

Scientific Practice and Analogical Reasoning:  
The Problem of Ingrained Analogy

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**Abstract**

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From metaphors that direct scientific inquiry to analogical inferences that justify particular hypotheses or models, analogical reasoning plays numerous roles in scientific practice.

While in some cases the analogy is explicit and carefully controlled, scientific communities are often not aware of the analogies that inform their research. More importantly, when scientific communities take the analogical correspondences presupposed by a metaphor for granted, a practice I refer to as the problem of ingrained analogy, the uncritical use of

analogy becomes dangerous. For example, the *race as species* metaphor that guided 19<sup>th</sup> century race science endorsed a research program that promoted a hierarchy of race, and

ultimately resulted in social policies that marginalized large segments of American society. A similar worry is linked with the metaphor, prenatal hormone as an organizer of the human brain, which motivates brain organization research. In both cases, reasoning from analogy

has the potential to misdirect research programs. Not only does it hinder or sometimes prevent the acquisition of knowledge, it can result in social policies that have profound, and

long-lasting, negative effects. In philosophy of science, the ambivalence toward analogical reasoning has taken the form of an unhelpful denial, which fails to recognize the constructive, and often invaluable, role that analogy plays. Thus, the challenge for any account of analogical reasoning is to recognize its contributions while also articulating normative guidelines that bring ingrained analogies to the surface and subject them to systematic, critical appraisal. In my dissertation, I argue that communities must adopt strategies that encourage an awareness of the assumptions associated with their use of analogy and propose strategies for critical assessment, drawing from feminist theory and social epistemology. By employing such strategies, scientific communities can not only avoid being misled by analogical reasoning, but by instituting practices that limit harm, scientific communities also become more socially responsible.



For my family

Tempunvyakvn ke tat vpeyet tos

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## Introduction

In *Darwin and His Critics*, David Hull describes Charles Darwin as having “both the good fortune and the misfortune to begin his scientific career at that moment in history when philosophy of science came into its own in England” (Hull 1973, 3). Imagine, a young scientist conducting research and developing a major treatise like the *Origin* amidst the discussions and debates on the methods of science and the nature of induction. Such was the case for Darwin; he developed his theory while witnessing the debate on induction between John Stuart Mill and William Whewell, as well as having John Herschel’s *Preliminary Discourse on the Study of Natural Philosophy* serve as a methodological exemplar. Darwinian scholars—namely Michael Ruse, C. Kenneth Waters, and Jonathan Hodge—note that Darwin wrote the *Origin* under the influence of Whewell and/or Herschel, as well as other works such as Humboldt’s *Personal Narrative* and George Lyell’s *Principles of Geology*.<sup>1</sup> So influential was the philosophical environment in which Darwin wrote the *Origin* that Gregory Radick concludes, “Darwin’s theory of natural selection was no gift of sheer, sublime, solitary genius, but in several key respects a product of Victorian culture” (Radick 2005, 147).

Interestingly, this auspicious state of affairs was also Darwin’s misfortune. As Hull notes, “Darwin braced himself for the potential hostility with which the *content* of his theory was received, especially given its implications for human evolution, but he was not prepared for the criticism against his *methodology*, which came from the most respected philosophers and scientists of his day” (Hull 1973, 6-7).<sup>2</sup>

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<sup>1</sup> See Ruse (1973, 1975), Waters (2003), and Hodge (1992). Hodge no longer maintains that Whewell influenced Darwin to the extent that Ruse and Waters do (Hodge, forthcoming manuscript).

<sup>2</sup> Emphasis in the original.

Of those responding negatively to Darwin's work, Hull states

Some of the reviewers were obviously biased. Some were merely mouthing undigested platitudes. But many of the reviewers were competent scientists honestly trying to evaluate a novel theory against the commonly accepted standards of scientific excellence and evolutionary theory consistently came up wanting.

(Hull 1973, 14)

Although Herschel, Mill, and Whewell did not formally review the *Origin*, their negative responses to Darwin's methods, particularly those provided by Mill and Herschel, surprised and even disappointed Darwin and his supporters (Hull 2003, 181 & 186).

While Darwin found the critiques discouraging, I disagree with Hull that it was a "misfortune". True, Darwin believed his theory of evolution was "grossly misrepresented, bitterly opposed and ridiculed;" yet, he also acknowledged that those same critiques were often tendered "in good faith" (Darwin 1896, 89). Rather, I suggest that the critiques issued against the *Origin* were not a bane, but were instead a boon. The objections tendered by Darwin's critics, while vexing, contributed to an atmosphere of robust scrutiny, which challenged the basic assumptions underwriting the metaphor at the heart of Darwin's theory. Through his attempts to address the objections of his critics, Darwin gained confidence in his particular use of analogical reasoning, in spite of its ability to mislead.<sup>3</sup>

In this dissertation, I examine the use of analogical reasoning in science, notably the relation between metaphor and analogy. In spite of the eventual success of Darwin's use of analogical reasoning, I demonstrate how being misled by a metaphor remains a legitimate worry for scientific communities. In addition, I argue that scientific communities must continually challenge the assumptions underwriting metaphorically driven science.

Metaphors guide research programs: they provide theoretical insights, inspire experiments, and even configure the evidence on which we rely to test and assess its epistemic promise.

Yet, as we shall see, the sets of similarities and differences the domain of a metaphor (often

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<sup>3</sup> Darwin notes in the last chapter of the *Origin* that analogy may be "a deceitful guide" (Darwin 1859, 483-484).

referred to as analogies) can mislead scientists, sometimes with detrimental epistemic and social effects.<sup>4</sup>

Scientific communities that subject their metaphors to robust scrutiny are more likely to mitigate, or perhaps prevent, a danger associated with analogical reasoning. This danger, which I refer to as the problem of ingrained analogy, occurs when a scientific community treats the metaphorical language of analogical reasoning as fact.<sup>5</sup> For example, a metaphor may be so commonplace within a community that the correspondences of similarities and differences associated with it are viewed as literal facts of nature.<sup>6</sup> In order to deal with the problem of ingrained analogy, I consider strategies that cultivate a critical environment within scientific communities, even when these communities lack the resources to develop a fully robust critique on their own. Creating such an environment offers the best hope for preventing communities from being misled by an ingrained analogy.

What exactly does the problem of ingrained analogy have to do with the community that witnessed the publication of the *Origin*? I believe some features of this particular community suggest a solution for dealing with the problem of ingrained analogy. Metaphors are a social product, and they are created in light of the epistemic needs of a historically situated community.<sup>7</sup> To understand a metaphor, one must have access to the language in which it developed; this includes a working knowledge of the “system of associated commonplaces” of the source and target (Black 1962, 40).<sup>8</sup> Taking Darwin’s community—

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<sup>4</sup> The terms positive and negative analogies, which refer to the sets of similarities and differences between two or more analogues, are used in Hesse (1966) and originate with Keynes (1921).

<sup>5</sup> Stepan describes how a metaphor can mislead scientists, using 19<sup>th</sup> century science on human difference as an example. In this dissertation, I explain in detail the problem described by Stepan, calling attention to how it results from the uncritical use of analogical reasoning within scientific communities. I chose the term “ingrained” to signify that a similarity or difference suggested by a metaphor is assumed as fact, and its veracity is not challenged by either the individual or community. That is to say, the analogy (either positive or negative) becomes mentally ingrained.

<sup>6</sup> As we shall see, Colin Turbayne refers to this error as an “insinuation of metaphysics” (Turbayne 1971, 4).

<sup>7</sup> For example, Kay (2000) notes that the selection and use of analogies underwriting the concept of “genetic code” were products of patronage, the backgrounds of community members, the research in other disciplines, and even the ideology of the cold war. See especially, Kay (2000) Chapters Three and Four.

<sup>8</sup> For Max Black, it is not necessary to understand the precise definition of the source/target. Instead, to successfully understand a metaphor, one need only be familiar with the way the particular words are used in one’s society (Black

one capable of a robust critique of its metaphors—as inspiration, I evaluate, and ultimately refine, Helen Longino’s account of transformative criticism in order to develop strategies for scientific communities.

Scientific communities are not necessarily aware of the implicit influence of their metaphors. Nancy Leys Stepan identifies the influence of metaphor as a possible cause of the problems resulting from 19<sup>th</sup> century race science. One metaphor, Dr. Samuel George Morton’s *race as a distinct species*, motivated a research program that promoted a hierarchy of race, and ultimately resulted in social policies that marginalized large segments of American society. Given the negative consequences (both epistemic and social) that result from a science misled by a metaphor, I argue that communities must adopt strategies that encourage an awareness that the assumptions associated with metaphors should not be taken literally.<sup>9</sup> In other words, once communities recognize the dangers of relying on metaphors, they are obliged to ensure that their practice includes methods that cultivate a critical environment and prevents potential harm to others.

In order to appreciate how profoundly an ingrained analogy can affect scientific practice, I begin Chapter One with an analysis of analogical reasoning, discussing its various uses within scientific practice.<sup>10</sup> Throughout my analysis, I provide the terminology used throughout the rest of the work, as well as presenting an initial sketch of scientific practice.<sup>11</sup> To be of general use, I seek a definition of practice that applies to a broad range of disciplines

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1962, 40). Black believes that the common usage of the analogues must be “readily and freely evoked” (Black 1962, 40). Thus, a metaphor in one community will not make sense in another community (Black 1962, 40).

<sup>9</sup> I recognize that for some accounts of science, such as Kuhn’s account of normal science, scientists must have faith in their paradigm, and that faith often keeps a stalled research program going. The point that I wish to make is that scientists must exercise care to prevent their metaphorical speech from being conflated with reality. Thanks to Paul Bartha for encouraging me to make this point more clear.

<sup>10</sup> Keith Holyoak and Paul Thagard discuss the uses of analogical reasoning in their 1995 book, *Mental Leaps*.

<sup>11</sup> For example, I distinguish between analogy (an initial comparison) and the analogies underwriting a metaphor. As we shall see, the terms used to describe different instances of analogical reasoning have been conflated, and as above, the same term is used to denote very different things. This has resulted in some confusion, which should be addressed at the outset of this project.

and assumes theoretical science figures into practice as well.<sup>12</sup> After discussing in general terms analogical reasoning, I relate it to scientific practice by providing examples that illustrate the ways in which analogy can be seen to function in different roles throughout practice. For example, a form of analogical reasoning may function as a source of inspiration for developing a model, or it may be used to justify a particular hypothesis. An important distinction to take away from these examples is that analogical reasoning includes a broad use, such as when it functions as a metaphor, or it may function more narrowly, such as when scientists reason from an analogical argument.<sup>13</sup> While this distinction can be traced back to the works of Aristotle, it is not carefully preserved in later philosophical accounts, and often the broad and narrow uses of analogical reasoning are conflated (White 2010, 6 & 51). This is especially apparent in Chapter Two where I discuss the recent critiques of the standard interpretation of the *Origin*.

Philosophers and non-philosophers alike often describe Darwin's most famous work as an exemplar of analogical reasoning. In Chapter Two, I propose that an analysis of the debate surrounding Darwin's use of metaphor and analogy in the *Origin* is valuable to an account of analogy in scientific practice. I open this chapter with an introduction of the standard interpretation of Darwin's methodology, citing work by Michael Ruse and Doren Recker. According to this interpretation, Darwin reasoned from analogy to establish the causal efficacy of natural selection. Recent challenges to the standard interpretation—offered by Richard Richards and Peter Gildenhuys—threaten the standard interpretation,

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<sup>12</sup> Metaphors that guide scientific research shape both the theoretical and practical aspects of scientific inquiry. Thus, any proper account of scientific practice “entails how to avoid getting stuck at a level of particularity that evades any reasonable effort to generalize” (Woody 2014, 124).

<sup>13</sup> As I mentioned earlier, a scientific metaphor guides a research program, even Stepan likens it to a Kuhnian paradigm (Stepan 1986, 272). An analogical argument in science, however, differs by being an inferential representation of an experiment, or perhaps a feature of the phenomenon under investigation. A metaphor generates numerous analogical arguments; each resulting from a hypothetical statement based on the link between the two or more analogues. Thus, Paul Bartha recognizes the distinction between “the broad role of analogy, in providing an underlying metaphor for a sustained research program, and the narrow role of grounding an individual argument” (Bartha 2010, pp. 11-12). I refer to all instances of using an analogy as part of analogical reasoning, which includes the broad and the narrow roles of analogy. Reasoning, in this sense, is not strictly limited to inferences.

and indirectly my position.<sup>14</sup> Richards and Gildenhuys claim that their positions are based on historical context and are supported by the views held by Darwin's contemporaries, like John Stuart Mill and John Herschel. However, I demonstrate that both objections turn on a naïve account of analogy; thus, preserving the standard interpretation. If the standard interpretation is correct, then the community that witnessed the publication of the *Origin* may exhibit features useful to the critique of a metaphor. Darwin's community is an example of robust critique. Not only did its members challenge the underlying assumptions of Darwin's metaphor, but his contemporaries also actively critiqued Darwin's methodology. This critical activity foreshadows what may be a necessary feature for a communal critique of analogy: the presence of outsiders that are informed in ways that promote critical discourse.

Although respected scientists, like Galileo and Newton, relied on analogical reasoning, philosophers of science, under the influence of positivism and a commitment to remove non-literal language from the discourse of science, insisted that analogical reasoning could not be used as a justificatory inference; it could only legitimately figure in the context of discovery. In Chapter Three, I discuss analogical reasoning and its neglect by the adherents of the Received View. It was not until the late 1950's, when Mary Hesse linked the use of analogy as a predictive inference within the practice of modeling, that analogy was seriously considered as a justificatory inference. Hesse defined a set of conditions under which an analogical argument could (legitimately) provide support for one hypothesis over another (Hesse 1966, 76-77).<sup>15</sup>

Although Hesse identifies some of the ways scientists use analogical reasoning to draw inferences and use models, her account is limited, especially if one considers the types of

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<sup>14</sup> I consider these two objections more fully elsewhere. See Sullivan-Clarke (2013).

<sup>15</sup> The conditions include: recognized similarities between source and target, a causal association between the similarities and the featured hypothetical analogy, and no relevant differences (Hesse 1966, 91).

abstractions/connections present in models today (Bartha 2010, 45). For example, Paul Bartha points out that Hesse requires the similarities between analogues to be observed, which neglects connections that are not directly observable (Bartha 2010, 42). In addition, Hesse only recognizes similarities that are causally related to what is being investigated (Bartha 2010, 43). Although Hesse reopened the conversation on analogical reasoning amongst philosophers of science, her understanding of models, as well as the requirement for similarities to be observable, makes for an insufficient account of analogy (Bartha 2010, 40-45).

In *By Parallel Reasoning*, Bartha introduces an account of analogy, called the Articulation Model, and this model overcomes the limitations of Hesse's account. Providing a new way of classifying analogical arguments, as well as how to assess their plausibility, Bartha introduces considerations of context into the normative account of analogy. Members of a community draw from their previous experience and expertise when evaluating an analogical argument. To aid in the process of evaluation, Bartha utilizes a rhetorical device: the hypothetical interactions between an advocate and critic (Bartha 2010, 5). The advocate and critic are motivated by two desires; the critic seeks an explicit statement of the relevant similarities between the analogues of the argument and the advocate desires being economical with respect to stating those similarities (Bartha 2010, 102). The need of the advocate to support the analogy is often at odds with the desire of the critic to find any relevant difference between the analogues. Because the advocate and critic rely on their background experience, the effectiveness of Bartha's device depends in part upon the epistemic diversity within the community. In communities whose members are homogeneous with respect to knowledge or experience, the interactions between the advocate and critic are likely to be ineffectual. Both advocate and critic will assume the same similarities and/or lack the ability to articulate possible relevant differences between the

analogues. Without substantive differences in experience and beliefs, the advocate and critic have little to disagree over, and the plausibility of some arguments will be accepted without question.

I introduce the problem of ingrained analogy in Chapter Four, using two cases: the research on human difference conducted by Samuel George Morton in the 19<sup>th</sup> century and the contemporary research on the putative difference in sexual behaviors between males and females, as presented in Jordan-Young (2010). These cases rely on metaphors that are taken for granted in the respective scientific communities. In the case of Morton's research, the metaphor *race as distinct species* met with little resistance because it was consistent with the expectations of the larger society. The similarities that were taken as fact emphasized human difference. That is to say—the physical, behavioral, intellectual differences between species of nonhuman animals could be extended to humans of different races. In some respects, the research on brain organization presented in Jordan-Young (2010) appears to follow the same course: the assumptions that males and females are different are culturally endorsed and persist unchallenged within the scientific community as well as the larger society. For brain organization researchers, the metaphor, *prenatal hormones as brain organizer*, sanctions gendered difference. Prenatal hormone exposure (hormones that are themselves gendered) influences individual behavior, which gives rise to 'stable' concepts like masculine and feminine sexual behavior.

Morton's research and that of the brain organization research community illustrate the influence of an ingrained analogy at the level of individual research and at the level of communal practice respectively. The similarities suggested by the metaphor in both examples are taken for granted, which affects the design of experiments as well as the interpretation of data from those experiments. The influence of an ingrained analogy does not stop at this point, however. The new information about the target is added to either the

positive or negative analogy associated with the metaphor. As a result, the metaphor becomes self-reinforcing; it directs the scientists' attention to the similarities between the source and target, and the information gathered becomes part of the filter for designing new experiments and collecting new data.<sup>16</sup> Without some type of interference to prevent the analogies associated with the metaphor from becoming ingrained, the metaphor will continue to exert its influence on a community, and even the larger society, for an indefinite period of time. Unfortunately, neither community was capable of generating a robust critique of their own metaphors, nor were the communities open to criticism made by individuals, who had the relevant background experience for a robust critique but were not a member of the community.

Both the historical and the contemporary cases of human difference research not only demonstrate how an ingrained analogy can establish itself within a community, but they also illustrate that there is more to the problem of ingrained analogy than the community being mistaken. The errors reproduced within a research program have broad impact well outside of the scientific community. Each case presented in Chapter Four is also an example of the potential harm that indirectly results from policies informed by a science unwittingly relying on an ingrained analogy. As we shall see, Morton, through his research, contributed to the marginalization and oppression of non-white races, while the brain organization research community actively contributes to the social bias against non-male gender.

Previous accounts of analogy, like those found in Hesse (1966) and Bartha (2010), focus on the narrow role for analogy, the individual arguments. The problem of an ingrained analogy, however, involves both metaphor and the arguments it generates. Contemporary

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<sup>16</sup> As we shall see, experiments are designed as a result of the neutral analogy (the putative similarities between the analogues that have yet to be established). The information garnered from experiments will either be assigned to the positive analogy (the similarities between the analogues) or to the negative analogy (the known differences between the analogues). Because the analogies influence how scientists or communities undertake research, I sometimes describe the analogies as underwriting the metaphor. These are the similarities and differences that motivate the activities of science, and if they did not, then the metaphor would not matter.

accounts, such as those offered by Turbayne (1971) and Recker (2004), identify the potential risks associated with the use of metaphor and suggest ways of dealing with it. The weakness of both accounts, however, is that they assume that a community will know whether a metaphor is misleading or not. In order to address a problematic metaphor, Collin Turbayne suggests challenging it with a competing metaphor (with the hopes that the competition will prevent either metaphor from being taken for granted). In contrast to Turbayne, Doren Recker proposes a process of continuous scrutiny, recognizing that a metaphor may still be useful once any dangerous assumptions have been revealed. Although both attempt to prevent a metaphor from misleading a community, neither account is sufficient to address the problem of ingrained analogy. For example, it might be the case that a community cannot come up with a new metaphor, or the community may not have the resources to come up with a critique robust enough to dislodge an ingrained analogy.

Scientific communities will not be in a position to recognize when a metaphor that informs their research is problematic. Therefore, they must cultivate a robust critique of all their metaphors. A metaphor may appear so obvious that a community accepts it without critical reflection, or the community may be blind to their own biases or assumptions regarding the analogues. Thus, any set of strategies must motivate the community to subject every metaphor to robust scrutiny. Since members of the community may assume the role of either advocate or critic, a diversity of experience and background knowledge will be necessary for the rhetorical device to be effective.

Helen Longino, in *The Fate of Knowledge*, describes conditions that, if met by communities, will provide “effective or transformative criticism” (Longino 2002, 134). Longino writes, “satisfaction of these norms assures that theories and hypotheses accepted in the community will not incorporate the idiosyncratic biases of an individual or subgroup” (Longino, 2002, 134). For communities to develop the critical environment needed for

optimal exchanges between Bartha's advocate and critic, Longino's norms for objective knowledge production may be a promising starting point. For example, she claims that communities need to provide "publicly recognized forums for the criticism of evidence, of methods, and of assumption and reasoning" (Longino 2002, 129). Journals, conferences, and papers are possible venues for the advocate and critic to challenge the assumptions underwriting a metaphor. Once a community creates venues, it should encourage the development of an optimum critical environment by allowing for the presentation of possible challenges to a metaphor, as well as showing respect for public critique with a dedicated space. Not only must communities make room for critique, Longino requires them to be responsive to and seriously consider critical input (Longino 2002, 129-130). An environment that values and seriously considers critique contributes to the success of Bartha's rhetorical device.

Longino's norms for public standards and her requirement for the tempered equality of intellectual authority suggest ways in which Bartha's rhetorical device might be best implemented. According to Longino, "participants in a dialogue must share some referring terms, some principles of inference, and some values or aims to be served by the shared activity of discursive action" (Longino 2002, 130). These standards are shared by the community, acknowledged publicly, and may even be challenged by those external to the community (Longino 2002, 130-131). The norm for public standards is crucial for helping the advocate and critic challenge the assumptions of a metaphor. Even when the community is epistemically homogeneous, members should be receptive to the contributions and changes occurring in other disciplines, even to the point of accepting critique from those outside of their own community—if pertinent.

Longino's conditions establish a framework for a communal response to the problem of ingrained analogy, but they lack the details necessary to realize them in practice. In the

final chapter, I aim to fill in those details, recognizing that my strategies might encounter resistance from the communities they are intended to assist. Critics may raise objections to the implementation of my strategies given the perceived costs. For example, requiring venues for critical uptake and encouraging critique from those believed to be outside the discipline not only costs money, but such strategies also divert time and resources away from the research program. Scientists may feel torn between being productive and questioning the assumptions that underpin their research. Others might question whether the development of robust critique is actually feasible, or they may balk at the idea of “outsiders” questioning their methods/assumptions.

While I recognize the costs associated with my view, I want to emphasize that my project is not merely to help scientists use analogy better; it has ethical implications as well. My examples of science demonstrate a serious potential for harm, which generally occurs in disciplines where research informs government social policies. Federal policies, like African slavery and Indian Removal, were justified on the basis of the science of the day. Even now, in the case of brain organization research, the treatment of women and homosexuals is pernicious.

Of course, not all outsiders should be included in the critical discourse, and the decisions about who is included—though not fully developed—will be made based on the subject matter/phenomena being investigated. This information will highlight the relevant experience needed to critique the assumptions of the metaphor. While communities should not expect critique to come solely from within, it does not follow from this that communities are obliged to engage all possible external critiques indiscriminately. I conclude Chapter Five with a sketch of future lines of inquiry opened up by the creation of zones for external critique.

Darwin and his supporters responded to most of the objections tendered by his critics. In some cases, the response took the form of a new edition of the *Origin*, or a paper presented at the Royal Society. The community that witnessed the publication of the *Origin* has something to offer to those interested in the social dynamics of epistemic communities. Analogical reasoning is firmly rooted in the practice of science. Given this, epistemic communities should implement sound practices for critically appraising analogical reasoning. Although ingrained analogies have the potential to mislead, scientific communities can improve their epistemic practice, thereby not only addressing the problem of ingrained analogy, but also making science more socially responsible.

## Chapter One

### Analogical Reasoning and Scientific Practice

The argument strategy in the *Origin*, particularly Charles Darwin's analogy between artificial and natural selection, is often cited as an example of good analogical reasoning.<sup>17</sup> Yet, Darwin also recognized a key limitation associated with inferences from analogy: in spite of a compelling number of similarities between two entities, reasoning from analogy has the potential to mislead. At the end of the *Origin* Darwin states,

It may be asked how far I extend the doctrine of the modification of species [...] I believe that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number.

