38-65). Cambridge: Cambridge University Press.
- Morrison, M. C., & Morgan, M. S. (1999). Models as mediating instruments. In M. S. Morgan & M. Morrison (Eds.), *Models as Mediators: Perspectives on Natural and Social Science* (pp. 10-38). Cambridge: Cambridge University Press.
- Murad, M. H. S. b. A. (2011). Models, scientific realism, the intelligibility of nature, and their cultural significance. *Studies in History and Philosophy of Science*, 42, 253-261.
- Nagel, E. (1961). *The Structure of Science: Problems in the Logic of Scientific Explanation*. New York: Harcourt, Brace and World, Inc.
- Nakayama, A., Kuwahara, F., & Sano, Y. (2009). Why do we have a bronchial tree with 23 levels of bifurcation? *Heat and Mass Transfer*, 45, 351-354.
- Popper, K. (1963). *Conjectures and Refutations*. London: Routledge

- Portides, D. P. (2005). A theory of scientific model construction: the conceptual process of abstraction and concretisation. *Foundations of Science*, 10, 67-88.
- Portides, D. P. (2011). Seeking representations of phenomena: Phenomenological models. *Studies in History and Philosophy of Science*, 42, 334-341.
- Psillos, S. (1999). *Scientific Realism: How Science Tracks Truth*: Routledge.
- Psillos, S. (2000). The Present State of the Scientific Realism Debate *The British Journal for the Philosophy of Science*, 51, 705-728.
- Putnam, H. (1972). Explanation and Reference. In G. Pearce & P. Maynard (Eds.), *Conceptual Change*. Dordrecht: Reidel.
- Putnam, H. (1973). Meaning and reference. *The Journal of Philosophy*, 70(19), 699-711.
- Quine, W. V. O. (1969). Natural Kinds. In W. V. O. Quine (Ed.), *Ontological Relativity and Other Essays*. New York: Columbia University Press.
- Sanders, R. K., & Farmer, C. G. (2012). The pulmonary anatomy of *Alligator mississippiensis* and its similarity to the avian respiratory system. *The Anatomical Record*, 295, 699-714.
- Schmidt, A., Zidowitz, S., Kriete, A., Denhard, T., Krass, S., & Peitgen, H. (2004). A digital reference model of the human bronchial tree. *Computerized Medical Imaging and Graphics*, 28, 203-211.
- Shapere, D. (1969). Notes Toward a Post-Positivistic Interpretation of Science. In P. Achinstein & S. F. Barker (Eds.), *The Legacy of Logical Positivism* (pp. 115-160). Baltimore: The Johns Hopkins Press.
- Singer, M., & Morrison, P. R. (1948). The influence of pH, dye, and salt concentration on the dye binding of modified and unmodified fibrin. *Journal of Biological Chemistry*, 175(1), 133-145.
- Smart, J. J. C. (1963). *Philosophy and Scientific Realism*. London: Routledge and Kegan Paul.
- Spencer, H. (1899). The Factors of Organic Evolution *Essays: Scientific, Political, and Speculative* (Vol. 1, pp. 389-466). New York: D. Appleton and Company.
- Standring, S. (Ed.). (2008). *Gray's Anatomy: The Anatomical Basis of Clinical Practice* (40 ed.): Churchill Livingstone Elsevier.
- Suarez, M. (1999). The role of models in the application of scientific theories: epistemological implications. In M. S. Morgan & M. Morrison (Eds.), *Models as Mediators: Perspectives on Natural and Social Science* (pp. 168-196). Cambridge: Cambridge University Press.
- Suppe, F. (1972). What's wrong with the received view on the structure of scientific theories? *Philosophy of Science*, 39(1), 1-19.

- Suppe, F. (1977). The search for philosophic understanding of scientific theories. In F. Suppe (Ed.), *The Structure of Scientific Theories* (2nd ed., pp. 3-15). Chicago: University of Illinois Press.
- Suppe, F. (2000). Understanding scientific theories: An assessment of developments, 1969-1998. In D. Howard (Ed.), *PSA 1998, Philosophy of Science Supplement 67* (pp. S102-S115).
- Suppes, P. (1960). A comparison of the meaning and uses of models in mathematics and the empirical sciences. *Synthese*, 12, 287-300.
- Tarski, A. (1935). Der Wahrheitsbegriff in den formalisierten Sprachen. *Studia Philosophica*, I, 261-405.
- Van Fraassen, B. (1980). *The Scientific Image*. Oxford: Clarendon Press.
- Wilson, E. O. (1975). *Sociobiology: The New Synthesis*. Cambridge, MA: Harvard University Press.
- Wimsatt, W. C. (1987). False models as means to truer theories. In M. H. Nitecki & A. Hoffman (Eds.), *Neutral Models in Biology* (pp. 23-55). New York: Oxford University Press.
- Woodward, J. (1989). Data and phenomena. *Synthese*, 79, 393-472.
- Woodward, J. (2000). Data, phenomena, and reliability. *Philosophy of Science*, 67 (*Proceedings*), S163-S179.

VITA

Bradley Morgan Wood, a native of Zachary, Louisiana, received his B.A. in Philosophy and his B.S. in Biology at Louisiana State University in 2011. Due to his interests in philosophy of science and philosophy of biology, he decided to enter graduate school in the Department of Philosophy and Religious Studies at Louisiana State University. He will receive his M.A. in Philosophy in May 2013 and plans to begin work on a Ph.D. in comparative and evolutionary anatomy upon graduation.