Analogy would lead me one step further, namely, to the belief that all animals and plants have descended from some one prototype. But analogy may be a deceitful guide. Nevertheless, all living things have much in common, in their chemical composition, their germinal vesicles, their cellular structure, and their laws of growth and reproduction [...] Therefore I should infer from analogy that probably all organic beings which have ever lived on this earth have descended from some one primordial form, into which life first breathed.

(Darwin 1859, 483-484)

Given Darwin's remarks, his continued reliance on analogy as a justificatory inference seems on the surface puzzling, prompting the question, "Is there a way to exploit analogical inferences while at the same time reducing the likelihood of being misled by one?" Recent contributions to the discussion on analogical reasoning do not have the resources needed to

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<sup>17</sup> See Arber (1947) p. 232, Bartha (2010) pp. 24-26, Carloye (1971) p. 562, Darden (1982) p. 132, Depew (2001) p. 13, Garfield (1986) p. 318, Harré (1988) p. 124, Holyoak and Morrison (2005) p. 117, Holyoak and Thagard (1995) pp. 173-174, Levine (2009) p. 593, Sober (1999), Thagard (1978) p.89, Thagard and Beam (2004) p. 513, and Waters (1986) p. 502.

address such a problem. Such contributions focus either on the assessment of individual analogical arguments or on the metaphor, neglecting the larger, iterative process that takes place between both metaphor and argument.

In this dissertation, I examine the problem of ingrained analogy: a problem of analogical reasoning which occurs when a metaphor that provides the conceptual framework for analogical reasoning within a scientific community is taken for granted.<sup>18</sup> Metaphors are an essential part of scientific reasoning and their influence, which is often implicit, profoundly affects knowledge creating practices and indirectly, policies in larger society. For example, metaphors motivate arguments that endorse a particular metaphysical view of the natural world, which is based on what is believed known about the source analogue.<sup>19</sup> What the community 'knows' about the source affects the design of experiments and the interpretation of results, making the metaphor self-perpetuating. If the assumptions underwriting the metaphor are taken for granted, they become ingrained within the minds of the members of the community. Even if the community is aware of the dangers of drawing analogical inferences, its members may not be in the position to contest the assumptions of the metaphor. Given that the problem of an ingrained analogy results from the exchanges of information between the metaphor and the arguments generated by it, normative accounts of analogy must address the entire process between metaphor and analogy.

### **§1.1 What is Analogical Reasoning? What is Analogy?**

When philosophers discuss analogy, they typically refer to an inference type, such as an argument from analogy. For example, a common analogical argument taught in

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<sup>18</sup> This type of metaphor is described by George Lakoff and Mark Johnson as a conceptual metaphor. According to Lakoff and Johnson, "*the essence of metaphor is understanding and experiencing one kind of thing in terms of another*" (Lakoff and Johnson 1980, 5). For example, the metaphor (*argument is war*) causes one to understand, experience, and talk about arguments in terms of war (Lakoff and Johnson 1980, 5). In other words, metaphor affects our concepts, such as our concept of argument.

<sup>19</sup> A source analogue is the more familiar entity (or concept) comprising the metaphor (Lakoff and Johnson 1980, 4-5).

introductory philosophy courses is Bishop William Paley’s watchmaker analogy in his book, *Natural Theology*. Paley used this analogy to infer the existence of a designer for the natural world.<sup>20</sup> Of course, not every instance of analogy is an argument or intended to persuade someone (Salmon 1995, 117). Some analogies “enhance our ability to see the world in a new way,” such as when the poet Robert Frost exploits the similarities between taking a journey and living one’s life in, “The Road Not Taken” (Salmon 1995, 118).<sup>21</sup> In this work, analogical reasoning does not merely refer to an argument form. Instead, it includes “any type of thinking that relies upon an analogy” (Bartha 2010, 1).<sup>22</sup> Under such a definition—metaphors, similes, allegories, parables, and the arguments underwritten by analogy—comprise analogical reasoning.<sup>23</sup>

In philosophy, analogy has acquired several meanings and is “understood by different people in very different ways” (White 2010, 6). Its history traces back to ancient Greece with the introduction of two definitions of analogy presented by Aristotle (White 2010, 51). As Roger White describes,

Aristotle starts out with a basically very simple idea, the idea that there are two different ways to compare things—a direct comparison where one notes common properties of the two things, and an indirect comparison where, whether or not the two objects have significant common properties, one effects the comparison by introducing a third and fourth term.

(White 2010, 51)

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<sup>20</sup> See Paley (1826), pp. 1-9.

<sup>21</sup> Frost compares life to a journey, stating—  
Two roads diverged in a wood, and I—  
I took the one less traveled by,  
And that has made all the difference.

For the full poem, see Frost (1916), p. 9.

<sup>22</sup> Although I adopt a broader understanding of analogical reasoning, I am restricting its use to science. This distinguishes it from the use of analogy in literature. In literature, there are no constraints for the selection of source analogies; instead, novel connections between source and target are valued. As we shall see, the use of metaphor in science is constrained by the phenomenon under investigation (the goal is to have a fruitful metaphor) and the value of similarity is tested by an obligation to seek out difference.

<sup>23</sup> Although similes, allegories, and parables are part of analogical reasoning, they do not figure into the practice or discourse of epistemic communities to the extent that metaphor and arguments from analogy do nor do they figure into the problem of ingrained analogy. Therefore, I have not addressed those linguistic devices here.

As part of my analysis, what underwrites analogical reasoning in general, is based on Aristotle's first meaning of analogy: a simple comparison of two or more entities, which is initiated by some perceived similarities.<sup>24</sup> I reserve Aristotle's second definition, which includes positing a likely similarity based on the previously observed similarities, for instances of argument from analogy and metaphor.<sup>25</sup> First, let's examine the features of a simple analogy.

Although White describes Aristotle's first meaning as a comparison between "two things," these things need not be physical objects (White 2010, 51). As Paul Bartha points out, an analogy can exist between two "systems of objects" (Bartha 2010, 2). For example, the Rutherford analogy, which draws upon the similarities of the solar system and the atom, does not exploit the physical features of either object (Gentner 1983, 162). Instead, the correspondences between the analogues are comprised of abstract relations present in the solar system, such as "revolves around" (Gentner 1983, 163).

Whether a physical object or a higher order abstraction, the comparison takes place between two or more analogues, referred to as the target and source.<sup>26</sup> The target is the subject of inquiry, while the source is generally better known or more familiar. For example, the Rutherford analogy explores what is unknown about the behavior of atoms (the target) by using the familiar orbits of our solar system (the source). Another example that I consider in the next chapter is the analogy Darwin draws between artificial and natural selection. According to the standard interpretation of the methodology in the *Origin*, Darwin

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<sup>24</sup> I use the word 'perceived' to emphasize how an analogy draws attention to the similarities between two or more entities, giving a sense of awareness to the person recognizing those similarities. In addition, it is worth noting that the similarities do not have to be observed. They can be higher-order entities.

<sup>25</sup> The difference between an analogical argument and metaphor is the number of similarities proposed. As we shall see, an analogical argument implies only one additional similarity (which is called a hypothetical analogy) based on the similarities that were previously recognized. A metaphor will suggest many possible similarities, which is why it is capable of motivating programs of research. I will say more about this later in the chapter.

<sup>26</sup> 'Analogue' is often dropped from use for convenience.

used the familiar features of domestic breeding (the source) to argue for a similar process occurring in nature, natural selection (the target).

Note that in the cases of the Rutherford and Darwin analogies, Aristotle's second definition of analogy is applicable as well. This second definition, which is an indirect comparison, reveals the ways in which analogy is an ampliative form of reasoning. When comparing the source and target, we might believe that the source has a particular property and what we would like to know is whether the target also shares that property (White 2010, 46-51). For example, Darwin noted that physical traits were often inherited by the offspring of domesticated plants and animals. As a result, he supposed that a similar process occurred in nature.<sup>27</sup> This particular example is commonly understood as an analogical argument. After all, Darwin only posits one similarity—the process—given the similarities that exist between domesticated animals/plants and their wild counterparts. Yet, if Darwin were to consider all the unknown, possible similarities that exist between the two analogues, and reason from analogy in each case, then he is relying on metaphor. Metaphor is important because scientists (and their communities) want to know more about a target than whether it has one property that is similar to the source. Instead, they want to learn as much about the target as possible. The examples of the Rutherford analogy and the Darwin analogy demonstrate that the use of metaphor as source of inspiration for more specific analogical inferences is not unusual in science.<sup>28</sup> Other metaphors used in science include *light as a particle/wave*, *race as species*, and *nature as machine*.

The similarities and differences between the source and target are critical to this definition of analogy, regardless of whether it takes the form of metaphor or analogical argument. Similarities between the analogues encourage the pursuit of scientific

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<sup>27</sup> See Darwin's discussion on domesticated species throughout the world in Chapter One.

<sup>28</sup> According to several philosophers, metaphor is the precursor to the use of some models. In the literature, these models are sometimes referred to as analogical models. See Leatherdale (1974), Bailer-Jones (2002), Harré (1988).

investigation or strengthen an argument, while differences might dissuade scientists from pursuing particular questions or cause them to reject a conclusion outright. For the present purposes, I distinguish between a metaphor and an analogical argument by appealing to the scope of each. A metaphor directs scientific enquiry by providing a set of potential correspondences for investigation, the neutral analogy. An analogical argument draws from certain similarities in order to infer only one possible correspondence.

At times, the terms of analogical reasoning are conflated and at other times, the terms can be downright confusing. In her primary work, *Models and Analogies in Science*, Mary Hesse draws attention to the components of a metaphor. According to Hesse, once the initial analogy between a source and target is made, scientists at once become interested in the similarities and differences between them. The set of similarities between two analogues comprise what Hesse refers to as the positive analogy and the set of differences form the negative analogy (Hesse 1966, 8-10).<sup>29</sup> In the context of scientific investigation, those features of the source and target that we do not know to be similar or different comprise the neutral analogy.<sup>30</sup> Referring to the sets of similarities, differences, and features that are neutral as analogies is a source of confusion, especially when the initial comparison between two or more analogues is also referred to as an analogy. Throughout this work, I will reserve the term 'analogy' for an initial comparison between two or more analogues. I will refer to the sets of similarities, differences, and neutral features as the positive analogy, negative analogy, and neutral analogy respectively. When I speak of the analogies that underwrite a metaphor, I am referring to the positive, negative, and neutral analogies. These are the sets

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<sup>29</sup> The terms positive, negative, and neutral analogies are used in Hesse (1966) but originate with Keynes (1921).

<sup>30</sup> Of course, some features of the target may not be of interest to scientists and although it is not known whether these features are similar or different, they may not figure as prominently in the actual research.

of ‘facts’ about similarity/difference between the source and target (and they are also what motivate a research program).<sup>31</sup>

To elaborate on the analogies underwriting a metaphor, I return to Darwin’s theory of natural selection. The positive analogy of Darwin’s metaphor refers to the set of similarities shared by artificial and natural selection. Members of this set include: the same subjects of selection (plants and animals), the belief that phenotypic traits are inherited, and a process for the removal of deleterious traits from the population (culling/roguing in domestic cases and extinction in nature) to name a few. Members of the negative analogy include: the question of agency (there is a breeder actively selecting in domestic cases, where nature does not actively select), the notion of progress (breeders aim at some perfect ideal, whether more yield, better meat, or finer quality of fiber and nature does not), and the amount of time it takes for change to occur (artificial selection never presented a new species although Darwin believed after an extremely long period of time, species would evolve).<sup>32</sup>

In the early chapters of the *Origin*, Darwin details the observable consequences of artificial and natural selection, noting that the similarities between the two prompted him to ask whether the selective process might be the similar in both cases (Darwin 1859, 80).<sup>33</sup> The proposition, “the two processes are similar,” is a member of the neutral analogy of Darwin’s metaphor.<sup>34</sup> In addition, Darwin mentions the correlation of growth, strong inheritance, and reversion as laws that are observed in domestic cases, but the particulars of

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<sup>31</sup> Some may press the issue and claim that the metaphor underwrites the analogy. I do not disagree with this statement, but it should be noted that the analogy in this case is the initial comparison. Ultimately, the positive, negative, and neutral analogies underwrite a metaphor, which in turn underwrites an analogy!

<sup>32</sup> Note that these differences were often cited by Darwin’s critics who believed the differences to be relevant and thus, weakening the justification for natural selection.

<sup>33</sup> Darwin asks, “Can the principle of selection, which we have seen is so potent in the hands of man, apply in nature?” (Darwin 1859, 80-81).

<sup>34</sup> Imagine the neutral analogy is a set of propositions about the unknown similarities/differences between the source and target. For example, “the process is the same” and “it is not the case that the process is the same” are members in the neutral analogy of Darwin’s metaphor. When Darwin uses an analogical argument, one member of the neutral analogy (sometimes called a hypothetical analogy) is in the conclusion. “The process is the same” was the conclusion of Darwin’s analogical argument, and it was a member of the neutral analogy of his metaphor. As we shall see, research or experimentation help to determine whether a hypothetical analogy is assigned to the positive or negative analogy.

these laws were not well known at the time (Darwin 1859, 11-15). Scientists might draw on a feature from one of the laws present in artificial selection to answer a question regarding the law in the case of nature. Other possible members of the neutral analogy might be the contributions Darwin suggests his theory of natural selection will make toward developing new areas of study (such as, embryology and geography), and possibly those questions that the theory of natural selection explains better than its competitors.<sup>35</sup> What comprises the neutral analogy is limited only by what scientists know about the source analogue.

Scientists rely on the positive, negative, and neutral analogies of a metaphor to generate analogical arguments that motivate the design of experiments used to test their hypotheses, and ultimately their theories.<sup>36</sup> The arguments generated by a metaphor are generally arguments from analogy and the premises of these arguments are created from the positive and negative analogies of the metaphor. The conclusion of these arguments is one of the unknown features from the neutral analogy (something known about the source that is hypothetically extended to the target). Although the premises of an argument support the conclusion, the similarities must also be relevant to the conclusion, while at the same time the differences cannot weaken the premise-conclusion connection.<sup>37</sup> Thus, it may be helpful to reconstruct such arguments in a way that makes evaluation easier.

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<sup>35</sup> For example, Darwin believed that natural selection could account for the distribution of distinct flora and fauna throughout the world. Climate or physical conditions were not enough to explain similar and different organisms in areas with similar environments (Darwin 1859, 346). In the last chapter, Darwin believes that the similarity of bones in mammals is explained by his theory (Darwin 1859, 479). Natural selection will contribute to the disciplines of classification, embryology, husbandry, and geology (Darwin 1859, 486-487).

<sup>36</sup> For some philosophers, the metaphor provides an analogical model that scientists consult to create hypotheses and test their theories via prediction and experiment. According to Rom Harré, "Often an iconic model is imagined, and its behaviour studied in a *gedanken-experiment*. For instance, the internal circulations of droplets falling in different media can be studied by imagining that a droplet is enclosed in a rubber balloon, that is, in a surface which is materially distinct from the liquid within. In this way surface tension effects can be neatly separated from internal processes, with the advantage of clear vision of the convection flows within" (Harré 1988, 121-122).

Given the confusion between metaphor and model, I opt to use metaphor. While I believe that most, if not all, models are supported by analogy, I will set this particular question aside for future research.

<sup>37</sup> In the standard premise-conclusion form of an analogical argument, there may be an implicit premise, which denies the existence of any relevant difference between source and target.

As part of her account, Hesse presents analogical arguments based on the metaphor as a tabular representation and not the standard premise-conclusion form that is commonly found in informal logic texts. This way of reconstructing the argument has some advantages. For example, consider Thomas Reid's argument for life on other planets.<sup>38</sup> Reid argues,

We may observe a very great similitude between this earth which we inhabit, and other planets, Saturn, Jupiter, Mars, Venus, and Mercury. They all revolve around the sun, as the earth does, although at different distances and different periods. They borrow all their light from the sun, as the earth does. Several of them are known to revolve round their axis like the earth, and by that means, must a like succession of day and night. Some of them have moons, that serve to give them light in the absence of the sun, as our moon does to us. They are all, in their motions, subject to the same law of gravitation, as the earth is. From all this similitude, it is not unreasonable to think that those planets may, like our earth, be the habitation of various orders of living creatures. There is some probability in this conclusion from analogy.

(Reid 1941, 36)<sup>39</sup>

Argument 1 represents Reid's argument, written in premise-conclusion form:

#### Argument 1

1. Earth and the other planets, Saturn, Jupiter, Mars, Venus, and Mercury all have the following attributes: each revolves around the sun, receives light from the sun, rotates upon an axis, and has moons.
2. Earth has the attribute of being populated with living creatures.
3. Therefore, the other planets, Saturn, Jupiter, Mars, Venus, and Mercury have the attribute of being populated with living creatures.

Note that the first premise of an argument from analogy, when written in the standard form, identifies the similar properties between the source (Earth) and the target (the other planets). Given these similarities and the additional information contained in the second premise (Earth has life on it), Reid suggests that the target will also have the additional

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<sup>38</sup> See Reid (1785) for the original argument. In addition, Reid does not suggest reconstructing the argument in either form. Argument 1 and Argument 2 serve to compare two different ways of reconstructing the same argument.

<sup>39</sup> Also appears in Copi (1978).

property of being populated with living creatures. Although Reid notes in his original argument that some possible differences between the source and target (such as revolving around the sun at different distances and different periods) exist, these differences do not appear in Argument 1.

Although the differences between the analogues do not normally appear in the premise-conclusion form, they do play a role in evaluating the strength or cogency of the argument. In some informal logic texts, the premise-conclusion reconstruction presupposes an implicit premise, one that rules out the existence of possible differences, or in other cases one that rules out **relevant** differences.<sup>40</sup> Another difficulty posed by the standard form is that the similarities are assumed to be relevant to the conclusion. By appearing in the premises, the similarities (revolving around the sun, rotating upon an axis, and having moons) are suggested by the argument to be relevant to the conclusion.<sup>41</sup> In other words, Reid intends for the similarities to be understood as being relevant to life existing on a planet. According to Reid's original argument, the differences are not relevant to whether life exists on the other planets, which is confirmed by his statement; "There is some probability in this conclusion from analogy" (Reid 1941, 36). Today, however, when we consider the two noted differences (revolving at different distances and different periods)

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<sup>40</sup> The presence of differences may not affect the strength of an argument from analogy because the source and target (by virtue of not being identical) will share some differences. Relevant differences, however, affect the strength of the argument because they challenge the relationship between the similarities in the premises with what is posited in the conclusion. An argument from analogy that also has a known relevant difference is extremely weak or too weak to matter.

<sup>41</sup> The problem with the standard premise-conclusion form is its emphasis on similarities to the neglect of the differences that are relevant to the argument. In addition, it may not include the implied premise that the similarities are relevant to the conclusion. Instead, the users of this reconstruction are especially motivated to accumulate similarities because that is all that is needed for the argument to be cogent. Furthermore, they often neglect to search for a relevant difference, which might refute the conclusion. In Chapter 2, I suggest that the revisionist accounts of Richard A. Richards and Peter Gildenhuys are informed by such an account of analogy. Arguments from analogy are believed by Richards and Gildenhuys to be strengthened by the number of similarities, whether relevant or not, and they believe any difference could refute a conclusion, whether relevant or not.

and whether the similarities are relevant to a planet being populated with living creatures, it should be apparent that the argument is too weak to be seriously considered.<sup>42</sup>

Contrast Argument 1 with the tabular representation of the argument from analogy below in Argument 2. In this type of reconstruction, the similarities that are attributed to the source and target found in Argument 1 are now part of the horizontal correspondences of the table.

### Argument 2

<b>Source:</b> Earth	<b>Target:</b> Saturn, Jupiter, Mars, Venus, and Mercury
Revolves around the sun	Revolves around the sun
Particular distance from sun	Different distances from sun*
Receives light from sun	Receives light from sun
Particular period (approx. 365 days)	Different periods <sup>43</sup> * (approx. 88 days-29 years)
Rotates on axis	Rotates on axis
Has moon/s	Has moon/s
Populated with life	?

The tabular representation found in Argument 2 offers a visual side-by-side comparison of the corresponding similarities **and** differences between the source and target.

The number of rows, which indicates the number of similarities and differences, is easily

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<sup>42</sup> This is a good example of how background knowledge plays a role in the analysis of analogical arguments. Argument 1 may be strong for one person and very weak for another. We will see in a later chapter that this feature of argument analysis is crucial for dealing with an ingrained analogy.

\* The asterisk denotes differences between the earth and the other planets.

<sup>43</sup> I am using the revolution period (the time it takes revolve around the sun), but the rotational period differs as well.

observed, making the evaluation of the argument easier.<sup>44</sup> Another advantage of the tabular representation is the association between the premises and the conclusion (that the similarities are relevant to what is posited in the conclusion) is understood in terms of the vertical relation between the properties of the source.<sup>45</sup> Understanding the relevance relation between the similarities posited by the horizontal correspondences is critical when attempting to infer that relation to the target. For example, in the above tabular representation, the properties of the source are believed to relate to the conclusion; that is, the properties stated in the premises support the possibility of life on other planets. If the argument is cogent, then the relation between the properties of the source and the property cited in the conclusion is likely to be similar to the relation between the properties of the target and the property cited in the conclusion. In the case of Reid's argument, the properties shared by the source and the target analogues are believed to be conducive to populations of living creatures.

Note that unlike the premise-conclusion reconstruction, the differences between the source and target can also be included within the tabular representation. For example, the orbits of the planets differ from that of earth (as well as the time that it takes for a planet to complete an orbit around the sun). This is an additional feature that the tabular representation may exploit to the benefit of those critiquing the argument. If these differences were found to be relevant, they would affect the argument's cogency. A large share of scientific investigation is guided by how scientists believe the similarities between

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<sup>44</sup> For example, the tabular representation of Reid's argument has the bonus of including perceived differences as well as similarities. Of course, Reid did not consider these differences relevant to the conclusion. Regardless, noted differences may turn out to be relevant (as in the case of the periodicities of the planets in Reid's argument). As we shall see, the inclusion of differences in the tabular representation will prove useful for implementing Bartha's rhetorical device, the interactions of the advocate and critic.

<sup>45</sup> Bartha describes relevance as a vertical relation of the properties of the source (2010) p. 25. Bartha also notes that in the case of scientific analogies, philosophers of science have a better idea of the different types of relevance relations (Bartha 2010, 8). For example, there are relations that are causally, explanatory, logically, and statistically based and these, according to Bartha, "constitute the relationships at the heart of most scientific analogies" (Bartha 2010, 8). I will explore his account in more detail in Chapter Three. For the moment, the goal of this section is to introduce the concepts and terminology needed to comprehend the mechanics of an ingrained analogy.

the source and target are related. As we will see when we consider Bartha's account, the tabular representation makes the relation between the similarities in the premises and the conclusion evident, and thus, subject to analysis. It also reveals the number of perceived similarities and the possible differences between the source and target; something the standard premise-conclusion reconstruction does not do as well, if at all. The tabular representation, as opposed to the standard pattern, makes the evaluation of analogical arguments easier.

Analogical reasoning takes various forms within scientific practice. From a simple analogy that underwrites the selection of a possible source to a metaphor that directs scientific investigation, analogy appears throughout scientific practice. It is not surprising that the term analogy has acquired various meanings over time, and that users often conflate analogy and metaphor.<sup>46</sup>

## § 1.2 Why Scientific Practice?

The question of what exactly constitutes scientific practice is not easily answered. Of course, science, at least for the most part, is what scientists do. However, if we allow that scientists are normal people, using everyday reasoning to solve problems or conduct research, then a large share of what scientists do cannot and, most likely is not considered scientific. For example, not just any type of problem solving counts as scientific practice; balancing a checkbook, planning a trip to a conference, and negotiating five o'clock traffic all require reasoning.<sup>47</sup> Yet, I doubt that many would consider such uses of reason scientific, or perhaps even noteworthy. However, if we limit what counts as scientific practice to the activities scientist performed at work, the definition would include the non-scientific actions

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<sup>46</sup> Daniela Bailer-Jones notes that metaphor and analogy are "often not distinguished very sharply" (Bailer-Jones 2002, 108). For example, in her description of 19<sup>th</sup> century race science, Stepan conflates metaphor with analogy. Stepan notes, "In this article, I have used the terms metaphor and analogy interchangeably" (Stepan 1986, 261).

<sup>47</sup> The last two, planning travel and negotiating traffic, may include analogical reasoning if the scientist uses previous experiential knowledge of an airport/airline or travel route.

performed on the clock, as well as omitting those times when a scientist reasons off the clock. Whether too narrow a conception or too broad, the task of delimiting scientific practice is not as easy as one first believes, especially if we expect to apply it to all disciplines. In this section, I propose thinking about scientific practice—especially the practice of working from models—in terms of a set of activities connected with analogical reasoning.<sup>48</sup> Thinking of practice in this way has some distinct advantages: it prevents an account of scientific practice from being too broad and it grounds the account in actual practice. Admittedly, my method for delimiting scientific practice may overlook other forms of reasoning.<sup>49</sup> However, the use of analogical reasoning does not necessarily preclude other forms of reasoning.<sup>50</sup>

The turn to practice, or the focus on practice, is a relatively recent trend in philosophy of science. As Andrea Woody observes, the turn to practice “refers not to a single shift, but rather a cluster of related, and potentially interdependent, changes, each consisting in a retreat from a certain sort of abstraction” (Woody 2014, 123). These changes include the move from emphasizing conception to representation, the shift of focus from the *a priori* to the empirical, the move from scientist as ideal agent to a human practitioner, and the switch from an epistemology of individuals to that of an epistemic community (Woody 2014, 123-124). These changes, according to Woody, were motivated by Ian Hacking’s and Allan Franklin’s “New Experimentalism,” as well as the work done in science studies.<sup>51</sup> Woody observes that these works were the impetus for moving from a “theory-centric bias within traditional philosophy of science” to discrete practices, such as “measurement techniques,

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<sup>48</sup> In the next section, I demonstrate the various uses of analogical reasoning in science and the activities associated with them.

<sup>49</sup> It may be the case that this account only applies to modeling. I reserve this topic for future research.

<sup>50</sup> In fact, Darwin is believed to have used his analogy as part of a larger argument strategy. See Hodge (1992), Recker (1987), Ruse (1975), Sintonen (1990), Sterrett (2002), and Wilson (1992).

<sup>51</sup> Woody points to work done by “Pickering, Latour, Woolgar, Collins, Galison, and others (See Hacking 1983, 1988, and Tiles (1992)” (Woody 2014, 124).

instrumentation, embodied skills, and the social organization of large scale laboratory science” (Woody 2014, 124).

Yet for all the novelty of these recent developments, Hasok Chang notes “many philosophers deeply concerned about scientific practice have tended to use rather haphazard or limited framework for description and analysis” (Chang 2011, 206).<sup>52</sup> For example, Chang cites Ian Hacking’s 1992 paper, “The Self-Vindication of Laboratory Studies,” which contains “15 different kinds of elements that enter into experimental practice” (Chang 2011, 206). Although extensive, Hacking’s list is still for Chang “overly haphazard” (Chang 2011, 206).<sup>53</sup> Hacking fails to account for how “these elements combine and interact,” offering a static depiction of scientific practice (Chang 2011, 206). As an alternative, Chang offers an account, which emphasizes the actions comprising scientific practice and attends to “what it is that we actually do in scientific work” (Chang 2011, 208).<sup>54</sup>

For Chang, the grammar of scientific practice amounts to a “simple change of viewpoint;” one that results from the use of verbs in descriptions of practice (Chang 2011, 208). By making the switch to active verbs, Chang suggests “a whole range of questions

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<sup>52</sup> Chang cites the accounts offered by Ian Hacking (1992), David Gooding (1990), and Joseph Rouse (1996).

<sup>53</sup> The complete list is separated into three categories and presented as follows:

**Ideas**

1. Questions
2. Background knowledge
3. Systematic theory
4. Topical hypotheses
5. Modelling of the apparatus;

**Things**

6. Target
7. Source of modification
8. Detectors
9. Tools
10. Data generators;

**Marks and the manipulation of marks**

11. Data
12. Data assessment
13. Data reduction
14. Data analysis
15. Interpretation.

(Chang 2011, 206)

<sup>54</sup> Chang also draws from Michael Polanyi (1962), J. L. Austin (1962), Stuart Hampshire (1982), and Marjorie Grene (1974) (Chang 2011, 206).

regarding actions emerge naturally, almost without effort: who is doing what, why, how, and in what context?" (Chang 2011, 208).

To further his point, Chang contrasts "the nature of a definition" with "what one has to do in defining a scientific term" (Chang 2011, 208).<sup>55</sup> Other examples provided by Chang include viewing confirmation as "various processes of hypothesis-testing" and thinking about scientific explanation in terms of "how the act of explaining arises and how it is best performed" (Chang 2011, 208). Scientific practice, for Chang, is "a coherent set of mental or physical actions (or operations) that are intended to contribute to the production or improvement of knowledge in a particular way, in accordance with some discernible rules" (Chang 2011, 209). Scientists participate as "epistemic agents" that have hopes/desires and are not alone in the creation of knowledge (Chang 2011, 210-214). They enter into "second-person" interactions with other scientists in ways often acknowledged by social epistemology (Chang 2011, 213).<sup>56</sup>

Chang's project is ambitious, and it seems implausible that the mere switch to active verbs can generate a meaningful account of practice.<sup>57</sup> The scope of my account of practice is more modest than Chang's; it is limited to the different uses of analogical reasoning in science. Instead of focusing on active verbs, I maintain that philosophers of science should

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<sup>55</sup> According to Chang,

Instead of thinking about the nature of definition, we can consider what one has to do in defining a scientific term: formulate formal conditions, construct physical instruments and procedures for measurement, round people up on a committee to monitor the agreed uses of the concept, and devise methods to punish people who do not adhere to the agreed uses. In one stroke, we have brought into consideration all kinds of things, ranging from operationalism to the sociology of scientific institutions.

(Chang 2011, 208)

<sup>56</sup> Chang believes that they enter into conversations and argue with each other with the hope of convincing or persuading one another (Chang 2011, 213).

<sup>57</sup> Chang notes that his goal is "to provide a systematic and comprehensive framework for the description and analysis of scientific practice" (Chang 2011, 205). If so, then it seems his account should delineate scientific practice from the practices of non-scientific communities, and I am not convinced his account can do that. I believe that a focus on analogical reasoning will generate the right questions about activities without presuming to already know what activities are scientific. This is a topic that I would like to develop further in the future. For the purposes of my project, however, I will only argue that reasoning is more fundamental to practice than a switch to active verbs. That is, focusing on analogical reasoning will yield some, if not a large share, of the activities consistent with our intuitions of practice.

exploit analogical reasoning as a guide to scientific practice. By making use of analogical reasoning, philosophers of science can identify the actual activities that make up scientific practice, such as designing experiments or working from models.<sup>58</sup> In addition, I contend that the constraints associated with analogical reasoning can provide some standards for assessing the answers to some of the questions Chang believes follows from a pursuit of active verbs.<sup>59</sup>

While my account of practice is more modest than Chang's, it also shares some of the same features. Like Chang's grammar of scientific practice, analogical reasoning is a mental, epistemic activity, and when science is directed by a metaphor, practice can be described as a cluster of epistemic activities performed by real people who make decisions, and even reason against a backdrop of hopes/desires.<sup>60</sup> Investigating how and why scientists use analogical reasoning—whether it is an enumeration of similarities, an inference endorsing a particular hypothesis, or a metaphor guiding research—reveals the activities that comprise scientific practice.

### § 1.3 The Different Roles of Analogical Reasoning in Scientific Practice

Keith Holyoak and Paul Thagard state that analogical reasoning has “at least four distinguishable uses in science: discovery, development, evaluation, and exposition” (Holyoak and Thagard 1995, 189).<sup>61</sup> These four uses can be used to identify some, if not all,

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<sup>58</sup> Although I do not have a fully developed argument for this claim, looking at the diagram of scientific practice in terms of analogical reasoning, below, will give an idea of the direction I plan to take in future papers.

<sup>59</sup> For example, questions about what source is used, why that source and not another, and what goals/aims motivated the selection of a particular source will introduce the context that Chang finds to be valuable when discussing practice. As we shall see in the next section, the constraints of selecting a source may be a good indicator of the goals/aims of who answers the questions.

<sup>60</sup> In fact, the hopes and desires (the human nature) of the scientists may contribute to the problem of ingrained analogy. Guided by a metaphor, one sees the world through its filter. Of course, this metaphor was chosen for particular reasons, and preference may have figured into its selection.

<sup>61</sup> Holyoak and Thagard remove exposition from conversations on scientific practice. Exposition is merely a heuristic device that can be used outside of the scientific community. Exposition is important, especially if we consider the training that a discipline requires of its members. Textbooks make use of analogies in order to temporarily explain concepts even though the analogies may confuse students later. Holyoak and Thagard's cites the

of the activities comprising scientific practice.<sup>62</sup> In the diagram below, I create an outline of scientific practice, using Holyoak and Thagard's four uses of analogical reasoning in science (see bold text).<sup>63</sup>

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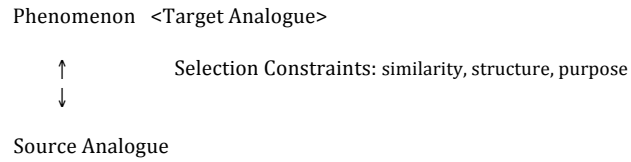
Rutherford-Bohr model of the atom as a model that is useful in teaching the concept of orbits, but will pose problems later when students learn quantum mechanics (Holyoak and Thagard 1995, 208).

<sup>62</sup> Holyoak and Thagard note that they have not been able to locate an analogy that participates in discovery, development, and evaluation. I am not quick to agree. I believe Darwin's theory of natural selection is a good candidate. I refer to Darwin's use of modelling in this chapter and the next. However, a more fully developed view would require more detail. In fact, I believe that Darwin's theory would also be included in the category often omitted from standard accounts of practice: exposition.

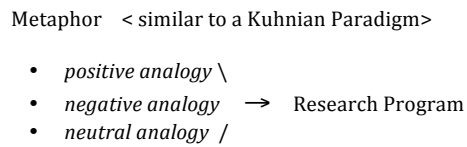
<sup>63</sup> Note this outline of scientific practice is limited to the activities connected with analogical reasoning.

Diagram 1. Analogical Reasoning in Scientific Practice

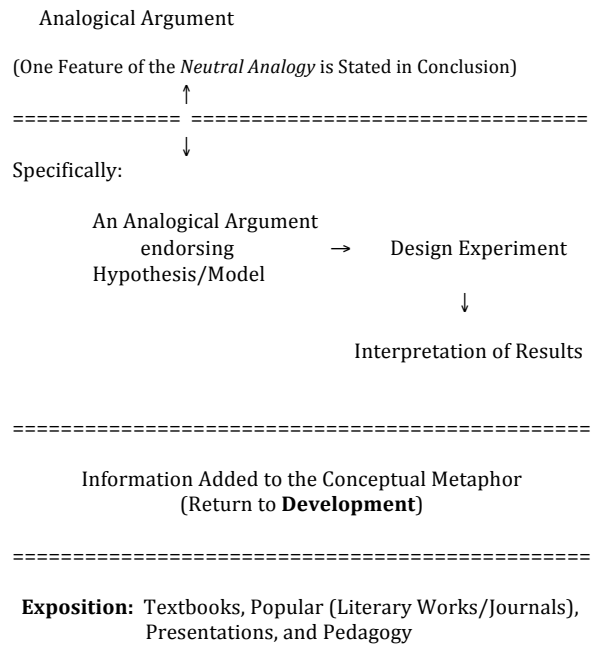
**Discovery:**



**Development:**



**Evaluation:**



With the exception of exposition, the diagram outlines a linear development of scientific research that is guided by a metaphor.<sup>64</sup> Each distinct use of analogical reasoning is associated with different activities and these activities are consistent with our intuitions of scientific practice. For example, scientists rely on an enumeration of similarity to select a potential source analogue.<sup>65</sup> Once they begin to exploit the neutral analogy for possible areas of research, by creating hypotheses and performing experiments, they move from a period of discovery to the development of a theory. In other words, the different uses of analogical reasoning in science result in different activities comprising practice.

The first use of analogical reasoning that Holyoak and Thagard identify as typical in science is **Discovery**. Scientists who encounter an unfamiliar phenomenon in nature look for features that are similar to a better-known entity. For example, Rom Harré notes that when attempting to explain the workings of the natural world, we do not perceive “brute facts” existing in nature (Harré 1988, 123). Instead,

...common experience is first differentiated and categorized with respect to some cluster of loosely organized common sense schemes...Many of these take the form of analogues 'brought up to' items of common experience. They sharpen our grasp of the patterns that are implicit in the experience or that can be made to emerge from it, by the similarities and differences they force us to take account of.

(Harré 1988, 123)

Research in cognitive science suggests that previous judgments about discovery—such as it being merely the result of inspiration and not inference—have been too hasty. As

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<sup>64</sup> By restricting my comments to scientific practice that is based on the use of analogies, it may seem to the reader that I am limiting myself to a very specific type of practice. However, given the metaphorical view of science offered by Max Black, and later developed by Hesse, I believe that my comments apply to a large share of scientific practice. While the reasoning used in scientific practice includes, induction, deduction, analogical and causal types, the use of metaphor and models assume some type of analogical foundation.

<sup>65</sup> This is Aristotle's first definition of analogy described in White (2010).

Holyoak and Thagard point out, “two fundamental processes underlie the use of scientific analogies: the selection of the source and the application of the source to target” (Holyoak and Thagard 1995, 191). In the case of selecting or finding a source, scientists may rely on some type of lower level cognitive process, but in other cases, such as how apt the analogy is to explaining the target, complex reasoning is involved (Holyoak and Thagard 1995, 191-197).

Holyoak and Thagard describe “four ways in which sources can originate: noticing, retrieving, compiling, and constructing” (Holyoak and Thagard 1996, 192). With the exception of construction, these ways are the result of lower level cognitive processes. For example, noticing is the “serendipitous” moment when a source appears by chance, such as Benjamin Franklin’s famous kite experiment (Holyoak and Thagard 1995, 185). In a letter dated November 7, 1749, Franklin embarks on the path of discovery by listing the properties in which “electrical fluid” was similar with respect to lightning.<sup>66</sup>

Cases of retrieving, like noticing, are also the result of lower level cognitive processes. In cases of retrieving, the source is not physically present, but is “retrieved” from one’s memory. As an example, Holyoak and Thagard cite Huygens’ retrieval of what was known about sound to propose a wave theory about light (Holyoak and Thagard 1995, 187 & 192). According to Holyoak and Thagard, “When Huygens thought about light: for example, he may often have been reminded of similar sound phenomena” (Holyoak and Thagard 1995, 192).

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<sup>66</sup> Franklin offers an abstract of how he was inspired to create an experiment:

Nov. 7, 1749. Electrical fluid agrees with lightning in these particulars: 1. Giving light. 2. Colour of the light. 3. Crooked direction. 4. Swift motion. 5. Being conducted by metals. 6. Crack or noise in exploding. 7. Subsisting in water or ice. 8. Rending bodies it passes through. 9. Destroying animals. 10. Melting metals. 11. Firing inflammable substances. 12. Sulphurous smell.—The electric fluid is attracted by points.—We do not know whether this property is in lightning.—But since they agree in all the particulars wherein we can already compare them, is it not probable they agree likewise in this? Let the experiment be made.

(Franklin and Cohen 1941, 111-112).

We rely on memory not only to retrieve a source, but it also serves in cases of compilation. “Instead of retrieving a single source, the scientist recalls various pieces of information that can be melded together to form a source analog” (Holyoak and Thagard 1995, 193). Holyoak and Thagard suggest that Darwin’s gathering of data on artificial selection is an example of compiling (Holyoak and Thagard 1995, 187-188 & 193). Darwin kept meticulous records of his correspondence with breeders and his own researches while on the *HMS Beagle*, and he used this information to create a new source, artificial selection.

The last method described by Holyoak and Thagard, construction, differs noticeably from the previous three. Construction uses the lower level cognitive processes of noticing, retrieving, and compiling to create “a source [that] is different from anything previously known” (Holyoak and Thagard 1995, 193). Holyoak and Thagard cite Kekulé’s discovery of the shape of benzene as an example of construction. According to Holyoak and Thagard, Kekulé did not arrive at the source solely through the workings of his unconscious (Holyoak and Thagard 1995, 193). Instead, his unconscious was part of a cognitive package that included his past attempts at construction as well as the knowledge of biochemistry retrieved from his memory (Holyoak and Thagard 1995, 193).<sup>67</sup> According to Holyoak and Thagard, “it is important to realize that selection of source analogs often involve very complex designs and constructions that go beyond the simple recall of past cases” (Holyoak and Thagard 1995, 194).

The process of finding a source analog includes the particular reasoning associated with discovering a source and assessing its suitability. Once a source has been selected, scientists wanting to do research on an unfamiliar phenomenon do so via a recursive process between the phenomena (target) and a proposed source (indicated by the bi-directional arrows on the diagram). They note the similarities and the differences between the two

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<sup>67</sup> Holyoak and Thagard also point to research done by James Clerk Maxwell and Thomas Morgan. Nancy Nersessian examines Maxwell’s creation of models in her 2008 book, *Creating Scientific Concepts*.

analogues. To further aid in the selection of a source analogue, Holyoak and Thagard posit constraints that help match the pertinent features of the target to the source. As we shall see, these constraints can also be used to assess the answers to Chang's questions about practice presented in the previous section.<sup>68</sup>

Holyoak and Thagard describe "three basic kinds of constraints on analogical thinking" (Holyoak and Thagard 1995, 5). Although specifically intended to work within a general account of analogical reasoning, these constraints—similarity, structure, and purpose—aid in the selection of a source phenomena/entity. For a source to be considered, it must bear some resemblance to the target. For example, Galileo suggested that the earth moved in an orbit like the moon, based on similarities between the two (Galileo 1967, 71-78). Similarities—like being spherical, dense, and having bodies of land and water on the surface—served to "justify drawing the analogy in the first place" (Holyoak and Thagard 1995, 5). Of course, merely identifying similarities between the source and target provides a limited justification for accepting a source. It's not enough that the source and target be similar, but "each element of the target domain should correspond to just one element in the source domain (and vice versa)" (Holyoak and Thagard 1995, 5-6). In other words, the perceived similarities of the source must correspond with those in the target in order to later identify which similarities are relevant to the task at hand.<sup>69</sup> In the previous example, Galileo not only relied on similarity to establish his analogy between the moon and the earth, he could also map the correspondences between the moon and the earth (e.g. the shape of the moon corresponds with the shape of the earth).

Once the correspondences between source and target are mapped, scientists can further assess the suitability of the source by consulting the third constraint, *purpose*

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<sup>68</sup> In fact, I will use constraints in the final chapter to help a community assess whether a critique from outside of the community is relevant or not.

<sup>69</sup> Structuralist accounts, like those tendered by Holyoak and Thagard or Deidre Gentner, maintain that the number of similarities contribute to the strength of an analogical argument. This does not take into account whether the similarities are relevant or not (Bartha 2010, 71).

(Holyoak and Thagard 1995, 6). In everyday reasoning, “the exploration of the analogy is guided by the person’s goals in using it, which provide the *purpose* for considering the analogy at all” (Holyoak and Thagard 1995, 6). For scientists, the overall goal or question about the phenomenon plays a role in the selection of a source analog because the features of the target that scientists seek to understand constrain what source might be epistemically promising.<sup>70</sup> For example, if Galileo wanted to know whether the earth moved in an orbit like the moon, he would not choose an immobile entity, like a rock, as a source. Instead, he would choose a source, which exhibited a similar behavior. At the time, the moon was understood to orbit the earth and thus, made a better choice for a source than another immobile entity.

Holyoak and Thagard point out that these constraints “do not operate like rigid rules dictating the interpretation of analogies” (Holyoak and Thagard 1995, 6). Instead, the constraints assist in coming up with a source analog. The constraints make no guarantee as to the epistemic success of the source nor do they offer guidance in selecting one source over another. As Holyoak and Thagard note, “Some of the mental leaps accomplished by analogy have ended in creative triumphs; others have ended in dismal failures” (Holyoak and Thagard 1995, 7). The activities of discovery include the selection a source and the choice, given the constraints upon analogy, is not “blind or random” (Holyoak and Thagard 1995, 7).<sup>71</sup> Because there are constraints on the selection of a source, the answers scientists give to Chang’s questions about practice can be subjected to critique. As we have seen, this type of reasoning precedes activities and so the focus on it instead of on verbs is more fundamental. In addition, questions about the use of analogy, particularly those directed at the purpose for

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<sup>70</sup> This is the use of analogy in science differs from that of literature or art. The purpose in science is to answer questions about a given phenomenon, and not just any source will have the resources to accomplish this goal. In literature, the emphasis seems to be on novelty, whereas in science, the emphasis should be epistemic promise.

<sup>71</sup> Thus, accounts that claim discovery lacks any type of reasoning fail to consider the processes supporting the selection and assessment of a source analogue.

the analogy and the scientist/community's goals for using the analogy, could serve to demarcate scientific practice.

The second use of analogy in science posited by Holyoak and Thagard is **Development**, and it signals a change in the community's use of analogy.<sup>72</sup> Scientists identify a neutral analogy—the set of properties for future investigation—and no longer rely solely on the similarities and differences between the source and target.<sup>73</sup> Instead, the use of analogy assumes the role of a metaphor that develops a theory by providing ways in which to create and test hypotheses. According to Holyoak and Thagard, analogical reasoning contributes to the development of a hypothesis in two ways.<sup>74</sup> The first is “theoretical, in which a hypothesis is refined and linked with other hypotheses” (Holyoak and Thagard 1995, 190). As an example of theoretical development, Holyoak and Thagard refer to Darwin's theory of natural selection, stating “Darwin stressed how useful the natural selection/artificial selection analogy was to him in constructing explanations and dealing with objections to his theory” (Holyoak and Thagard 1995, 190).<sup>75</sup>

The second way in which scientists use analogical reasoning to develop a hypothesis is through the design of experiments. According to Holyoak and Thagard, “the empirical consequences of a hypothesis are worked out and translated into feasible experiments” (Holyoak and Thagard 1995, 190). For example, Franklin's electricity/lightning analogy is a

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<sup>72</sup> At this point, the community's use of analogy assumes the form of a metaphor that suggests a research program. This metaphor will motivate the analogical arguments discussed in the next phase, **Evaluation**.

<sup>73</sup> The *negative* analogy is present in the stage of discovery even though it seems to function as a constraint on the analogy. That is to say, in the presence of a relevant difference/s, the analogy may fail to get uptake, or be deemed unsuitable.

<sup>74</sup> Holyoak and Thagard suggest, “even if an analogy does not initially form a hypothesis, it can aid greatly in its development” (Holyoak and Thagard 1995, 190).

<sup>75</sup> For example, Darwin admits in a letter to Alfred Russel Wallace that his latest edits of the 5<sup>th</sup> edition of the *Origin* was improved given his responses to the critique written by Fleeming Jenkin. See Darwin (1887), vol. 3 p. 107.

In a second letter to Wallace, Darwin writes

I must have expressed myself atrociously; I meant to say exactly the reverse of what you have understood. F. Jenkin argued in the 'North British review' against single variations ever being perpetuated, and has convinced me, though not in quite so broad a manner as here put. I always thought individual differences more important; but I was blind and thought that single variations might be preserved much oftener than I now see is possible or probable.

(Darwin 1887, vol. 3, p. 107)

“clear case of how analogy can serve to develop experiments” (Holyoak and Thagard 1995, 190). Franklin relied on the similarities between source and target to design his famous kite experiment. Of course, such analogies need not lead to hands-on experiments. The positive, negative, and neutral analogies may inspire *gedanken*-experiments as well.<sup>76</sup> These experiments are generally conducted in the mind and not in the laboratory. Rom Harré suggests that we might understand the behavior of phenomena by positing an idealized model (Harré 1988, 121). For example,

Often an iconic model is imagined, and its behaviour studied in a *gedanken*-experiment. The internal circulations of droplets falling in different media can be studied by imagining that a droplet is enclosed in a rubber balloon, that is, in a surface which is materially distinct from the liquid within. In this way surface tension effects can be neatly separated from internal processes, with the advantage of clear vision of the convection flows within.

(Harré 1988, 122)

Harré also points out that “when Darwin imagines what would happen if there were natural selection, the results of his *gedanken*-experiments must square with the results of applying the coordinate analytical analogue [source] to nature” (Harré 1988, 127). In the case of Darwin’s work on natural selection, the point of discovery is the link between the presence of naturally occurring species and the population work by Thomas Malthus combined with the experiences from domestic breeding.<sup>77</sup> During development, however, Darwin begins to fill out an analogy in his notebooks, using these sources. The resulting conceptual argument is later found in the first chapters of the *Origin*.<sup>78</sup> Thus, *gedanken*-

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<sup>76</sup> For example, according to Harré (1988), “We know from experiment and observation, within the conceptual possibilities constrained by the analytical analogue [source], how the real productive mechanism behaves. We imagine, through the joint constraints of the behavioural and material analogies, what that mechanism or process might be like” (Harré 1988, 127).

<sup>77</sup> This is supported by the work of Lindley Darden (1982), who also suggests that Darwin had two sources of inspiration: the selection of domestic breeds and Malthus’ theory of competition in light of scarce resources (152-156).

<sup>78</sup> I take the *neutral analogy* of the metaphor to be the future research that Darwin suggests in the later third of the book.

experiments, like their more tangible counterparts, are in a position to support or deny scientific claims.

The use for analogical reasoning in Holyoak and Thagard's **Evaluation** phase includes the analogical arguments motivated by the neutral analogy of a metaphor that supports a theory. Scientists create their analogical arguments by isolating one member of the *neutral analogy* (taking it as the basis for the conclusion) and using the results of their experiments to confirm whether the member belongs in the positive or negative analogues. For example, William Gilbert used a *earth is a small magnet* metaphor to establish that earth behaves like a magnet and Galileo relied on the *earth as the moon* or *earth as a ship* metaphors to ultimately support his theory that the "earth does in fact move" (Holyoak and Thagard 1995, 186-187 & 190-191). The metaphors to which Galileo appealed illustrate how analogical reasoning differs when evaluating a hypothesis (argument from analogy) as compared to developing a theory (metaphor).

Working from the *earth as moon* metaphor, Galileo established the possibility that the earth revolves around the sun. Argument III, below, is a tabular representation of Galileo's argument based on the *earth as moon* metaphor.

Argument III

<b>Source:</b> Moon	<b>Target:</b> Earth
Spherical shape	Spherical shape
Dense and solid	Dense and solid
Expanses of light and dark	Expanses of light and dark
Expanses of land and sea	Expanses of land and sea
Moves in orbit	?

Based on the similarities between the earth and the moon, Galileo argued that the earth moved. Argument III established the possibility that earth moved; however, another argument was required to reject the opposing theory that the earth was stationary.

According to Holyoak and Thagard, Galileo used the *earth as a ship* metaphor to argue against the popular opinion that the earth was stationary (Holyoak and Thagard 1995, 186-187). “Opponents of [Galileo’s] theory argued that if a rock is dropped from a tower, it lands at the base of the tower, suggesting that the tower, and hence the earth, is not in motion” (Holyoak and Thagard 1995, 186). Because the rock did not land far horizontally from the point it was released, the consensus at the time was that the earth did not orbit the sun. Galileo, however, recognized that the tower was similar to the mast of a ship (Holyoak and Thagard 1995, 186). Using a lead ball dropped from the mast of a moving ship, he demonstrated that such objects do not fall far from their release point as well (Galileo 1967, 146). Both sides—those who endorsed a stationary earth hypothesis and those who

believed the earth moved in an orbit—reasoned from a conditional state of affairs.<sup>79</sup> What made Galileo’s argument persuasive was that the *earth as ship* metaphor included a relevant similarity in the positive analogy that appeared in the premises; both the source and the target assumed movement. The opposition’s argument did not. Galileo’s metaphors worked together to support the possibility of a moving earth: the first suggested an earth that moved and the second refuted the competing theory that the earth was stationary.

Not every scientific disagreement is resolved by means of a single, crucial experiment. Generally, when scientists design experiments based on the neutral analogy of the metaphor, the resulting data motivates further questions, or may even require additional clarification. While Diagram 1 describes the different phases of scientific practice, it does not fully represent the iterative process of research guided by metaphor. Diagram 2, below, depicts the recursive exchange that occurs between the developmental and the evaluative phases of scientific practice.

As we have seen, the positive analogy, the negative analogy, and the neutral analogy are the sets of correspondences—similar, dissimilar, and hypothetical—associated with a particular metaphor. Scientists rely on these analogies to motivate the first formulations of analogical arguments that endorse a particular hypothesis, which is a member of the neutral analogy. They design their experiments and gather data in order to resolve whether the correspondence under investigation should be incorporated into the positive and/or negative analogies of the metaphor. A modified version of the metaphor—one that has the new information added to its positive or negative analogies—generates new analogical arguments that contain the previously incorporated information in its premises. Thus, the process continues until the metaphor loses favor or is abandoned by the community.

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<sup>79</sup> For example, if the earth moves in an orbit, then they would expect certain events to occur.

Diagram 2. The Recursive Process of Science Guided by Metaphor

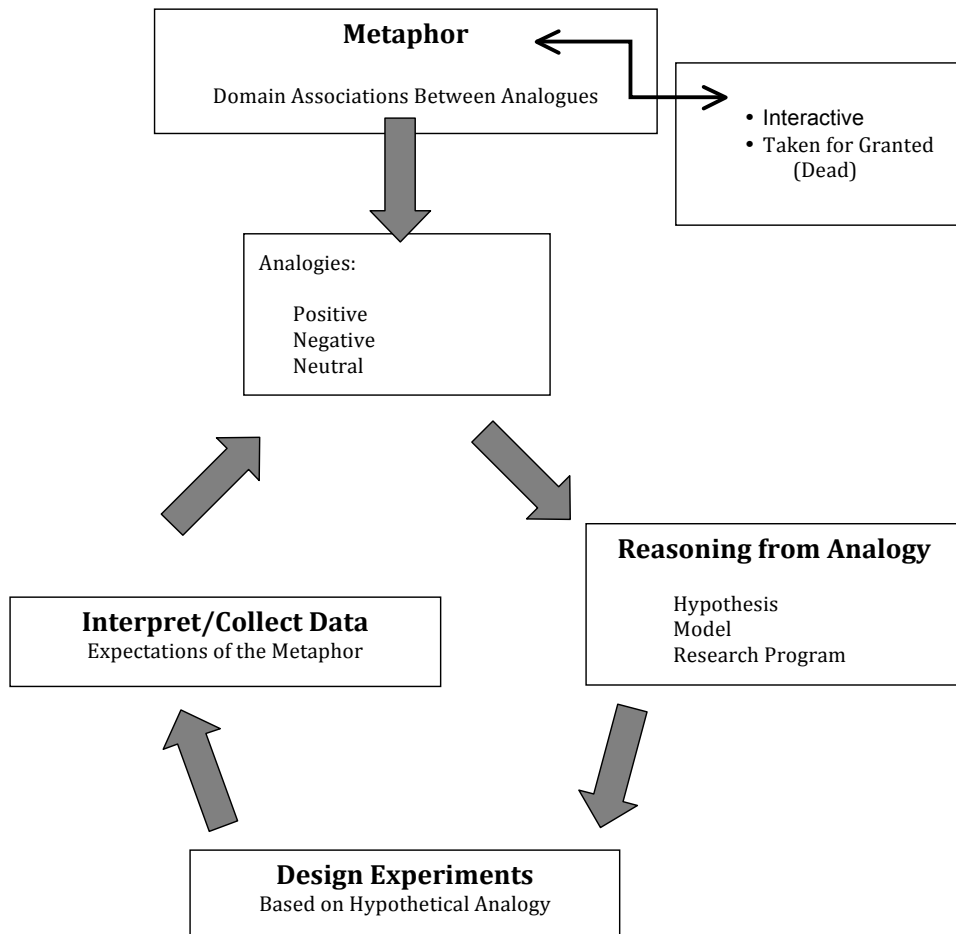


Diagram 2 reflects the relationship between the broad role for analogy (metaphor) and its narrow role (analogical arguments). Both roles are interdependent; the framework assumptions of the metaphor (the positive, negative, and neutral analogies) provoke particular formulations of arguments and designs of experiments. The resulting data contribute to the modification of the metaphor. The problem of ingrained analogy affects this particular process. When one or more of the members in the positive or negative analogies is taken for granted, the subsequent arguments and later iterations of the metaphor could be adversely affected. I return to this process in Chapter Four and lay out the problem in greater detail.

The final use of analogical reasoning cited by Holyoak and Thagard is **Exposition**. In this phase, analogy functions as a heuristic or as a part of pedagogy, such as visual representations or verbal explanations of phenomena. Expository analogies need not be extensive. Amongst the analogies cited in Behar *et al.* (2013) “Twenty-Five Analogies for Explaining Statistical Concepts” some are quite simple. For example, instructors might refer to the concept of innocence in a court of law to explain the concept of the null hypothesis.

Behar *et al.* explain

In a trial, the null hypothesis is innocence. The objective is not to demonstrate that the accused is innocent but to see if the evidence (the data) contradicts this hypothesis. If there is no evidence, the accused cannot be declared guilty, but this does not mean innocence has been proven.

(Behar, *et al.* 2013, 46).

The familiar concept of evidence is a way to help students studying statistics to grasp the inability to prove the null hypothesis. This analogy requires very little from either student or instructor, except a familiarity with the court system.<sup>80</sup>

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<sup>80</sup> Of course, this assumes the common language posited in Black (1962, 40)

Other expository analogies may be more complex, involving abstract representations and relations. For example, according to research conducted by Laura R. Novick and Kefyn M. Catley,

Diagrams have long been an integral part of biology because (a) they depict the way things are or are hypothesized to be and (b) they are important tools for learning, reasoning about, and communicating structures, processes, and relationships (Clark, 2001; Kindfield, 1993/1994; Novick, 2006).

(Novick and Catley 2007b, 197)

For example, cladograms are “used to depict *phylogenies* (evolutionary histories among species or groups of species)”(Novick and Catley 2007b, 197). In fact, Novick and Catley note, “cladograms are hypotheses about nested sets of taxa that are supported by shared evolutionary novelties called *synapomorphies* (Hennig, 1966)” (Novick and Catley 2007b, 197). According to the following cladogram in Figure 1, the ability to see UV light is shared by both birds and lizards and at some point in their histories, each had the common ancestor who “evolved” this ability (Novick and Catley 2007b, 197). Because birds and lizards have a “most recent common ancestor (MRCA),” they are believed to be more closely related to each other than lizards would be to other groups, such as spiders. Spiders and lizards share a common ancestor, one that evolved segmentation, although it is less recent than the ancestor shared with birds (see UV light).

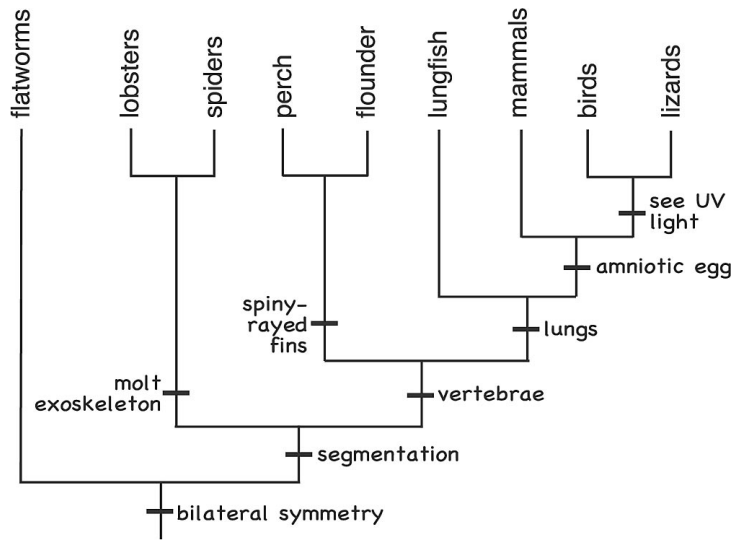


Figure 1. A cladogram showing a subset of the synapomorphies that support the evolutionary relationships among the nine taxa depicted.

(Novick and Catley 2007b, 198)

Cladograms are useful because

the patterns of evolutionary relationships observed today are the result of many processes involving the interaction of, for example, geological events, extinction, speciation, and the formation of higher taxa. Because these changes cannot be seen during the course of a human lifetime, understanding macroevolutionary processes can be difficult for students and other nonspecialists.

(Novick and Catley 2007b, 198)

Cladograms can be drawn in two formats: the tree (as in Figure 1) or as a ladder (see the comparison between a tree and a ladder below) and “such diagrams are included in contemporary textbooks from middle school life science through high school biology to college-level biology, zoology, and botany”(Novick and Catley 2007b, 199).<sup>81</sup>

<sup>81</sup> Novick and Catley maintain that the ladder-shaped cladograms are more difficult to understand than the tree-shaped ones (Novick and Catley 2007b, 216). According to Novick and Catley,

We hypothesized that the ladder format is more difficult to understand than the tree format because the interpretation suggested by the Gestalt principle of good continuation is at odds with the correct interpretation. The results of this experiment support our hypothesis that this perceptual principle interferes with participants’ ability to (a) extract the critical structural information regarding

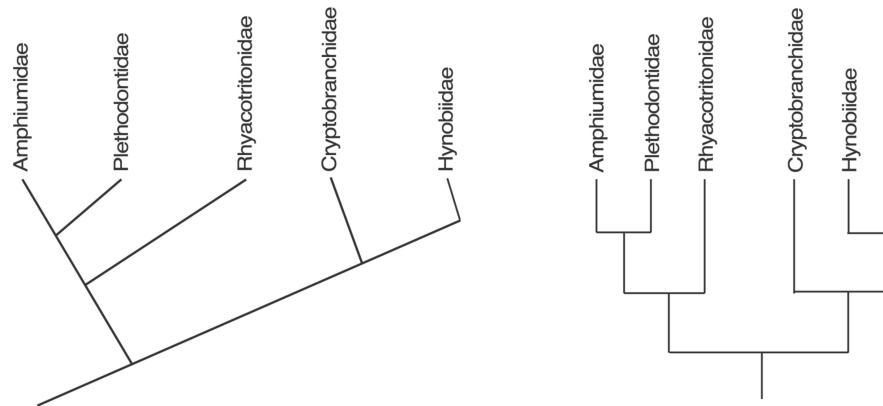


Figure 2. An example of a ladder diagram with an angled branch at the top right that received partial credit and the isomorphic structure rendered in a tree diagram.

(Novick and Catley 2007b, 204)

Scientists or instructors may refer to the source analogue when creating their expository analogies, or they may use something completely different, something that has pedagogical value. While both cases are affected by the problem of ingrained analogy, the effects of an ingrained analogy on expository analogies differ from the uses of analogical reasoning during development and evaluation. Expository analogies indirectly contribute to the self-reinforcing nature of a metaphor through its influence on the scientific community. That is to say, the training that researchers receive may be what prevents them from providing robust critique. This will become apparent once I address how to incorporate novel, external critique into the community.

Holyoak and Thagard outline four uses of analogical reasoning in science, and these different types appear to provide a delineation of the different actions/phases of scientific

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hierarchical levels from a ladder and (b) create ladders that appropriately depict a given hierarchical structure.

(Novick and Catley 2007b, 216)

This is important for educators in biology since the majority of textbooks use the ladder format, see (Novick and Catley 2007a).

practice. Because different forms of analogical reasoning are associated with different activities, an account of scientific practice should include an examination of the uses of analogical reasoning, and this will vary depending on what phase of scientific practice is at issue. Like Chang's account, the activities connected to analogical reasoning will lead to questions of context about practice. What differs from Chang's account is that a focus on analogical reasoning motivates other philosophically interesting questions in addition to those cited by Chang. The constraints placed on the use of analogy, particularly why a particular source is selected, indicate the goals and purposes of either the individual members and/or their communities, which is key to delineating scientific practice as well as contributing to the critique of metaphor.

As we have seen in the discussion of analogical reasoning in scientific practice, Holyoak and Thagard assume that Darwin's methodology in the *Origin* involves the use of analogical reasoning in the broad sense—metaphor—and the narrow sense, an analogical argument. As we will see in the next chapter, this is consistent with the standard interpretation of the *Origin*.<sup>82</sup> Recently, this view has come under attack. If Darwin did not rely on an analogy to support his theory, then my argument for examining Darwin's community as a source for dealing with an ingrained analogy is moot. In the next chapter, I address the objections to the standard interpretation, demonstrating how each relies on a naïve account of analogy. By successfully responding to these objections, I not only preserve the status of Darwin's community as an exemplar of robust critique, but also show that relevance is key to the assessment of an analogy.

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<sup>82</sup> See also *fn. 1*.

## Chapter Two

### Darwin, the *Origin*, and Analogical Reasoning

In the last chapter of the *Origin*, Charles Darwin describes his most famous work as “one long argument,” and the scholars who analyze his methodology take Darwin at his word (Darwin 1859, 459). While most scholars maintain that the *Origin* contains some form of analogical reasoning, they differ as to the overall structure of the work. For example, C. Kenneth Waters notes

Philosophers analyzing the logic and structure (or the form and strategy) of the *Origin* have adopted two different approaches. Some have appealed to contemporary ideals of science to elucidate the deep logic of Darwin’s argumentation (Lloyd 1983, Philip Kitcher 1993a, Thagard 1978, Waters 1986). Others stress the ideals of Darwin’s day to clarify his reasoning and explain the structure of the *Origin* (Hodge [1992], Recker 1987, Ruse 1979).

(Waters 2003, 138)

Of the two approaches described by Waters, the arguments found in Doren Recker (1987) and Michael Ruse (1979) exemplify what I refer to in this chapter as the standard interpretation.<sup>83</sup> According to this view, Darwin relies on an argument from analogy as part of his overall argument for evolution by means of natural selection.

Recently, the standard view has come under attack, particularly its assertion that analogy plays a justificatory role in the methodology of the *Origin*. Richard A. Richards

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<sup>83</sup> Arguably, there are other accounts that could be described as the standard account of Darwin’s use of analogical reasoning. I have specifically chosen Recker (1987) and Ruse (1979) for two reasons. First, it is this interpretation that is directly challenged by Richard A. Richards (1997, 1998, 2005) and Peter Gildenhuys (2004). Second, Recker and Ruse both attempt to explain Darwin’s methodology within a contextual, historical framework, which will be informative when discussing the community that witnessed the publication of the *Origin*.

(1997, 1998, 2005) and Peter Gildenhuys (2004) take a historical approach, but challenge the standard view by basing their claims on how they believe analogy was understood at the time. As we shall see, this debate is not wholly novel. In fact, their objections are successors to a more fundamental debate “over the nature of inductive reasoning in science” initiated by John Stuart Mill and William Whewell in the middle of the 19<sup>th</sup> century (Snyder 1997, 162). Both Mill and Whewell were motivated by a strong desire to reform British society, believing the best way to begin was by improving the methods of Victorian science—particularly the use of induction introduced by Francis Bacon in the *Novum Organum*. (Snyder 2006, 7-8).<sup>84</sup> Notably, Whewell included analogy in his account of induction, while Mill did not.<sup>85</sup> Ultimately, Whewell’s account of induction, which was linked with intuitionism, never gained widespread acceptance (Snyder 2006, 326).<sup>86</sup> Instead, Victorian society embraced the empiricism and utilitarianism endorsed by Mill’s account.<sup>87</sup> Induction, for Mill, was “a narrow logical operation involving only enumeration or eliminative forms of reasoning,” and as we shall see, this affected how analogical reasoning was conceived of in modern philosophy of science (Snyder 2006, 331).<sup>88</sup>

There is more at stake in the Mill-Whewell debate, and in the critiques by Richards and Gildenhuys, than the correct interpretation of a highly influential scientific work. Taken by some to be a potential exemplar of analogical reasoning in science, the specific ways in which Darwin relies on an analogy motivates my normative account of scientific practice. If the

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<sup>84</sup> Laura Snyder states, “Both men believed that proper scientific method could aid in renovating not only science but also morality and politics” (Snyder 2006, 4). She also notes that both men were interested in political economy, ethics, and social conditions, even voicing opinions on slavery in the United States (Snyder 2006, 1-5).

<sup>85</sup> As we shall see, Mill thinks analogy is useful for suggesting hypotheses, but it does little to justify a hypothesis. His praise for the *Origin* attests to this. See Mill 1900, p. 355 and Snyder 2006, p. 194.

<sup>86</sup> For example, Whewell provided such terms as, anode, cathode, and ion at the request of Michael Faraday (Snyder, 2009). According to Snyder, Whewell wrote on such subjects as “architecture, crystallography, educational reform, geology, international law, mechanics, mineralogy, morality, natural theology, political economy, and the science of the tides, and translated Plato’s dialogues as well as German novels and poetry” (Snyder 2006, 1). Notably, Whewell is believed to be the first to use the term “scientist” as well. See Ross (1962) p. 72.

<sup>87</sup> See both Hull (1973) p. 5 and Snyder (2006) p. 326. It should also be noted that Whewell died nine years prior to Mill and that Mill was a visible member of British Parliament (Snyder 2006, 323).

<sup>88</sup> The book had a total of eight editions from 1843 to 1872 (Jacobs 2003, 202).

standard interpretation is correct and Darwin does indeed draw on various forms of analogical reasoning, then what we know about the historical critiques of the *Origin* can be brought to bear on questions regarding the social epistemology of scientific practice, especially the activities associated with the practice of models, such as knowledge production and the design of experiments.<sup>89</sup> In this chapter, I argue against Richards and Gildenhuys, claiming that their accounts rely on an impoverished understanding of analogy. The use of analogical reasoning in science differs from that in literature or in the arts. Not only should the similarities between analogues be relevant to the member of the neutral analogy under investigation, there should not be any differences that might weaken this relation. Because Richards and Gildenhuys embrace a naïve account of analogy, they ignore considerations of relevance and thus, their interpretation of the *Origin* suffers. Not only do they miss the mark with respect to Darwin’s use of analogy, they fail to recognize the community’s contributions with respect to providing a robust critique of the more sophisticated forms of analogical reasoning. Once I have established that the standard interpretation of the *Origin* is correct—and that analogy was regarded as a legitimate tool for scientific justification in the 19<sup>th</sup> century—an important question to ask is “Why was analogy not considered a legitimate scientific inference post Darwin?”

## **§2.1 The Standard Interpretation of Darwin’s Analogy in the *Origin***

There is an extensive literature addressing Darwin’s use of analogy in the *Origin*, but until Richards’ objection, no one questioned whether Darwin actually draws an analogy between artificial and natural selection.<sup>90</sup> Instead, the discussion centered on the role

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<sup>89</sup> Although I have limited the discussion to the historical critiques, the contemporary critiques of Richards and Gildenhuys also contribute to a robust critique of analogy (at the meta-level). For example, these critiques motivate questions about whether there is an analogy and what standards the community recognizes.

<sup>90</sup> Previous literature includes the historical reviews of the *Origin* contained in David Hull (1973) and more recent accounts offered by Michael Ruse (1971, 1973, 1975, 1978, 1979), Hodge (1977, 2013), Lloyd (1983), L.T. Evans

analogy plays in the *Origin*, such as whether it functions solely as a heuristic device or as a justificatory inference. In this section, I present the standard view, relying on the accounts offered by Michael Ruse and Doren Recker. Both Ruse and Recker argue that Darwin relied on an argument from analogy as a justificatory inference in the *Origin*, and Recker's account, particularly his claim that Darwin draws an analogy between artificial and natural selection because it would be "causally efficacious" to do so, is singled out by Richards and Gildenhuys (Recker 1987, 165).

The accounts put forth by Recker and Ruse regarding Darwin's analogy in the *Origin* are strikingly similar. For example, Recker and Ruse believe that Darwin exploited an analogy with artificial selection to establish natural selection as a *vera causa*, in the Herschellian sense (Recker 1987, 162-165 & Ruse 1975, 175-177). The term, *vera causa*, is traced back to Isaac Newton's *Principia* and his " 'First Rule of Reasoning' (Recker 1987, 161).<sup>91</sup> According to Newton, when attempting to explain phenomena, "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances" (Newton 1962, vol. II, p. 398). In other words, one cannot posit an explanation that goes beyond the available evidence. As we shall see, there are different methods for establishing a *vera causa*.

Ruse's account of Darwin's use of analogy presupposes the methods set forth by Herschel in *A Preliminary Discourse on the Study of Natural Philosophy*. Ruse believed that "the key to a [Herschellian] *vera causa* was an *analogy*" (Ruse 1978, 324). For example, Herschel states

If the analogy of two phenomena be very close and striking, while, at the same time, the cause of one is very obvious, it becomes scarcely possible to refuse to admit the action of analogous cause in the other, though not so obvious in itself. For instance, when we

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(1984), Robert Young (1985, 1993), Ken Waters (1986), Doren Recker (1987), Ernst Mayr (1991), Daniel Dennett (1995), S. Schweber (1977), S. G. Sterrett (2002), and P. Gildenhuys (2004).

<sup>91</sup> A *vera causa* is a cause that really exists in nature (Whewell 1860, 191).

see a stone whirled round in a sling, describing a circular orbit round the hand[...]we never hesitate to regard it as retained in its orbit by the tension of the string, that is by a *force* directed to the centre; for we feel that we do really exert such a force. We have here *the direct perception* of the cause.

(Herschel 1987, § 142)<sup>92</sup>

Herschel claims that when we observe a similar phenomenon, such as the moon circling the earth, we cannot help but posit a cause similar to that observed in the stone and sling example: “a *force* directed constantly to the centre” (Herschel 1987, § 142).<sup>93</sup> Ruse links Darwin’s analogy between artificial and natural selection with Herschel’s description of a *vera causa*, stating “we have in artificial selection, a *force directly perceived and caused by us*; hence analogically, given the struggle and given wild variation, it cannot be denied that there is a natural force of selection making different organisms, just as man makes different organisms” (Ruse 1975, 176).<sup>94</sup> According to Ruse, Darwin “begins with Malthusian premises that organic beings tend to increase at a geometric rate and that increase outstrips supplies of food [...] This leads to the struggle for existence” (Ruse 1979, 190). After introducing the struggle for existence, Ruse contends that Darwin “adds in claims, based on analogy from the domestic world, that some of the variation in nature will help in the struggle and some will hinder, and thus neatly implies natural selection...” (Ruse 1979, 190).

For Recker, the overall structure of the *Origin* amounts to three “independent but integrated argument strategies” supporting the “probable ‘causal efficacy’ of natural selection (Recker 1987, 165). Of Recker’s three strategies, the first, referred to as the ‘Empiricist *Vera Causa* Strategy’ (EVC), is found in the first four chapters of the *Origin* (Recker 1987, 165).<sup>95</sup> Recker claims that Darwin creates an analogy between the two

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<sup>92</sup> Emphasis in original.

<sup>93</sup> Emphasis in original.

<sup>94</sup> Emphasis in original.

<sup>95</sup> The EVC is part of Ruse’s analysis and terminology. It is found in Ruse (1979), particularly in Ruse’s diagram of “The three key structural elements of Darwin’s theory” on page 198.

processes by pointing out the similarities between artificial and natural selection in Chapters I-III (Recker 1987, 166).

At the beginning of Chapter IV, Recker claims that Darwin makes three appeals to artificial selection (Recker 1987, 166).<sup>96</sup> The first appeal is the claim that “artificial selection is *known* to be potent in accumulating variations in a definite direction in domestic cases,” which causes Darwin to ask whether some similar process exists in nature (Recker 1987, 166). For Recker, this first appeal highlights Darwin’s intention to draw an analogy, implying that the causal efficacy of natural selection arises from the similarities between it and artificial selection (Recker 1987, 166). The second appeal is Darwin’s claim that in cases of domestic breeding the “organisation becomes in some degree plastic” (Darwin 1859, 80).<sup>97</sup> Darwin asserts that we can observe changes in the offspring of domesticated animals as a result of breeding **and** that this process takes place in nature as well because all living organisms are capable of change (Recker 1987, 166). Darwin’s final appeal to artificial selection comes when he asks “Can it, then, be thought improbable, seeing that variations useful in some way to each being in the great and complex battle of life, should sometimes

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<sup>96</sup> At the beginning of Chapter IV, Darwin asks,

Can the principle of selection, which we have seen is so potent in the hands of man, apply in nature? I think we shall see that it can act most effectually. Let it be borne in mind in what an endless number of strange peculiarities our domestic productions, and in a lesser degree, those under nature, vary, and how strong the hereditary tendency is. Under domestication, it may be truly said that the whole organisation becomes in some degree plastic. Let it be borne in mind how infinitely complex and close-fitting are the mutual relations of all organic beings to each other and to their physical conditions of life. Can it, then, be thought improbable, seeing that variations useful in some way to each being in the great and complex battle of life, should sometimes occur in the course of thousands of generations? If such do occur, can we doubt (remembering that many more individuals are born than possibly survive) that individuals having any advantage, however slight, over others, would have the best chance of surviving and procreating their kind? On the other hand, we may feel sure that any variation in the least degree injurious would be rigidly destroyed. This preservation of favourable variations and the rejection of injurious variations, I call natural selection.

(Darwin 1859, 80-81)

<sup>97</sup> What Darwin means by “organization” is the sum total of an organism’s traits. For example, the organization of a domestic sheep would include its type of wool, structure of its limbs, and any trait that could be modified as a result of mating two sheep.

occur in the course of thousands of generations?” (Darwin 1859, 80). This particular question emphasizes a similarity between the processes of selection in domestic cases and those occurring in nature: the accumulation of traits. For example, domestic stock, at the hands of breeders, has changed drastically from the original forms. Animals with the best meat or fiber (given the breeder’s aim) are preserved while animals without the particular trait are culled from the flock. Over time, this creates great physical differences between animals.<sup>98</sup> An analogous process occurs in nature—individuals without a specific, beneficial trait find it harder to compete with the other advantaged individuals, and ultimately become extinct.

These three appeals to domestic cases—the process of selection, that changes are inherited, and that some of these changes contribute to an individual’s survival/reproduction—are best viewed “as presenting and supporting the analogical *goal* of [Darwin’s] argument, and to view the entire argument as supporting the causal efficacy of natural selection by appealing to *known* causal efficacy of artificial selection” (Recker 1987, 166). In other words, given the analogy between artificial and natural selection and what we know about the process of selection in domestic productions, we can infer a similar process in nature. Further, Chapters I through III support the analogical argument by “make[ing] all the similarities between artificial selection and natural selection as striking as possible” (Recker 1987, 167). For example, Recker claims “The first and third chapters appeal to the power of selection, both artificial and natural, the latter being even more powerful than the former since there are important limitations on human selection, and nature has vast amounts of time in which to ‘act’, etc.” (Recker 1987, 166).

As Recker points out, Darwin identifies features of artificial selection that would be similar to those features found in nature (Recker 1987, 166). These are “a high degree of

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<sup>98</sup> Of course, breeders also select for certain behaviors, such as temperament. Bird dogs that are gun shy are not allowed to mate because their behavior prevents them from learning to return with their quarry.

variability,” “a large number of individuals,” and “preventing crosses” (Darwin 1859, 41-42). Given the similarities listed in Chapters I-IV and the appeals to natural selection in Chapter IV, Recker suggests that Darwin is attempting to establish a Herschellian *vera causa*.

Up to this point, Recker’s account is similar to Ruse’s account; he distinguishes his view from Ruse’s when it comes to explaining the overall structure of the *Origin*.<sup>99</sup> Where Ruse believes that the analogy ultimately contributes to a hypothetico-deductive argument grounded by an analogical argument, Recker believes that the overall strategy is initially a Herschellian *vera causa*, which is later combined with the explanatory power of a Whewellian *vera causa* (Recker 1987, 167). As we saw with Herschel, a *vera causa* is established through an analogy with an observed cause. For Whewell, a *vera causa* is a true cause, or “individual event kinds [that are subsumed] into a more general kind composed of sub-kinds that share a kind essence” (Snyder 2006, 174). The resulting general causal law arises from a consilience, or recognition, of the shared essences of the different phenomena under investigation (Snyder 2006, 175).<sup>100</sup> A general causal law can be tested when it is able to subsume different sub-kinds (Snyder 2006, 175). According to Recker, when Darwin appeals to what could be explained by natural selection, he is appealing to a Whewellian *vera causa* (Recker 1987, 171-173).<sup>101</sup> Despite this difference, the accounts of Ruse and Recker comprise what I refer to as the standard view of Darwin’s use of analogy in the *Origin*.

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<sup>99</sup> Recker also distinguishes his view from that in Thagard (1978) and Lloyd (1983). Thagard claims that Darwin was influenced more by Whewell, who believed that *vera causa* could not be established via an analogy. Instead of a justificatory inference, the analogy has “heuristic value” and had explanatory power (Thagard, 1978, 356). Lloyd writes that “the analogical argument serves as supporting evidence for the basic construction of model types” according to a semantic view of theories (Lloyd 1983, 120). According to Lloyd, “These model types serve as formats for explanations; the particular terms or factors in a model type vary in each application, depending on the outcome of the model and various assumed conditions” (Lloyd 1983, 119). These model types are numerous and enabled Darwin to make weak predictions from more specific versions (Lloyd 1983, 124-126). The analogy, for Lloyd, is an inference, but it is not used to establish causal efficacy and is not considered by Richards and Gildenhuys.

<sup>100</sup> According to Laura Snyder, a “consilience is importantly different from the type of reasoning known as ‘inference to the best explanation’ [...] the causal law is not imposed from above as a means of tying together different kinds. Instead, it wells up from beneath in each separate case” (Snyder 2006, 175). A consilience is “separate lines of induction [that] lead from each event kind to the same causal mechanism” (Snyder 2006, 175).

<sup>101</sup> Recker refers to this as the Explanatory Power Strategy; see Recker (1987) p. 173.

## § 2.2 Richards and the Inefficacy of Darwin's Analogy

The argument that the Fixed Stars are like the Sun, and therefore the centers of inhabited systems as the Sun is, is sometimes called an argument from Analogy; and this word *Analogy* is urged, as giving great force to the reasoning. But it must be recollected, that precisely the point in question is, whether there *is* an analogy.

William Whewell<sup>102</sup>

In his book, *Of the Plurality of Worlds*, William Whewell notes that mere similarity is not enough to support an argument from analogy. For an analogical argument to be strong, the similarities must not only be numerous, but also relevant to the phenomenon under investigation; more importantly, there should be no relevant differences between the two analogues (Whewell 1854, 185-186).<sup>103</sup> Did Darwin use an analogy in the *Origin*? Most Darwinian scholars believe he did, and given the volume of literature devoted to the overall structure of Darwin's argument in the *Origin*, Richard Richards' objection, that Darwin did not rely on analogy as a justificatory inference, is unexpected.<sup>104</sup>

Not only does Richards' objection challenge the standard interpretation of Darwin's most famous work, but his challenge also threatens considerations that are essential to the social epistemology component of my account of analogy. If Richards is correct and Darwin did not use analogical reasoning in the *Origin*, then it makes little sense to recommend Darwin's community as an environment capable of robust critique of analogy. I contend, however, that Richards makes use of an impoverished definition of analogy, which underwrites his objection to the standard. In addition, I suggest that the community that witnessed the publication of the *Origin* performed a valuable function in the critique of Darwin's use of analogical reasoning, and that scientific communities today can benefit from

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<sup>102</sup> Whewell, W. and M. Ruse (2001). *Of the Plurality of Worlds*. Chicago, University of Chicago Press. p. 161.

<sup>103</sup> Whewell did not use the term 'analogue.' Instead, he uses the actual analogues, the fixed stars and the sun. The phenomenon under investigation is whether there exist life on other planets, and thus, for the analogical argument to be strong, the similarities must be relevant to supporting life (Whewell 1854, 185-186).

<sup>104</sup> In this section, I focus on Richards' view in Richards (1997), given that Richards notes that "some of his analysis...is found in more detail" (Richards 2005, fn 5, 233).

examining the previously unrecognized strategies at work in this particular community. Darwin's community, however, can only be said to be capable of a robust critique of analogy if Darwin is understood to have relied upon analogy in his work. Thus, I first turn my attention to Richards' objection.

Richards begins by claiming that the standard account is based on an inaccurate interpretation of Darwin's reasoning. In fact, the standard interpretation "seems to be committing [Darwin] to an argument that is obviously counterproductive" (Richards 1997, 75). Citing previous examples from Darwin's contemporaries, particularly Charles Lyell and co-discoverer of natural selection Alfred Russel Wallace, Richards notes that the domestication of animals initially supported the immutability of species, and the differences between artificial and natural selection would not support an analogy. Therefore, the fact that Darwin relies on an analogy revealing the fixity of species is "puzzling" to Richards because it "suggests that Darwin was making an argument that he knew would be taken to contradict his intended conclusion" (Richards 1997, 77).<sup>105</sup>

Given the putative paradox associated with Darwin's analogy, Richards considers three options for making sense of Darwin's method of arguing for natural selection. The first option is to acknowledge Darwin's use of an analogy in the *Origin*, in spite of it being paradoxical (Richards 1997, 77). On this account, Darwin does not seem rational because domestic cases—the breeding of plants and animals—were believed to support the immutability of species given the tendency to revert to ancestral forms. Richards' second option is to retain the analogy (and preserve the notion of causal efficacy) by appealing to the possibility of speciation if permitted ample time (Richards 1997, 77).<sup>106</sup> Although this option is attractive to those endorsing the standard view, Richards believes this option is

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<sup>105</sup> See also, Richards (2005, 209-212).

<sup>106</sup> Proponents, according to Richards, maintain causal efficacy by arguing that if given enough time, artificial selection would eventually produce new species, circumventing the assertions that artificial selection supports immutability (Richards 1997, 77).

untenable because it is based on a “crucial misunderstanding of Darwin’s position” (Richards 1998, 106). The misunderstanding results from the “ambiguous” use of *change* by those positing causal efficacy (Richards 2005, 215).

Having dismissed the previous two options, Richards endorses a third option, according to which Darwin did not rely on an analogy between artificial and natural selection, but argued from an enumerative, inductive pattern of argument (Richards 1997, 77). According to Richards,

A better understanding of Darwin’s argument is that he was not arguing by analogy with domestic breeding to establish the casual efficacy of natural selection; he was doing something very different...instead of relying on the similarities between artificial and natural selection (as would be expected in an analogical argument), Darwin’s argument relies on the **differences** between artificial and natural selection.<sup>107</sup>

(Richards 1998, 106)

On this view, those supporting the standard interpretation are mistaken about the nature of the analogy. According to Richards, Darwin focuses on the differences, or the negative analogy between artificial and natural selection, in order to support an inductive argument. Richards believes that the emphasis on the negative analogy prevents any appeal to an argument from analogy because the existence of differences between the source and the target defeats the analogy. For Richards, analogical reasoning relies upon making connections between the similarities of two objects or analogs. The more similarities there are between the two objects, the stronger the analogy is believed to be. If Darwin relied on the differences between artificial and natural selection, an argument from analogy would not be possible. The analogy does not play a positive role by drawing attention to what domestication and nature have in common; instead, Darwin uses a negative analogy as part of an inductive argument where the cumulative differences between the two objects

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<sup>107</sup> The emphasis is mine.

“contribute[s] to the force of an inductive inference, by establishing the variety of instances” (Richards 1997, 80).<sup>108</sup>

For Richards, Darwin’s reliance on a negative analogy is consistent with the beliefs of his contemporaries. Prior to the publication of the *Origin*, geologist Charles Lyell argued for the immutability of species in his book, *Principles of Geology*.<sup>109</sup> Lyell argues that because organisms reverted to their ancestral forms, species had “boundaries [that] cannot be breached” (Richards 1997, 76). Although breeding is capable of large change, Lyell’s argument was meant to show that artificial selection is not capable of forming new species. Given that Darwin “admired” Lyell’s reasoning, Richards believes that Lyell’s work would influence Darwin’s views (Richards 1997, 76).

As another element of support, Richards cites the work of Alfred Russel Wallace, who used a negative analogy in the Darwin-Wallace paper of 1858 (Richards 1997, 80-81).<sup>110</sup>

Wallace states,

...In the domesticated animal all variations have an equal chance of continuance; and those which would decidedly render a wild animal unable to compete with its fellows and continue its existence are at no disadvantage whatever in a state of domesticity. Our quickly fattening pigs, short-legged sheep, pouter pigeons, and poodle dogs could never have come into existence in a state of nature, because the very first step towards such inferior forms would have led to the rapid extinction of the race...

(Darwin and Wallace, 10-11)<sup>111</sup>

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<sup>108</sup> That Richards refers to Darwin’s argument as inductive (as opposed to analogical) may seem somewhat puzzling. Generally speaking, analogical arguments are considered to be inductive. The point to take away from Richards’ distinction is the shift from that of a positive analogy to a negative one, which is a component of a larger inductive argument.

Unlike an argument from analogy, an inductive inference relies on a variety of cases/instances for support. Richards believes Darwin used the differences between artificial and natural selection to support an inductive inference “to establish first, the laws of organic nature—laws governing sterility, inheritance, reversion, and the correlation of parts, and second, the existence of natural selection” (Richards 1997, 78).

<sup>109</sup> Lyell sent the book to Darwin while he was working on the *Beagle* as a ship’s naturalist in 1832 (Richards 1997, 76).

<sup>110</sup> The Darwin-Wallace paper was actually the compilation of two separate papers presented to the Linnaean Society in 1858. Neither Darwin nor Wallace was present at the Society. Darwin remained at home after losing his son to scarlet fever and Wallace was on expedition in Malaysia. For information on the biographies of both gentlemen, the reader may find the following websites useful, The Alfred Russel Wallace Website (<http://wallacefund.info/biography-wallace>) and Darwin Online (<http://darwin-online.org.uk/>).

<sup>111</sup> Cited in Richards (2005) page 213.

Wallace draws a negative analogy between artificial and natural selection as a result of the effects of each on an organism's fitness (Richards 1997, 81). According to Wallace, not only would the organisms changed by domestication not survive in nature, those animals would not have arisen in the first place. An analogy between artificial and natural selection fails because artificial selection produces unfit organisms. The type of change involved, for Richards, "is a difference in kind, not in degree" (Richards 2005, 215). Based on the decrease in an organism's fitness, Richards argues that even with enough time artificial selection could not possibly result in a new species.

Using the summary of Chapter IV, Richards reconstructs what he believes is Darwin's argument in the *Origin*:

#### Argument IV

1. Organic beings vary.
2. Each species increase in number geometrically, which results in a struggle for existence.
3. Some variations are more useful.
4. Those individuals with useful variations are more likely to survive.
5. From the 'strong principle of inheritance', we know that these individuals are likely to produce similar offspring.
6. Because 'more living beings can be supported in the same area the more they diverge in structure, habits, and constitution,' natural selection results in unlimited divergent modification.

(Richards 1997, 86)

The above argument, Richards contends, is how Darwin should, and indeed does, argue for his theory of natural selection (Richards 1997, 86). Using the above argument, Darwin

can “argue for the unlimited diverging modification of species” without encountering the limits to fitness implied by an argument from analogy (Richards 1997, 86). This is crucial because “artificial selection does not just favour fitness at a reduced pace or efficiency. It always *opposes* fitness” (Richards 1997, 95).

Artificial selection, as a “dead-end process,” cannot yield the unlimited divergent modifications needed for new species and therefore cannot be used in an analogical argument (Richards 1997, 95).<sup>112</sup> Instead, Darwin has a single aim: the discovery of the laws of nature via an inductive argument (Richards 1997,87).<sup>113</sup> Richards believes Darwin’s pursuit of organic laws is evidenced by his selection of Whewell’s quote at the beginning of the *Origin* and continues throughout the work, ending with the laws found in the passage above (Richards 1997, 88).<sup>114</sup> These laws can be fleshed out through an inductive argument because they “apply identically to all organic beings, whether in nature or in domestication” (Richards 1997, 88). The crucial point is not that the laws can be discovered within cases of artificial selection and then extended to natural selection because the processes are analogous. Instead, Richards argues Darwin makes an inductive argument by “emphasize[ing] both the number and variety of instances, as he does in the *Origin*, where he refers to wild and domesticated organisms, as well as humans” (Richards 1997, 89). Below is Richards’ reconstruction of the argument pattern he believes Darwin makes in the *Origin*.

#### Argument V

1. A has  $P_1$  and  $P_2$  and  $NA_1$
2. B has  $P_1$  and  $P_2$  and  $NA_2$

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<sup>112</sup> Richards notes that artificial selection is not referred to directly in this argument except as a support for the strong principle of inheritance, which asserts that phenotypic change is observed being passed from parent to offspring (Richards 1997, 86).

<sup>113</sup> Susan Sterrett (2002) argues for an analogy between two principles in the *Origin*: methodical selection and unconscious selection, and in doing so, argues against Richards’ view. See Sterrett (2002), pp. 15-17.

<sup>114</sup> The quote from Whewell is taken from his *Bridgewater Treatise*. “But with regard to the material world, we can at least go so far as this—we can perceive that events are brought about not by insulated interpositions of Divine power, exerted in each particular case, but by the establishment of general laws” (Darwin 1859, 2).

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3. Z has  $P_1$  and  $P_2$  and  $NA_n$

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C: Therefore, for all A, B...O..., if there is an advantage,  
then there is an increased chance for survival.

(Richards 1997, 93)<sup>115</sup>

In this argument, the uppercase letters (A, B...N...) signify the individuals or populations of species (Richards 1997, 93).  $P_1$  represents a particular advantageous trait and  $P_2$  is the likelihood for the organism to survive and reproduce (Richards 1997, 93).  $NA_n$  in the above argument represents the negative analogy or more simply, the differences between varieties of an organism or the traits present within a given species. The presence of  $NA_n$  is crucial to Richards' argument because it fulfills the antecedent condition by providing the examples from domestic cases that show a detrimental trait resulting in an organism's decreased survivability (Richards 1997, 93). In short, artificial selection can offer test cases for modifications made to an individual or population of individuals, allowing Darwin to "infer the existence, force and direction of natural selection" (Richards 1997, 92).

Richards takes advocates of the standard interpretation to be making several errors. First, it is unlikely that Darwin relied on an analogy between artificial and natural selection, given that the analogy had been used to demonstrate the immutability of species. Second, the standard interpretation utilizes a vague notion of causal efficacy. Instead of arguing in favor of large change, those seeking causal efficacy for natural selection must locate new species in domestic cases. Finally, in light of the differences between artificial and natural

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<sup>115</sup> I have changed the appearance of the argument to prevent confusion. In the last premise, I changed the letter for the individual to 'Z' to avoid confusing 'N' with the negative analogy 'NA'. I also added line numbers and a conclusion symbol, 'C'.

selection (such as fitness) it only makes sense that inferences about natural selection result from an inductive argument based on a variety of instances and not from analogy. Given that this type of inductive argument is compatible with the passages from Darwin, Richards' position seems firmly defended.

### § 2.3 Causal Efficacy and the Analogy in the *Origin*<sup>116</sup>

Richards objects to the standard interpretation by first pointing to a putative paradox that results if Darwin had relied on an analogy as part of his justification for the causal efficacy of natural selection. According to Richards, using artificial selection as a source analogue runs counter to Darwin's goal because the analogy "was widely used by those with opposing views" (Richards 1997, 76). These views were primarily held by creationists, who believed that species were perfectly created by God and tended not to stray from the ancestral form. Renowned geologist, Charles Lyell, discussed the reversion of plants and animals to their ancestral forms in his book, *The Principles of Geology*. Since Lyell was a prominent figure and highly respected by Darwin, Richards claims that Darwin would hesitate to use artificial selection.<sup>117</sup>

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<sup>116</sup> Historically, discussions about Darwin's analogy between artificial and natural selection center on the role of the analogy as it is found in the *Origin*. Richards, however, seeks to extend his analysis of efficacy to include Darwin's *Variation of Plants and Animals under Domestication* (Richards 1998, 107). Using Darwin's introductory remarks in the *Origin*, Richards argues that the work is merely an abstract lacking the details needed to determine the efficacy of Darwin's analogy (Richards 1998, 107). However, I disagree with Richards' assessment.

First, Darwin's remarks in the introduction to the *Origin* are not sufficient to establish the need to refer to the *Variation*. Looking at the quote selected by Richards, this becomes apparent. Richards refers to the introduction of the *Origin* where Darwin confides

I cannot here give references and authorities for my several statements; and I must trust to the reader reposing some confidence in my accuracy... No one can feel more sensible than I do of the necessity of hereafter publishing in detail all the facts, with references on which my conclusions have been grounded; and I hope in a future work to do this...

(Darwin 1859, 2)

These remarks, however, are not an admission that the argument in the *Origin* is incomplete by any means. In fact, there is a missing section in the quote used by Richards. Following the appeal to the reader for confidence in his work, Darwin states, "No doubt errors would have crept in, though I hope I have always been cautious in trusting to good authorities alone. I can here only give the general conclusions at which I have arrived, with a few facts in illustration, but which I hope, in most cases will suffice" (Darwin 1859, 2). In the *Origin*, Darwin makes use of a large amount of empirical work not necessarily collected by him. Using the social connections available at the time, such as personal correspondence and networking within various scientific societies, Darwin's comments are intended to acknowledge those contributing to his work and to mitigate the errors of his note keeping.

Even though the *Origin* as an abstract does not include every piece of support for natural selection, it seems, as Darwin notes, to provide an argumentative framework with enough examples to draw the initial conclusions. In the final chapter of the *Origin*, Darwin admits that "this whole volume is one long argument" which suggests that Darwin himself recognized it has an argumentative structure—albeit a scantily clad one (Darwin 1859, 459). Given that Darwin recognizes the *Origin* as a complete argument, a view that is shared by his critics as well as those holding the standard interpretation, which Richards argues against, I too will limit my evaluation to the objections made against the standard interpretation of the analogy as it appears in the *Origin*.

<sup>117</sup> Darwin had a high regard for Lyell and was known to have read the first and second volumes of Lyell's *Principles* while on the *H. M. S. Beagle* (Ruse 1979, 49). In addition, Darwin states in his autobiography, "I saw more of Lyell than

Whether Lyell's use of reversion was part of a creationist account of the immutability of species has no bearing on how Darwin would make use of the same data. Even though Darwin's views differed from those of Lyell, he was able to convince Lyell of his theory of natural selection. In a letter to Lyell dated 11 October 1859, Darwin, recognizing the value of convincing Lyell of his theory, writes

I am very glad you think it worth-while to run through my Book again, as much or more for the subject-sake than for my own sake [...] If you think enough, I expect that you will be *perverted*; & if you ever are, I shall know that the theory of Nat. Selection is in the main safe

(Burkhardt and Smith, 347).

In a later letter (October 28, 1859), Lyell admits he is convinced, although he acknowledges that opponents may seize upon the differences between artificial and natural selection in order to object to Darwin's theory. Lyell writes

I understand you to say that selection under domestication rarely does in a shorter time what Nature might do in a longer...I cannot help thinking that by taking this concession, one which regards a variable species, about which we know most (little tho' it be) an adversary may erect a battery against several of your principal rules, & in proportion as I am *perverted* I shall always feel inclined to withstand so serious a wavering.

(Burkhardt and Smith, 363)

The running joke between Darwin and Lyell is a good-natured play on the word 'converted.' From Darwin's letter, we can see that he not only values Lyell's opinion, but convincing Lyell would lend support to his theory.<sup>118</sup>

Richards does not rely only on an appeal to Lyell's influence to make his point, he also draws from Wallace's contribution to the 1858 paper presented at the Linnaean Society, as well as a letter from Darwin, to argue that Darwin and Wallace shared similar opinions

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any other man both before and after my marriage. His mind was characterised, as it appeared to me, by clearness, caution, sound judgment and a good deal of originality" (Darwin 1887, 100).

<sup>118</sup> This is supported by Lyell's "increasing tolerance toward an evolutionary perspective" (Recker 1990, 472).

regarding domestic cases and evolution. In the 1858 paper, Wallace notes the tendency for domestic plants and animals to revert to ancestral forms, stating

One of the strongest arguments...adduced to prove the original and permanent distinctness of species is, that varieties produced in a state of domesticity are more or less unstable, and often have a tendency, if left to themselves, to return to the normal form of the parent...

(Darwin and Wallace, 1)

As regards whether an analogy might be drawn between artificial and natural selection, Wallace claims

It will be observed that this argument rests entirely on the assumption, that varieties occurring in a state of nature are in all respects analogous to or even identical with those of domestic animals...But it is the object of the present paper to show that this assumption is altogether false.

(Darwin and Wallace, 1).

In addition to Wallace's remarks against using an analogy between artificial and natural selection, Richards cites a letter from Darwin to argue that both men are working from similar assumptions. In a letter (May 1, 1857), Darwin informs Wallace, "I have acted already in accordance with your advice of keeping domestic varieties and those appearing in nature distinct; but I have sometimes doubted of the wisdom of this, and therefore I am glad to be backed by your opinion (Darwin 1958, 193).

For Richards, the above quote suggests that Darwin and Wallace share similar beliefs regarding what inferences can be drawn from an analogy between artificial and natural selection (Richards 1998, 106-107). Although this evidence seems to support Richards' view, there is reason to doubt his claim. First, Richards relies on a letter written by Darwin in 1857, at the beginning of his relationship with Wallace and two years before the first

edition of the *Origin* was published. Even if Darwin and Wallace may have shared similar beliefs initially, Darwin had ample time to change his mind before writing his book.

Second, there are discrepancies between Wallace's 1858 paper and the text of the *Origin*. According to Wallace, the differences between domesticated animals and their wild counterparts are striking. For example,

The domestic animal, on the other hand, has food provided for it, is sheltered, and often confined, to guard against the vicissitudes of the seasons, is carefully secured from the attacks of its natural enemies, and seldom even rears its young without human assistance. Half of its senses and faculties are quite useless; and the other half are but occasionally called in to feeble exercise, while even its muscular system is only irregularly called into action.

(Darwin and Wallace, 4)

As this passage indicates, Wallace considers domesticated animals inferior when compared with those in the wild. Given their inferiority, it comes as no surprise that domestic stock either reverts to the ancestral forms or are so unfit, they do not survive. Yet, I contend that Darwin did not find these differences to be as striking as Wallace did. Darwin notes

I may refer to a statement often made by naturalists—namely, that our domestic varieties, when run wild, gradually but certainly revert in character to the aboriginal stocks. Hence it has been argued that no deductions can be drawn from domestic races to species in a state of nature. I have in vain to discover on what decisive facts the above statement has so often and so boldly been made. There would be great difficulty in proving its truth...

(Darwin 1859, 14)

Even though most domestic species could not survive in the wild, the above passage from the *Origin* demonstrates that Darwin disagrees with Wallace's claim (Darwin 1859, 14). Domestication had occurred across such a vast expanse of time that it is hard to ascertain whether reversion was complete or whether it had even taken place (Darwin 1859, 14). Were we able to judge that a perfect reversion had occurred then Darwin is willing to admit

domestic cases offer little support for his view (Darwin 1859, 14). However, we know so little of the ancestral forms that we cannot be sure of our conclusions involving reversions of species (Darwin 1859, 14). Darwin claims, “there is not a shadow of evidence in favor of this view: to assert that we could not breed our cart and race-horses, long and short-horned cattle, and poultry of various breeds, and esculent vegetables, for almost infinite number of generations, would be opposed to all experience” (Darwin 1859, 15).

Darwin further responds to Wallace’s claim regarding domestic animals by also noting “in regard to the domestic animals kept by uncivilized man, it should not be overlooked that they almost always have to struggle for their own food, at least during certain seasons” (Darwin 1859, 38). Given the lack of shelter and the difference in care, some domestic animals are closer to being in a state of nature than those found in Europe. For Darwin, “This, perhaps, partly explains what has been remarked by some authors, namely, that the varieties kept by savages have more of the character of species than the varieties kept in civilized countries” (Darwin 1859, 38). Unlike Wallace, Darwin does see similarities between domestic and wild organisms.

Another point, perhaps more detrimental to Richards’ claim, is Darwin’s remarks regarding incipient species in the *Origin*. Darwin states, “According to my view, varieties are species in the process of formation, or are, as I have called them, incipient species” (Darwin 1859, 111). According to Darwin, “no clear line of demarcation has as yet been drawn between species and sub-species” (Darwin 1859, 51). Thus, if we consider a particular trait and its modification over time, we can see a series of sub-species, the gradation closest to the new species being the incipient species (Darwin 1859, 51).<sup>119</sup> Darwin believes that there are many examples of incipient species in domestic breeding. Of these, Darwin observes “there are hardly any domestic races, either amongst animals or plants, which have not been

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<sup>119</sup> Incipient species, for Darwin, often become distinct species, but it is not necessary. Darwin notes that they can also “become extinct, or they may endure as varieties for very long periods” (Darwin 1859, 52).

ranked by some competent judges as mere varieties, and by other competent judges as the descendants of aboriginally distinct species” (Darwin 1859, 16). That a variety could be classified as a distinct species by an experienced judge attests to the possibility that artificial selection could yield new species.

When attempting to answer the question of how varieties become species, Darwin suggests, “As has always been my practice, let us seek light on this head from our domestic productions. We shall here find something analogous” (Darwin 1859, 111-112). Darwin describes a possible scenario involving the traits of pigeons:

A fancier is struck by a pigeon having a slightly shorter beak;  
another fancier is struck by a pigeon having a rather long beak;  
and on the acknowledged principle that ‘fanciers do not and will not admire a medium standard, but like extremes’ they both go on (as has actually occurred with tumbler-pigeons)...the differences would be very slight; in the course of time... the differences would become greater, and would be noted as forming two sub-breeds; finally after the lapse of centuries, the sub-breeds would become converted into two well-established and distinct breeds.

(Darwin 1859, 112)

Given Darwin’s appeal to incipient species, Richards’ objection to the standard interpretation seems unreasonable. In fact, incipient species shows that it is possible for artificial selection to yield new species; the only reason we have yet to see them is time.

Darwin draws this implication further, asking

But how it may be asked, can any analogous principle [selection] apply in nature? I believe it can and does apply most efficiently, from the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and wildly diversified places in the polity of nature, and so be enabled to increase in numbers.

(Darwin 1859, 112)

Although Darwin and Wallace posit the same process, it is shortsighted to assume that arriving at the same conclusion entails using similar arguments. Over the period of their relationship, Darwin and Wallace disagreed over other aspects of the theory of evolution they were developing, notably the process of sexual selection and how higher intellectual faculties of humans developed given that music and math offered no benefit to survival and reproduction. As a result of those disagreements and the above passages taken from the *Origin*, I suggest that Richards' claim concerning the influence of Wallace and Lyell is not as strong as he takes it to be.

I also argue that Richards' critique of the standard interpretation is not supported by the views expressed in the *Origin*. As we saw in section 3, Richards suggests three responses to Darwin's problematic analogy: 1) ignore the paradox to the detriment of Darwin's reasoning, 2) maintain the existence of the analogy citing species will emerge if given enough time, and 3) to reject the use of analogy.<sup>120</sup> Richards dismisses the second response on two grounds: first, it plays on the ambiguity surrounding the concept of causal efficacy and second, it is based on a misinterpretation of the *Origin*. However, I contend that Richards' objection is predicated on a naïve understanding of analogy. Citing from the works of 19<sup>th</sup> century logicians, John Hibben and John Stuart Mill, Richards depicts arguments from analogy as inferences based solely on the accumulation of similarity (Richards 1997, 79). What he fails to recognize is the need for an analogical argument to be based not on an indiscriminate assortment of similarities, but on a collection of relevant similarities *in the absence of relevant differences*.<sup>121</sup> I suggest that Darwin recognized the differences between

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<sup>120</sup> The first option, of course, is not tenable because it subjects Darwin's justification for the entire theory to criticism.

<sup>121</sup> A forthcoming manuscript by R. White, J. Hodge, and G. Radick addresses this issue. I thank all three for their assistance and correspondence. In fact, White states,

It is also not correct to present this pattern of argument as *the* understanding of what was meant by argument by analogy among 19<sup>th</sup> Century logicians. Indeed, Mill himself does not simply present the style of argument by analogy quoted by Richards, but begins his chapter 'Of Analogy' by giving first an account of

artificial and natural selection, but he did not consider them problematic, or relevant. In fact, I believe that we can resurrect Richards' second option if we employ a definition of causal efficacy that is more consistent with Darwin's text. A more textually accurate definition of causal efficacy demonstrates that Darwin stressed the positive analogy as opposed to the negative analogy, making the second option viable and not the paradox Richards claims it to be.

Richards objects to the standard interpretation because it exploits an ambiguity of the term "causal efficacy." For Richards, an argument based on an analogy between artificial and natural selection must go beyond positing change. Recall that, for Richards, large phenotypical change is not sufficient for causal efficacy; natural selection must yield new species. Yet, artificial selection is detrimental to the fitness of domestic organisms.

According to Richards, the standard interpretation overlooks a key difference between artificial and natural selection: the fitness of the organism. However, if it can be demonstrated that Darwin does recognize this key difference and does not believe it relevant—that is, if it can be accommodated by the two selective processes—then Richards' suggestion that Darwin uses an inductive argument is incorrect. This is what I intend to demonstrate; not only does Darwin recognize that artificial selection is often detrimental to an organism's fitness, he also includes it within his theory of natural selection. By including the concepts of rouging/culling and extinction within the processes of selection, I suggest that Darwin intends to draw an analogy between the two. Further, this analogy is so strong that Darwin is convinced that artificial selection would produce new species if given enough time.

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argument by analogy that is completely different from the one we are now discussing.

(Unpublished draft text on Darwin's selection analogies by R.White, J.Hodge and G.Radick. Cited with authors' permission)

Richards claims that supporters of the standard interpretation of the *Origin* would reconstruct Darwin's analogical argument as follows:

Argument VI<sup>122</sup>

AS and NS have P<sub>1</sub>.

AS and NS have P<sub>2</sub>.

.

.

AS is causally efficacious in the formation of new species.

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Therefore, NS is also causally efficacious in the formation of new species.

(Richards 1997, 85)

According to Richards, mere change is not enough to attain causal efficacy; artificial selection must yield new species in order for it to be a suitable source for natural selection. Of course, those supporting the standard interpretation, however, would not accept Argument VI. The last premise is false, making Argument VI as a whole uncogent.<sup>123</sup> Richards' definition of causal efficacy in Argument VI is problematic: it fails to capture Darwin's intent for causal efficacy. Natural selection for Darwin is not limited solely to the creation of new species. It also includes the evolutionary losers, those organisms that lacked the specific trait, which enabled their competitors to survive and reproduce. Darwin writes "natural selection is daily and hourly scrutinizing, throughout the world, every variation, even the slightest; rejecting that which is bad, preserving and adding up all that is good; silently and insensibly working, whenever and wherever the opportunity offers" (Darwin

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<sup>122</sup> In Richards' text, this is Argument II.

<sup>123</sup> I am using 'uncogent' to indicate that the premises of a non-deductive argument do not entail the likelihood of the conclusion.

1859, 84). Darwin describes natural selection not merely as the creation of new species, but it also includes the cessation of individuals with less favorable traits. Without the unsuccessful survival and reproduction of less fit organisms, there is no preservation of the favored individuals in a given population. For Darwin, the practice of culling less favored individuals from a flock of sheep or roguing less desirable plants from a particular crop is analogous to the process of extinction in the state of nature. Darwin notes “The key is man’s power of accumulative selection: nature gives successive variations; man adds them up in certain directions useful to him. In this sense he may be said to make for himself useful breeds” (Darwin 1859, 30). Part of breeding for a particular trait includes the conscious mating of those individuals having that trait, but that is only part of the process. According to Darwin, “When a race of plants is once pretty well established, the seed-raisers do not pick out the best plants, but merely go over their seed-beds, and pull up the ‘rogues,’ as they call the plants that deviate from the proper standard. With animals, this kind of selection is, in fact, also followed; for hardly any one is so careless as to allow his worst animals to breed” (Darwin 1859, 32-33). The culling or roguing of less desirable individuals is a necessary part of artificial selection and as we shall see, the removal of less fit individuals is just as necessary for natural selection.

According to Darwin, “we see beautiful adaptations everywhere and in every part of the organic world,” which, he argues, came into being in the following way:

All these results [the adaptations]...follow inevitably from the struggle for life. Owing to this struggle for life, any variation, however slight, and from whatever cause proceeding, if it be in any degree profitable to an individual of any species, in its infinitely complex relations to other organic beings and to external nature, will tend to the preservation of that individual, and will generally be inherited by the offspring.

(Darwin 1859, 61)

That the struggle for life is part of artificial and natural selection is undeniable. In fact, Darwin links these two processes together, asserting “I have called this principle, by which each slight variation, if useful, is preserved, by the term of Natural Selection, in order to mark its relation to man’s power of selection” (Darwin 1859, 61). The struggle for existence also plays a critical role in natural selection. Were animals allowed to reproduce unchecked, there would be a multitude of organisms populating the planet. Yet, even in times of plenty, there is a competition for available resources (Darwin 1859, 63-76).<sup>124</sup> Competition is a major contributor to the process of natural selection and neglecting its import is irresponsible. Darwin states “This preservation of favourable variations and the rejection of injurious variations, I call Natural Selection. Variations neither useful nor injurious would not be affected by natural selection, and would be left a fluctuating element” (Darwin 1859, 81). Because Richards omits consideration of the struggle for existence in the definition of causal efficacy in Argument VI, his objection against the standard view suffers.<sup>125</sup>

A revised definition of causal efficacy, one that can accommodate the different amounts of time required by artificial and natural selection to produce new species, removes Richards’ worry about time; time is no longer a relevant difference, making Richards’ claim that Darwin relied on a negative analogy unsustainable. Darwin states “That natural selection will always act with extreme slowness, I fully admit...I further believe, that this very slow, intermittent action of natural selection accords perfectly well with what geology tells us of the rate and manner at which the inhabitants of this world have changed” (Darwin 1859, 108-109). The possibility of an older earth opens the door for the needed expanse of time for species to develop. Thus, the claim that ‘we have yet to witness a new species

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<sup>124</sup> The competition can occur between species or within species. There are also other factors, such as the ability to exploit a previously unoccupied habitat, predator and prey relations, and the interactions between organisms, such as symbiosis.

<sup>125</sup> I have argued elsewhere for a different, I believe more textually accurate, way of defining causal efficacy. See Sullivan-Clarke (2013).

emerging from artificial selection is that there has not been enough time' seems quite rational.

Further, if incipient species are the varieties found within nature and domestic stock, it makes sense to believe that the varieties found in domestic cases could, in the course of time, yield a new species (Darwin 1859, 55-56). This claim is supported by the example of domesticated stock in less civilized countries. Darwin states "If it has taken centuries or thousands of years to improve or modify most of our plants up to their present standard of usefulness to man, we can understand how it is that neither Australia, the Cape of Good Hope, nor any other region inhabited by quite uncivilized man, has afforded us a single plant worth culture" (Darwin 1859, 37-38). These areas have diverse populations of organisms, but they have not had the time to improve their stock to the extent that European breeders have (Darwin 1859, 38). Given enough time, Darwin has no doubt that the purportedly less civilized regions would yield "something distinct and valuable, and will then probably first receive a provincial name" (Darwin 1859, 40). If we imagine the movement from variety to species as a continuum, then those receiving a provincial name are a hair's breadth away from being a new species. Thus, domestic populations could yield a new species.

The definition of causal efficacy that Richards attributes to the standard interpretation is problematic. In light of the more plausible conception of causal efficacy that advocates of the standard interpretation attribute to Darwin—an interpretation that is consistent with Darwin's text—the claim that Darwin argued from an analogy between artificial and natural selection no longer seems unreasonable. The fact that artificial selection has yet to produce a new species does not undermine the standard interpretation of Darwin's theory. Rather, it supports his belief that the varieties found in domestic cases could be incipient species. Given enough time, Darwin fully expects to see new species emerging from domestic cases. This of course vindicates the appeals to time made by the standard interpretation, and it

vindicates Darwin's remarks about why domestic cases would be of primary importance. Domestic cases are important given their potential to offer a new species.

#### § 2.4 Metaphor, Analogy, and their roles in the *Origin*: A Response to Gildenhuys

There is no word, which is used more loosely, or in a greater variety of senses, than Analogy.

John Stuart Mill<sup>126</sup>

Richards is not the only philosopher to challenge the standard interpretation. Peter Gildenhuys also questions the standard view that Darwin used an analogical argument to justify natural selection as a *vera causa*. In contrast to Richards' account, however, Gildenhuys acknowledges that Darwin used of analogical reasoning in the *Origin*, but not in the way advocates of the standard view claim. Gildenhuys describes Richards' wholesale rejection of an analogy between artificial and natural selection in the *Origin* as "too narrow" (Gildenhuys 2004, 594). Where Richards denies that Darwin relied on analogical reasoning to support his theory, Gildenhuys believes that Darwin uses analogy in accordance with John Herschel's account of induction. According to Gildenhuys, a Herschellian analogy is used to "generalize" a law from a group of similar phenomena (Gildenhuys 2004, 594). In other words, scientists refer only to the positive analogy to "arrive at overarching laws of nature" (Gildenhuys 2004, 595).<sup>127</sup> Gildenhuys stresses that the process of generalizing over phenomena is not motivated by an analogical inference—even though Darwin refers to his theorizing as using analogy.<sup>128</sup> Rather, Gildenhuys maintains that Darwin's analogy is simply

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<sup>126</sup> Mill, J. S. (1900). *A System of Logic, Ratiocinative and Inductive*. New York, Harper and Brothers Publishing. p. 393.

<sup>127</sup> As we shall see, there is no appeal to relevance, the negative analogy, or the neutral analogy in this particular account.

<sup>128</sup> Gildenhuys recognizes this at the outset, citing a letter from Darwin to George Bentham dated May 22, 1863, and another to John Stevens Henslow, dated May 8, 1860. For the letter to Bentham, see Burkhardt and Smith (1985) vol. 11, p.432-433 and for the letter to Henslow, see Darwin (1967) p. 203-204. A portion of the letter to Henslow is also cited in Lloyd (1983), p. 113.

a catalogue of similarities that Darwin used to classify selective phenomena (Gildenhuys 2004, 596).<sup>129</sup>

Given that Gildenhuys acknowledges a more substantial role for analogy in the *Origin* than Richards, I believe that his account has something to offer pedagogically, especially with respect to analogical reasoning. As we saw in the previous sections, Richards overstates the negative analogy between artificial and natural selection and trades on a naïve conception of analogical arguments. However, as the quote from Mill attests, analogy is used in many different ways, and in this section, I argue that Gildenhuys' attempt to assert Herschel's influence in the *Origin* exemplifies a common problem for analogical reasoning: the failure to distinguish between analogy and metaphor.<sup>130</sup> After an analysis of Gildenhuys' account, we shall see that his account and the standard interpretation are not mutually exclusive.

Gildenhuys' objection relies on Herschel's methodology, which can be found in his original work, *A preliminary discourse on the study of natural philosophy*.<sup>131</sup> According to Herschel, "the first thing that a philosophic mind considers, when any new phenomenon presents itself, is its *explanation*, or reference to an immediate producing cause" (Herschel 1987, 144 §137).<sup>132</sup> If an explanation is not available, Herschel suggests that one can "*generalize* the phenomenon, and include it, with others analogous to it, in the expression of some law" (Herschel 1987, 144 §137). By generalizing over the phenomena, Herschel believes that "a more advanced state of knowledge, may lead to the discovery of an adequate proximate cause" (Herschel 1987, 144 §137).

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<sup>129</sup> According to Gildenhuys, "Analogies between phenomena are what allow us to group them together as sharing the same explanation (Herschel, 1987, pp. 95, 102, 149)" (Gildenhuys 2004, 596). This differs from an argument, where an additional feature is attributed to the target based on the similarities it shares with the source. A Herschellian analogy is merely to recognize a common cause based on similarities (Gildenhuys 2004, 596).

<sup>130</sup> Leatherdale (1974) notes "analogy, model, and metaphor are intimately related concepts in relation to science" (vii). Bailer-Jones (2002) points out that metaphor and analogy are "often not distinguished very sharply" (Bailer-Jones 2002, 108). For example, Stepan (1986) uses metaphor and analogy "interchangeably" in her paper on the effects of metaphor/analogy on 19<sup>th</sup> century race science (Stepan 1986, 261 *fn* 1).

<sup>131</sup> Hereafter, Herschel's book is referred to as *Discourse*.

<sup>132</sup> I have included the section numbers for Herschel's *Discourse* so that other editions can be referenced.

Experience is a crucial part of Herschel's account because it reveals the causes of phenomena, particularly the *vera causae* or "causes recognized as having a real existence in nature, and not being mere hypotheses or figments of the mind" (Herschel 1987, 144 §138).<sup>133</sup> Experience of the *vera causae* is a necessary condition for Herschel; its results must be observed. As an example, Herschel discusses finding a potential explanation for seashells that appear in rocks "at a great height above the sea" (Herschel 1987, 144-145 §138). Herschel considers several possible causes for the presence of the shells in an unexpected location, ruling out those processes that have not been directly observed, such as of fermentation (Herschel 1987, 145 §138).<sup>134</sup> Although fermentation is a real cause in the sense that it is a process that exists in the world, no one has actually witnessed seashells appearing via fermentation, making a different cause more likely (Herschel 1987, 145 §138).

According to Herschel, "the great, and indeed only ultimate source of our knowledge of nature and its laws, [is] EXPERIENCE; by which we mean, not the experience of one man only, or of one generation, but the accumulated experience of all mankind in all ages, registered in books or recorded by tradition" (Herschel 1987, 76 §67). Experience, for Herschel, comes in two forms: experience from observation or from experiment (Herschel 1987, 76 §67). Observation, for Herschel, is "noticing facts as they occur, without any attempt to influence the frequency of their occurrence, or to vary the circumstances under which they occur" (Herschel 1987, 76 §67). Whereas, experiment is "putting in action causes and agents over which we have control, and purposely varying their combination, and noticing what effects takes place" (Herschel 1987, 76 §67).<sup>135</sup> Of the two, Herschel prefers

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<sup>133</sup> Herschel acknowledges Newton's term, *vera causae* (Herschel 1987, 144 §138).

<sup>134</sup> Other causes noted by Herschel include influence by celestial bodies, transportation by pilgrims known to wear scallops, carried by birds, geological explanation (that the levels of land and sea altered) (Herschel 1987, 145 §138). Celestial influence was ruled out as belonging to fancy (Herschel 1987, 145 §138).

<sup>135</sup> Herschel's empiricism is apparent in his descriptions of observation and experiment—as is also the "passive" nature of observation when compared to the "active" one of experiment" (Herschel 1987, 77 §67). Herschel admits "the terms *passive* and *active observation* might better express their distinction" (Herschel 1987, 77 §67). He further

the more active experiment, noting that the sciences advanced much quicker as a result of experiment (Herschel 1987, 78 §67). For example, “the moment that [astronomy] became a branch of mechanics, a science essentially experimental [...] its progress suddenly acquired a tenfold acceleration” (Herschel 1987, 78 §67).

Through experiment, scientists are able to explain the *phenomena* or “the sensible results of processes” (Herschel 1987, 85 §76). Not all processes are directly observed, but they can be “rendered *sensible*; that is to say, analysed” (Herschel 1987, 85 §76). In other words, phenomena are complex and through experiment, scientists can break down the complex phenomena into simpler phenomena (Herschel 1987, 88 §79). As an example, Herschel points to the phenomenon of sound (Herschel 1987, 88 §79). Herschel first cites the similarities between “the various cases in which sounds of all kinds are produced” (Herschel 1987, 88 §79).<sup>136</sup> Once the similarities are listed, the causes that are required to have “a true and complete knowledge” of the phenomena are determined (Herschel 1987, 88-89 §79).<sup>137</sup> In fact, causes lead to the formulation of natural laws, as Herschel points out,

Whenever, therefore, we perceive that two or more phenomena agree in so many or remarkable points, as to lead us to regard them as forming a class or group, if we lay out of consideration, or *abstract*, all the circumstances in which they disagree, and retain in our minds those only in which they agree, and then under this kind of mental convention, frame a definition or statement of one of them, in such words that it shall apply equally to them all, such statement will appear in the form of a general proposition, having so far at least the character of a law of nature.

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describes observation as “listen[ing] to a tale;” while experiment is like “cross-examin[ing] a witness” (Herschel 1987, 77 §67).

<sup>136</sup> Herschel lists the similarities as,

1<sup>st</sup>, The excitement of a motion in the sounding body. 2<sup>dly</sup>, The communication of this motion to the air or other intermedium which is interposed between sounding body and our ears. 3<sup>dly</sup>, The propagation of such motion from particle to particle of such intermedium in due succession. 4<sup>thly</sup>, Its communication, from the particles of the intermedium adjacent to the ear, to the ear itself. 5<sup>thly</sup>, Its conveyance in the ear, by a certain mechanism, to the auditory nerves. 6<sup>thly</sup>, the excitement of sensation.

(Herschel 1987, 88 §79)

<sup>137</sup> Causes that can no longer be analyzed are considered “simple, elementary, and referable” (Herschel 1987, 89 §79).

(Herschel 1987, 99 §89)

Analogy in the above description is a way of clustering together distinct phenomena in order to formulate a general statement, which in most cases is like a law. Using an example from optics, Herschel points out that “a great number of transparent substances” when subjected to a beam of light give off “periodical colours” (Herschel 1987, 99 §89). Of course, not all transparent solids produce colors, but of those that do, one similarity (double refraction) becomes apparent (Herschel 1987, 100 §90). This, according to Herschel, prompts scientists to posit a hypothesis and test whether “Doubly refracting substances exhibit periodical colors by exposure to polarized light” (Herschel 1987, 100 §90). Since all known cases supported this claim, Herschel believed it could be “regarded as a law of nature” (Herschel 1987, 100 §90).<sup>138</sup> This manner of analysis continues until it is no longer possible, and what is left, according to Herschel, are the “*axioms* of the highest degree of generality of which science is capable,” also known as *vera causae* (Herschel 1987, 102 §94). The likelihood of finding a *vera causa* is improved given an increase in the “number and variety of causes,” the way in which the causes are used to explain the phenomena, the number of “analogous phenomena,” and how close/strong the analogy is between the different phenomena (Herschel 1987, 148 §141).<sup>139</sup>

For Gildenhuys, the analogical reasoning present in Herschel’s account of induction is not justificatory, but is merely a comparison of similar features. Thus, he believes that his

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<sup>138</sup> Herschel recognizes that the groupings of entities based on similarities, which are likely to yield a cause or law, require knowledge of particular laws/causes (Herschel 1987, 142-143 §136). Scientists should exercise care, especially with classification into groups and naming (Herschel 1987, 138-139 §132). Classifications, for Herschel, are “a general fact; and when we have amassed a great store of such *general facts*, they become the objects of another and higher species of classification” (Herschel 1987, 102 §94).

<sup>139</sup> The above description is sometimes referred to as Herschel’s first formulation of *vera causae*. Another formulation, which approaches Whewell’s consilience of inductions, appears in §176 and §181. According to this formulation, a law posited from a *vera causa* will not be a general law unless it can apply to other cases beyond those which were used previously (Herschel 1987, 167 §176). “The surest and best characteristic of a well-founded and extensive induction...is when verifications spring up...from quarters where they might be least expected, or even among instances that very kind which were at first considered hostile to them (Herschel 1987, 170 §180). The second formulation does not directly appeal to an analogy and contributes little to my analysis of Gildenhuys. It also is subject to debate. Ruse states that Herschel “did not have consilience in mind for his *vera causa* concept (Ruse 1978, p. 324).

objection, which relies on Herschel's account, differs from the standard interpretation. But does it really differ? According to the standard interpretation posited by Recker and Ruse, Darwin argues for the causal efficacy of natural selection given its analogy with artificial selection, but more importantly, he does so to demonstrate that natural selection is a *vera causa* in the Herschellian sense (Gildenhuys 2004, 595).<sup>140</sup> In light of the confusion resulting from the many roles of analogical reasoning, Gildenhuys' account and the standard interpretation of Darwin's use of analogy may not be mutually exclusive. Instead, the disagreement between Gildenhuys and the standard interpretation hinges on how to interpret a Herschellian analogy.

When explaining Herschel's account of induction, Gildenhuys claims, "analogies between phenomena are what allow us to group them together as sharing the same explanation" (Gildenhuys 2004, 596). These analogies need not be striking; in fact, the analogues "need only be similar insofar as the same cause operates to produce both" (Gildenhuys 2004, 595). The strength of the analogy, for Gildenhuys, depends solely upon the number of similarities of the phenomena (Gildenhuys 2004, 604). Using §89 (cited earlier on page 31) as support, Gildenhuys claims that Herschel emphasizes the similarities between phenomena, and downplays the differences when attempting to formulate a law or general statement. For example, Gildenhuys notes that Herschel posits the generalization—that "Doubly refracting substances exhibit periodical colors by exposure to polarized light"—in spite of differences in "color, texture, weight, hardness, form, and composition" between the substances (Herschel 1987, 100 §90). Gildenhuys also claims that Herschel believes the differences between phenomena are useful, particularly when they make the similarities

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<sup>140</sup> Their reliance on Herschel is apparent given that Herschel states, "Whenever [...] any phenomenon presents itself for explanation, we naturally seek, in the first instance, to refer to it some one or other of those real causes which experience has shown to exist, and to be efficacious in producing similar phenomena" (Herschel 1987, 148 §141).

Gildenhuys cites Ruse (1999, p. 177) and Ruse (1975, p. 175). However, he neglects to include Ruse (1978), which happens to be a clarification of his interpretation of Herschel after critique from Thagard (1978).

between phenomena “more prominent and striking” (Herschel 1987, 118-119, §109).<sup>141</sup> Ultimately, because Darwin acknowledges the differences between artificial and natural selection, Gildenhuys concludes that Darwin could not be using an argument from analogy; he must be using a Herschellian analogy.

Gildenhuys, however, relies on Richards’ account of how analogy was understood during the 19<sup>th</sup> century and, like Richards, he is committed to a simple conception of analogy, notably one lacking the concept of relevance.<sup>142</sup> Thus, any difference, according to Gildenhuys, would defeat an argument from analogy (Gildenhuys 2004, 604). In the case of Darwin’s analogy, the presence of dissimilarities between artificial and natural selection contradict the standard interpretation by defeating the analogical argument linking the two processes (Gildenhuys 2004, 604).<sup>143</sup> Noting differences, however, do not compromise grouping phenomena based on the similarities (Gildenhuys 2004, 596). If Darwin employed a Herschellian methodology, then the differences would not matter (Gildenhuys 2004, 604). Thus, “...even though Darwin’s argument is not an argument from analogy, it can nonetheless be said to *depend* on an analogy since the same principle of selection must operate in both [nature and domestic cases]...to qualify as a *vera causa*” (Gildenhuys 2004, 604).<sup>144</sup> According to Gildenhuys, “Darwin can deduce facts about species from facts about domestic races because he generalizes from the breeding to produce a general law of nature from

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<sup>141</sup> In §109, Herschel states

Whenever, therefore, we would either analyze a phenomenon into simpler ones, or ascertain what is the course or law of nature under any proposed general contingency, the first step is to accumulate a sufficient quantity of well ascertained facts, or recorded instances bearing on the point in question...the more different these collected facts are in all other circumstances but that which forms the subject of enquiry, the better; because they...render those in which they agree more prominent and striking.

(Herschel 1987, 118-119, §109)

<sup>142</sup> Richards appeals to accounts offered by John Hibben (1896), Wm Stanley Jevons (1874), and John Stuart Mill (1843). According to Richards, the strength of an analogy depended upon the number of similarities and the lack of differences between the source and target (Richards 1997,79). This, of course, as we have seen neglects to consider whether the similarities and differences were relevant or not.

<sup>143</sup> See Gildenhuys (2004, 604 *fn* 5) for the disanalogies. These same disanalogies were also mentioned in Richards (1997, 1998, 2005) and originally in Darwin’s *Origin*, see the fourth paragraph of Chapter IV.

<sup>144</sup> Emphasis in original.

which he can deduce how species evolved” (Gildenhuys 2004, 606). In other words, Gildenhuys claims that Darwin looks to domestic cases “to support a *deductive*, rather than an *analogical*, inference to the production of species” (Gildenhuys 2004, 606).

Gildenhuys’ objection to the standard interpretation, while not as radical as Richards’, depends upon the same simplistic conception of an argument from analogy. As a result, both Gildenhuys and Richards fail to recognize the role of considerations of relevance when assessing the plausibility of arguments from analogy. Thus, even if his interpretation of Herschel is correct, Gildenhuys overlooks the possibility that his objection and the standard interpretation of Darwin’s methodology are not mutually exclusive. Darwin might avail himself of both strategies; he may have grouped similar phenomena together via their similarities and he may have relied on the similarities (and lack of relevant differences) to posit the causal efficacy of natural selection. While we are in a position to reject Richards’ objection, Gildenhuys’ objection is wrong only insofar as it denies Darwin used analogy as a justificatory inference in the *Origin*; Darwin may have used both forms of analogical reasoning.<sup>145</sup>

In my discussion of analogical reasoning in the first chapter, I stated that a metaphor consists of three types of analogical comparisons: the positive analogy, the negative analogy, and the neutral analogy. The neutral analogy motivates analogical arguments in order to determine whether a proposed correspondence between the source and target is a similarity or difference. Much like my discussion of metaphor, Herschel’s account of induction— which focuses on the similarities between phenomena to motivate general statements (hypotheses)— also generates arguments for recognizing further similarities, which become the basis for testing those hypotheses.

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<sup>145</sup> Note that I do **not** claim that Darwin did in fact use both methods, nor do I claim that he was conscious of using either. Instead, I argue that his using one form of analogy does not in itself establish that he relied solely on one, and not the other.

Gildenhuys argues that Darwin's metaphor of selection is supported by the positive analogy, the similarities between artificial and natural selection stated in the first couple of chapters of the *Origin* (Gildenhuys 2004, 606). Darwin looks to the different breeding techniques amongst the various plants and animals, and creates a general hypothesis about selection (Gildenhuys 2004, 606). Darwin then uses the hypothetical cases in Chapter Four to test his hypothesis (Gildenhuys 2004, 606). On Gildenhuys' description, Darwin's methodology—and Herschel's account of induction—closely parallel that of a metaphor guiding scientific research.

Herschel's methodology is consistent with the standard interpretation because, like metaphor, it motivates a test of a hypothesis. As we have seen, this test often takes the form of an argument. Gildenhuys believes that Darwin uses a deductive argument in the *Origin*; however, he and Richards rely on an impoverished sense of analogy. Because Gildenhuys fails to recognize that Darwin had considered whether the differences between artificial and natural selection were relevant, he concludes that Darwin did not use an argument from analogy in the *Origin*.

I suggest that Herschel's account of induction captures a process of reasoning from comparison for similarity and difference that motivates arguments, including analogical arguments.<sup>146</sup> As we have seen, metaphors can also motivate analogical arguments and, as Mill describes, the term analogy may have been conflated with metaphor. An analogical argument could be motivated by the account of induction described by Herschel or by a metaphor because both are so very similar to each other. Hence, both argument from analogy and metaphor can be present in the *Origin* without conflict. Gildenhuys' objection demonstrates that how we define analogy gives rise to the subtle distinction between analogy and metaphor. Given that analogy and metaphor are often conflated and the

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<sup>146</sup> In the future, I would like to consider how similar Herschel's account of induction is to metaphor and the practice of using models in science.

definition of analogy in the 19<sup>th</sup> century was not static, it comes as no surprise that it is difficult to attribute a particular view to Darwin's overall argument strategy in the *Origin*.

## **§2.5 Differing Accounts of Induction, Differing Accounts of Analogy**

As we have seen, both Richards and Gildenhuys appeal to historical context when formulating their objections to the standard interpretation. Unfortunately, they relied on a narrow sense of analogy, one that did not include the consideration of whether differences were relevant or not.<sup>147</sup> As a result, neither believed that Darwin used an analogical argument in support of his theory. Of course, any attempt to explain Darwin's overall argument in the *Origin*, including the standard interpretation, will be informed by a particular philosophical stance toward analogy in general. What is philosophically interesting is why the reliance on analogy, which was used by scientists prior to the publication of the *Origin*, went largely unrecognized by philosophers of science post-publication.

At the time the *Origin* was published, the use of analogy was an accepted part of scientific practice, recognized by the likes of Galileo, Newton, and Lyell. For example, J.E. McGuire notes that Newton's third rule of philosophizing includes analogical reasoning, which enables scientists to make claims about unobservable entities from observable ones (McGuire 1970, 3-4).<sup>148</sup> According to McGuire, "Newton was primarily concerned to investigate the natural world, and he tended to view [his] third rule as describing a methodological procedure compatible with his experimental empiricism" (McGuire 1970, 11). Newton states in his third rule, "we are certainly not to relinquish the evidence of experiments for the sake of dreams and vain fictions of our own devising; nor are we to

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<sup>147</sup> White, R., J. Hodge, and G. Radick (manuscript) will argue that other, more robust senses of analogy were available during the 19<sup>th</sup> century, giving Darwin more options than those stated by Richards and Gildenhuys.

<sup>148</sup> McGuire refers to this as the problem of transduction (McGuire 1970, 3-4).

recede from **the analogy of Nature**, which uses to be simple, and always consonant to itself” (McGuire 1970, 3).<sup>149</sup> “The analogy of nature,” according to Newton, implies that all entities, observable or not, behave in an analogous fashion and are thus, subject to experiment (McGuire 1970, 4).

Charles Lyell offers another example of analogical reasoning in scientific practice in the *Principles of Geology*. In support of his theory of uniformitarianism of geological processes, Lyell observes that the collections of fossils belonging to Georges Cuvier and Jean-Baptiste Lamarck have “a powerful effect in dispelling the illusion which had long prevailed concerning the absence of **analogy** between the ancient and modern state of our planet” (Lyell 1872, 85).<sup>150</sup> Lyell further notes,

It may be necessary in the present state of science to supply some part of the assumed course of nature hypothetically; but if so, this must be done without any violation of probability, and always consistently with the analogy of what is known both of the past and present economy of our system.

(Lyell 1872, 299)

Lyell’s *Principles of Geology*, as well as Herschel’s *Discourse*, influenced not only Darwin’s methodology, but also his scientific method. Apparently, analogy played a more robust role in scientific practice in the 19<sup>th</sup> century and yet, 20<sup>th</sup> century philosophers of science have largely ignored it. Indeed, the logical positivists, and those endorsing the Received View, vigorously argued against its use outside of the context of discovery. This disconnection between scientific practice and philosophy of science arose as a consequence of Mill’s triumph in his debate over induction with Whewell. In this section, I explore how the status of analogical reasoning changed as a result of the influence of Mill’s account of induction on the philosophy of science.

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<sup>149</sup> Originally found in Sir Isaac Newton, *The Mathematical Principles of Natural Philosophy*, translated by Benjamin Motte (London, 1729), vol. II, p. 203. The emphasis is mine.

<sup>150</sup> Emphasis mine.

In the famous mid-19<sup>th</sup> century debate over the nature and role of induction, Mill and Whewell, among others, sought to improve upon 19<sup>th</sup> century science, which entailed revising the account of induction advanced by Francis Bacon in the *Novum Organum*.<sup>151</sup> The Mill-Whewell debate is of special interest because each represents a different stance toward analogy.<sup>152</sup> In fact, Whewell acknowledges that analogy may play a justificatory role as part of his account of induction while Mill does not. Even though Mill and Whewell considered themselves empiricists, they each produced an epistemologically distinct revision to Baconian induction.

Whewell predicated his account of induction on an epistemology that admitted “the use of both sensations and conceptions” (Snyder 2006, 50). In other words, Whewell believed that knowledge not only came from sensory inputs, but also required rational conceptions with which to organize those inputs in some way (Snyder 2006, 39). Addressing Bacon in the *Novum Organon Renovatum*, Whewell lists “space, number, case, composition, resemblance” as some of the concepts used to organize information from the senses (Whewell 1858, 30). According to Whewell, these concepts are applied to facts provided by the senses and thus “render observation certain and exact” (Whewell 1858, 58). Thus, Whewell describes knowledge as a synthesis, noting,

In all cases, Knowledge implies a combination of the Thoughts and Things. Without this combination, it would not be Knowledge. Without Thoughts, there could be no connexion; without Things there could be no reality. Thoughts and Things are so intimately combined in our Knowledge, that we do not look upon them as distinct.

(Whewell 1858, 25)

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<sup>151</sup> See Bacon (1857-1861).

<sup>152</sup> When contrasted with the account of Mill, Herschel’s account of induction bears a striking resemblance to that of Whewell’s. Having discussed Herschellian induction in a previous section, I have omitted Herschel from this section, allowing Whewell to be representative of the type of induction also noted by Herschel. Whewell and Herschel’s accounts differ with respect to the role of analogy. Where Herschel employs an analogy to establish *vera causae*, Whewell believes *vera causae* are found only through a consilience of inductions (Ruse 1978, 324 & 328)

The combination of facts and thoughts, once conceived in the mind, are then further categorized in relation to other conceptions, using various forms of inference to develop a hypothesis (Whewell 1858, 78). Whewell recognized multiple inference patterns—such as enumerative induction, eliminative induction, and analogical reasoning—as having a role in the development of laws (Snyder 2006, 73).<sup>153</sup> In contrast to Mill, Whewell understood this process of colligation to be a source of evidential support (Snyder 2006, 75).

Mill's account of induction differs from Whewell's insofar as Mill grounds his in "an ultra-empiricist epistemology" that relies strictly on sense experience for knowledge (Snyder 2006, 126). In *A System of Logic*, Mill distinguishes his account from Whewell's by stating, "Whatever we are capable of knowing must belong to the one class or to the other; must be in the number of the primitive data, or of the conclusions which can be drawn from them" (Mill 1900, 20). For Mill, sensory inputs are brute facts on which scientists use induction to form generalizations. Later in his text, Mill specifically addresses Whewell's version of induction, declaring that Whewell's a priori organization of the sense data is not a part of induction; instead, what is taking place is merely the enumeration of distinct, yet similar, instances (Mill 1900, 454).

Given his epistemic commitments, Mill drastically limits the types of inference used in the generalization of data. In particular, Mill's account of induction does not include analogical inference. For example, Mill states

It is hardly necessary to add that, however considerable this probability may be, no competent inquirer into nature will rest satisfied with it when a complete induction is attainable; but will consider the analogy as a mere guidepost pointing out the direction in which more rigorous investigations should be prosecuted.

(Mill 1900, 396)

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<sup>153</sup> Whewell provides an example of the reasoning Kepler used to develop the elliptical orbits of the planets. This reasoning goes beyond the simple enumeration of facts. There is a connection between the separate facts that highlights their similarity. See Whewell (1858) pp. 110-111.

Analogy for Mill is not an argument form that can be used to justify hypotheses; instead, its value is to suggest directions for scientific investigation (Mill 1900, 396-397). Both Mill and Whewell reacted negatively to the methodology Darwin used in the *Origin*.<sup>154</sup> Although each rejected Darwin's theory of natural selection, their approaches differed. Whewell included analogical reasoning as a source of justification in his account of induction and thus, interpreted Darwin's analogy as an attempt to justify natural selection. However, his analysis of Darwin's method challenges the cogency of the analogical inference drawn between domesticated breeds and natural species. According to Whewell, humans are uniquely rational creatures, which makes them relevantly dissimilar from domesticated animals (Snyder 2006, 200). Given the presence of a relevant difference between animals and man, the analogy that Darwin drew was weak according to Whewell and, thus Darwin's theory, although using analogical reasoning, remained unconfirmed (Snyder 2006, 196).<sup>155</sup>

Although Mill commended Darwin's reasoning to Henry Fawcett, who reported this fact in a letter to Darwin, dated 16 July 1861, Mill was convinced that Darwin did not use an analogy to justify his thesis of natural selection.<sup>156</sup> In a footnote in the eighth edition of *A System of Logic*, Mill writes

It is unreasonable to accuse Mr. Darwin (as has been done) of violating the rules of Induction. The rules of Induction are concerned with the conditions of Proof. Mr. Darwin has never pretended that his doctrine was proved. He was not bound by the rules of Induction, but by those of Hypothesis. And these have seldom been more completely filled. He has opened a path of inquiry full of promise, the results of which none can foresee.

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<sup>154</sup> According to Hull (2003) of the three most influential figures of scientific methodology of the time, the discussion of the *Origin* by John Herschel, William Whewell, and John Stuart Mill amounted to only six printed pages.

<sup>155</sup> One might think that Whewell's objection serves only to limit the application of evolution; in short, evolution would only apply to animals. For the creationist, the acts of God applies to every living being, so what would apply to plants and animals must also apply to man.

<sup>156</sup> In the letter, Fawcett writes "I was spending an evening last week with my friend Mr. John Stuart Mill, and I am sure you will be pleased to hear from such an authority that he considers that your reasoning throughout is in the most exact accordance with the strict principles of logic. He also says the method of investigation you have followed is the only one proper to such a subject" (Hull 1973, 277).

(Mill 1900, 355)

For Mill, an inductive inference requires a direct observation of the phenomena, a condition that Darwin could not meet (Snyder 2006, 194).<sup>157</sup>

The Mill-Whewell debate was not confined to scientific circles; it extended into the social and political spheres as well. Even though Whewell was a highly respected philosopher/historian/theologian/scientist who is credited with coining many of the terms used in 19<sup>th</sup> century science, his account of induction never gained widespread acceptance.<sup>158</sup> His epistemology was associated with intuitionism and “seemed out of step with the dominant...outlook of the time” (Snyder 2006, 326). By contrast, Mill’s account took hold in a Victorian society that was primed for empiricism and utilitarianism.<sup>159</sup> His highly popular *A System of Logic* left a methodological and philosophical legacy in which induction was depicted as “a narrow logical operation involving only enumeration or eliminative forms of reasoning” (Snyder 2006, 331).<sup>160</sup> According to Snyder, “Mill, almost single-handedly, convinced the Anglo-American Philosophical world that induction could not involve any contributions of the mind, or any type of creativity, but rather was an extremely narrow logic” (Snyder 2006, 331). The exclusion of creativity from scientific inference laid the foundations for a distinction between the methods of discovery and that of justification; “scientific discovery was not, and could not be, inductive” (Snyder 2006, 331-332). Thus, the use of analogical reasoning was ignored by philosophers of science, in spite of its continued

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<sup>157</sup> Snyder suggests that Mill understood Darwin to be suggesting only a hypothesis, and not proof of his theory, because the footnote cited in the text appears in a section on hypotheses (Snyder 2006, *fn* 159).

<sup>158</sup> For example, Whewell provided such terms as, anode, cathode, and ion at the request of Michael Faraday (Snyder, 2009). Notably, Whewell is believed to be the first to use the term “scientist” as well. See Ross (1962) p. 72.

<sup>159</sup> See both Hull (1973) p. 5 and Snyder (2006) p. 326. It should also be noted that Whewell died nine years prior to Mill and that Mill was a visible member of British Parliament (Snyder 2006, 323).

<sup>160</sup> The book had a total of eight editions from 1843 to 1872 (Jacobs 2003, 202).

use by scientists. As we shall see in the next chapter, it was not until the mid-1950's that philosophers begin to consider analogy as a legitimate inference within scientific practice.<sup>161</sup>

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<sup>161</sup> Rudolf Carnap, one of the logical empiricists did attempt an account of analogy. Carnap (1980) contains his attempt to formulate principles for analogy. I thank Paul Bartha for calling this to my attention.

## Chapter Three

### A Renewed Interest in Analogical Reasoning

After the publication of the *Origin*, Millian models of induction dominated philosophical thinking about analogical reasoning until the middle of the twentieth century (Snyder 2006, 331). Supporters of logical positivism, such as the members of the Vienna Circle and Hans Reichenbach's Berlin School, discounted arguments based on analogy.<sup>162</sup> This philosophical tradition, canonized as the Received View by Frederick Suppe (1974) and Wesley Salmon (1989), assumes a commitment to eliminate scientific explanations couched in "terms of abstract metaphysical entities" (Suppe 1974, 6).<sup>163</sup> In *Experience and Prediction*, Hans Reichenbach describes the initial motivations for the adherents of the Received View stating,

Though there is no philosophic system which unites these groups there is a common property of ideas, principles, criticisms, and working methods—all characterized by their common descent from a strict disavowal of the metaphor language of metaphysics and from a submission to the postulates of intellectual discipline.

(Reichenbach 1938, v)

The Received View assumed a "mechanistic materialism"—a view that maintains the processes and laws of nature can be known via empirical inquiry without the influence of *a priori* reasoning (Suppe 1974, 8). For those endorsing the Received View, the "observation of the world is immediate in the sense that no *a priori* or conceptual mediation is involved in

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<sup>162</sup> According to John Passmore, "The logical positivists thought of themselves as continuing a nineteenth century Viennese empiricist tradition, closely linked with British empiricism and culminating in the antimetaphysical, scientifically oriented teaching of Ernst Mach" (Passmore 1967, 52). For more information regarding the Berlin School, see Rescher (2006).

<sup>163</sup> Suppe credits the name this tradition, *The Received View of Theories*, to Hilary Putnam (Suppe 1974, 3). See Putnam (1962).

observational knowledge” (Suppe 1974, 8). The influence of Mill’s “ultra-empiricism” on the supporters of the Received View is evident, especially toward the use of *a priori* reasoning and metaphorical language describing reality (Snyder 2004, 112).<sup>164</sup> Their attitude toward analogical reasoning appears in the Received View’s doctrine of cognitive significance:

The only meaningful discourse was that done either in terms of phenomenal language or using terms which were abbreviations for (that is, could be phrased equivalently as) expressions in phenomenal language; any assertions failing to meet these conditions were metaphysical nonsense.

(Suppe 1974, 13)<sup>165</sup>

For supporters of the Received View, ordinary language was neither part of the phenomenal language nor the axiomatic calculi and therefore, the tools of language such as metaphor and analogy, were considered “covertly” metaphysical (Suppe 1974, 13). This is particularly evident given what supporters of the Received View required for a theory to be confirmed. For example, describing the hypothetico-deductive (H-D) model of confirmation, Carl Hempel claims, “the defining characteristic of an empirical statement is its capability of being tested by a confrontation with experiential findings, *i.e.* with the results of suitable experiments of focused observations” (Hempel 1965, 3). According to Hempel, this feature of empirical statements enables philosophers of science to delineate statements that have empirical content from those of math and logic, which require no test; statements that are “formulations of transempirical metaphysics” are not capable of being tested (Hempel 1965, 3). On the H-D model of confirmation, all justification consists in the test of the observable

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<sup>164</sup> This runs counter to Whewell’s account of induction. For Whewell, *a priori* reasoning helped to categorize observational inputs (Whewell 1858a, 25).

<sup>165</sup> According to Suppe, Carnap highlights Wittgenstein’s doctrines of a logically perfect language as the influential role in the formation of this view. The need for language to be cognitively significant is apparent in Frege’s *On Sense and Reference*, see Moore (1993) and Carnap (1936). Alex Rosenberg further notes that the advance in logic by Bertrand Russell, Alfred North Whitehead, and Gottlob Frege, as well as the discovery of Einstein’s theories of relativity, contributed to the development of the Received View (Rosenberg 2012, 15-16). Rosenberg states that Einstein’s contribution included defining “the most fundamental concepts in science—length, time, velocity, mass” in terms of “experiences, empirical manipulations, and operations” (Rosenberg 2012, 16). In other words, Einstein’s scientific practice emphasized an empiricist epistemology (Rosenberg 2012, 16).

consequences of a theory, which proscribes the use of analogical inferences. As Hempel explains, while analogies “may prove useful ‘in the context of discovery,’ i.e. they may provide effective heuristic guidance in the search for new explanatory principles [...] an analogical model itself explains nothing” (Hempel 1965, 441).<sup>166</sup>

Mill’s ultra-empiricism exerted its influence on the philosophy of science; at least until the mid-1950’s when Mary Hesse revived philosophical interest in the role of analogical reasoning in science. For Hesse, to merely claim that models are “purely subjective, psychological, and adopted by individuals for private heuristic purposes” does not adequately describe their “function in science” (Hesse 1966, 165). Rather, Hesse claims that models differ from literary metaphors; models are “meant to be exploited energetically and often in extreme quantitative detail and in quite novel observational domains...” (Hesse 1966, 169). Although, as we shall see, Hesse’s account suffers from a key limitation, her renewed interest in analogy paved the way for more sophisticated accounts, like that provided by Paul Bartha.

### § 3.1 The Contributions of Mary Hesse

In the middle of the twentieth century, Michael Scriven (1958, 1959, 1962, 1963) and Norwood Russell Hanson (1959) initiated critiques of the Received View, challenging both Carl G. Hempel’s deductive-nomological (D-N) model of scientific explanation and the hypothetic-deductive model of confirmation (Salmon 1989, 4).<sup>167</sup> Only a few years prior, Mary Hesse drew attention to the capacities of analogies associated with iconic models for generating predictions. Of course, Hesse did not challenge the Received View outright.

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<sup>166</sup> In his discussion of the H-D model of explanation, Hempel notes that the context of discovery is “*not logically determined*” and thus, the analogies in the context of discovery, while helpful, are not inferential (Hempel 1965, 5).

<sup>167</sup> According to Salmon, the D-N account “had been promulgated by R.B. Braithwaite (1953), Carl G. Hempel (Hempel and Oppenheim 1948), Ernest Nagel (1961), and Karl Popper (1935, 1959), among many others—supplemented perhaps by Hempel’s then recent account of statistical explanation (Hempel 1962)” (Salmon 1989, 4).

Instead, she sought to make philosophical accounts of explanation consistent with actual practice, particularly that of modeling. According to Hesse,

...one of the main functions of analogy or model is to suggest extensions of the theory by considering extensions of the analogy, since more is known about the analogy than is known about the subject matter of the theory itself...A collection of observable concepts in a purely formal hypothesis suggesting no analogy with anything would consequently not suggest either any directions for its own development.

(Hesse 1952, 291)

Formal theories, for Hesse, do not direct enquiry because shared mathematical terms offer no guidance for further research. The neutral analogy between a source and target, however, provides possible directions for future research that a mathematical formula cannot.

In *Science and the Human Imagination*, Hesse points out that

Although Mill's canons describe the method used in an early stage in the development of a science—the stage of qualitative analysis of causes and effects—they do not fully describe the type of analogy we are dealing with in advanced mathematical physics.

(Hesse 1954, 143)

In other words, limiting analogical reasoning to contexts of discovery neglects a salient feature of scientific practice: scientists rely on analogical inferences to “provide a method of hypothesis selection” (Hesse 1966, 77). Hesse writes “analogical argument is necessary only in situations where it has not been possible to observe or to produce experimentally a large number of instances in which sets of characters are differently associated” (Hesse 1966, 76). As such, analogical reasoning assists where “Bacon's or Mill's method of agreement and difference cannot be applied” (Hesse 1966, 76).

For Hesse, an analogical inference may be weaker than enumerative induction, but “it has the advantage of being applicable where straightforward generalization is not” (Hesse

1966, 76). For example, Hesse uses a table of the properties of water waves, sound waves, and light waves to illustrate how the similarities between the properties of the three phenomena are consistent with Mill's method of agreement; yet, she also states,

In all three cases there are present motion, something transmitted indirectly from one place to another by hitting an obstacle, and bending round obstacles. This suggests that the three processes are perhaps alike in more fundamental respects, and in order to investigate this possibility, we look more closely at the one of the three we know most, namely, water waves.

(Hesse 1966, 11)<sup>168</sup>

In other words, to learn more about sound Hesse claims “we construct one-to-one correspondences between the observable properties [...] and we are then in a position to test the mathematical wave theory as a theory of sound” (Hesse 1966, 14).

Not only does an analogy generate research questions, but limiting analogical reasoning to “the psychology of discovery, and not as a subject for logical analysis... seems to arise more from the absence of any satisfactory analysis of the analogy-concept itself than from any strong convictions that its role in science is unimportant” (Hesse 1959, 79). Given that “scientific theories describe nature in terms of *analogies* drawn from familiar types of experience,” an account of analogy is needed (Hesse 1954, 12-13). This is especially the case because neither the classical accounts nor the accounts of analogy in modern logic adequately capture the justificatory role played by analogical reasoning, particularly in cases where scientists test their hypotheses via the analogies associated with models (Hesse 1966, 57).<sup>169</sup> Intended for more than merely locating “an identity of logical or mathematical

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<sup>168</sup> This is also compatible with Mill's assessment of analogy as a guide for scientific investigation in his *System of Logic*, see Mill (1900, 355).

<sup>169</sup> Hesse writes that Keynes (1921) provides criteria for analogical inferences in a way that is consistent with the physical theories of the time. However, it offers only “a superficial description of what the scientist does, and goes no way towards uncovering the assumptions which lead him to do it, or towards showing that prediction on the basis of theories thus produced is reasonable” (Hesse 1954, 143). In short, Keynes' account does not offer a reason for why the scientists find the analogical inferences as plausible or as justified. For Keynes' criteria, see Keynes (1921) page 222.

structure,” scientists draw on the analogies associated with models to make predictions and thus, test their hypotheses (Hesse 1954, 143).

Taking inspiration from Max Black’s (1962) account of interactive metaphor, Hesse describes the conditions under which a metaphor contributes to a scientific explanation (Hesse 1966, 171). One condition in particular is the ability to generate predictions. Hesse maintains, “Without models, theories cannot fulfill all the functions traditionally required of them, and in particular that they cannot be predictive” (Hesse 1966, 5). The analogies underwriting a model not only assist in generating predictions, they also offer epistemic justification as well. Hesse distinguishes explanation from justification by appealing to two different conceptions of model, which appear in the hypothetical dialogue between a Duhemist and a Campbellian in *Models and Analogies in Science*.

The Duhemist, on Hesse’s account, represents the views expressed by Pierre Duhem in his 1914 book, *La Théorie Physique*.<sup>170</sup> For Duhem, models “may be useful psychological aids in suggesting theories” but they function only as a heuristic (Hesse 1966, 2-3). Once the formalization is established, the model, and its analogies, is disposable. In short, if there are analogies underwriting models, they are temporary and non-logical.

The Campbellian, in contrast to the Duhemist, believes that analogies play a more epistemic role in scientific practice, and it is this view that Hesse would like her contemporaries to consider. For Campbell, models are necessary for two reasons: to extend a theory and to generate predictions (Hesse 1966, 4).<sup>171</sup> Hesse refers to these two conceptions of model as model<sub>1</sub> and model<sub>2</sub>. Model<sub>1</sub>, according to Hesse, is “when we consider a theory based on a model as an *explanation* for a set of phenomena, we are considering the positive and neutral analogies, not the negative, which we already know we

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<sup>170</sup> A translation of the book exists; see Duhem (1954).

<sup>171</sup> Actually, Hesse (1966) advocates for a position that holds analogies are “*essential* and *integral* components of theories” (Suppe 1974, 99). Paul Bartha will address this particular aspect of Hesse’s account later, and so I table it until then.

can discard” (Hesse 1966, 9-10).<sup>172</sup> For Hesse, the negative analogy is a set of the relevant differences between the source and target; these would not be used to support a particular explanation.

Hesse’s model<sub>2</sub>, which includes the negative analogy along with the positive and neutral analogies, plays a role in establishing grounds for believing proposed similarities between a source and the phenomenon are plausible. The presence of similarities in model<sub>2</sub>, and the lack of relevant differences, warrants suggesting that the claims made by scientists about the connection between source and target is plausible (Hesse 1966, 26-31).<sup>173</sup> To claim that something is a model (that is, Hesse’s model<sub>2</sub>) is to emphasize the similarities between source and target, but it is not merely the lack of relevant differences that convinces scientists that the model is a sound choice.<sup>174</sup>

Hesse also identifies two further types of analogical reasoning that play a role in testing a model. The first is the formal analogy, which amounts to “the one-to-one correspondence between different interpretations of the same formal theory” (Hesse 1966, 68). For example, “the formal analogy between elliptic membranes and the acrobat’s equilibrium” are both described by Mathieu’s Equation (Hesse, 1966, 69). Instead of the horizontal correspondences of properties based on similarity between the source and target, the same formalism, Mathieu’s Equation, represents the source and the target (Hesse 1966, 68). This type of analogy, according to Hesse, is “useless for prediction” because there is no reason for anyone to expect a shared formalism to reveal the “similarity between corresponding terms” or to provide an impetus for making a prediction (Hesse 1966, 69). Without observable, pretheoretic similarities between source and target, the neutral analogy is not clear and cannot motivate predictions (Hesse 1966, 69).

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<sup>172</sup> Emphasis added.

<sup>173</sup> I have replaced Hesse’s “analogue” with “source” and “target”—to distinguish between the more familiar “source” of the analogy and the “target” to which the extended.

<sup>174</sup> Model will hereafter refer to Hesse’s model<sub>2</sub>, unless stated otherwise.

The second type of analogy is, on Hesse's account, the material analogy (Hesse 1966, 68). Analogies of this kind focus on the observed similarities between sets of properties of the source and the target. Recall that for an argument presented in tabular form, the similarity relations are the horizontal correspondences between the source and target domains (Hesse 1966, 68). An important feature of the material analogy is that the perceived similarities and differences are unaffected by theory; they are, according to Hesse, pre-theoretic (Hesse 1966, 68). In fact, if scientists are to make predictions from the model, then the material analogies "must be [the] *observable* similarities between corresponding terms and must not depend on a theory of the explicandum" (Hesse 1966, 69).<sup>175</sup>

Although "the description of [the] similarities and differences between two analogues is a notoriously inaccurate, incomplete, and inconclusive procedure," Hesse maintains that an analogical inference "provide[s] a method of hypothesis-selection which is justifiable on at least some of the criteria for such selection" (Hesse 1966, 76-77). By adhering to the following criteria for developing the material analogy, inferences based on analogical reasoning are justified. According to Hesse, the material analogy should meet the following criteria:

1. "The horizontal relations in the tabular representation should be based on similarity, "where similarity can [...] be reduced to identities and differences between sets of characters making up the terms" (Hesse 1966, 87).
2. "The vertical relations should reveal "causal relations in some acceptable scientific sense, where there are no compelling a priori reasons for denying that causal relations *of the same kind* may hold between terms of the explicandum" (Hesse 1966, 87).
3. "The essential properties and casual relations of the model have not been shown to be a part of the negative analogy between model and explicandum" (Hesse 1966, 91).

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<sup>175</sup> Hesse states that these models are often considered to suggest themselves due to the observable, pretheoretic similarities (Hesse 1966, 69-70). Given that these similarities are not strict identities, there is a spectrum that ranges between identity and difference (Hesse 1966, 70-71).

Hesse believes that relying on the above criteria enables scientists to extend a causal relation believed to be in the source to the target (Hesse 1966, 78). Hesse maintains the “arguments from models involve those analogies which can be used to predict the *occurrence* of certain properties or events, and hence the relevant relations are causal, at least in the sense of implying a tendency to co-occur” (Hesse 1966, 78). In other words, the causal relation (or vertical relation) in the source induces scientists to look for a similar relation in the target. By appealing to the observable similarities and the absence of relevant differences between the source and target, scientists posit the likelihood of a particular property (referred to as the hypothetical analogy). If scientists detect the predicted property, then that constitutes evidence for an analogous cause in the target. This is not mere postulation by scientists; rather, the process meets the requirements of prediction. Scientists state an observable property expected in the target and thus, test the outcome of the inference.<sup>176</sup>

According to Hesse,

An observation statement may always said to be falsifiable in the sense that the circumstances it describes...may always in principal be repeated...[and] a scientific theory is required to be falsifiable in the sense that it leads to new observation statements which can be tested, that is, that it leads to new and perhaps unexpected and interesting predictions.

(Hesse 1966, 36-37)

If the state of affairs described by the prediction obtains, then the model is conditionally confirmed, and if the states do not obtain, then the results “may be regarded as sufficiently serious to refute the theory and model<sub>1</sub> together” (Hesse 1966, 33). Even if we do not know

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<sup>176</sup> Hesse notes “in analyzing the nature of analogical argument in general, it is only necessary to point out that the argument, if valid, carries over the *same* sense of causal relation from model to explicandum, in virtue of the relations between the characters which the model and explicandum share” (Hesse 1966, 79).

exactly what the cause is, “the analogical argument presupposes a stronger causal relation than mere co-occurrence” (Hesse 1966, 84). Consequently, the confirmation of the expectations of an analogical model provides grounds for more than mere correlation. As Hesse maintains,

Analogical description is not literal description, as previous generations of scientists imagined, but neither is it entirely unrelated to the reality of nature. It owes its success both to the fidelity to nature as revealed in experiments, and to the fertile imagination which selects the appropriate analogies from familiar experience and familiar types of language, and thus exhibits relations between one aspect of experience and another.

(Hesse 1954, 13)

This is, of course, a pivotal role for analogy that challenges the impoverished account of analogical reasoning found in the empiricism of Mill’s classical positivism and the logical positivism of the Received View. Analogical reasoning not only figures in the context of discovery as suggested by Mill. It can, according to Hesse, be used to motivate and test predictions based on models.

### **§3.2 Paul Bartha and the Limitations of Hesse’s Account**

Today, as Bartha observes, “research on analogy has been transformed by the efforts and interests of AI [artificial intelligence] researchers, psychologists, and cognitive scientists” and yet, normative philosophical questions about the justification and evaluation of analogical arguments have yet to be adequately addressed (Bartha 2010, vii-viii). While Hesse sought to provide answer to these questions, her efforts were intimately connected with locating analogical reasoning within a framework of confirmation, which resulted in a “limited” account of the plausibility of analogical reasoning (Bartha 2010, 45). For example, Bartha takes issue with Hesse’s first criterion (the requirement of a material analogy), which states, “An acceptable analogical argument must include ‘observable similarities’ between

domains” (Bartha 2010, 42).<sup>177</sup> For Bartha, accounts of analogy that relies only on observed similarities cannot make sense of the abstract associations found in recent computational theories in cognitive science (Bartha 2010, 42).<sup>178</sup>

Computational theories of analogical reasoning, such as those put forth by Dedre Gentner (1983) and Holyoak and Thagard (1989), do not always rely on observable similarities but are based on high level abstractions or appeals to “systematicity” (Bartha 2010, 42).<sup>179</sup> Systematicity allows for “complex, high-level networks of relations and attributes ” to be included in the horizontal correspondences (Bartha 2010, 93). Hesse insists that the horizontal correspondences in a tabular representation are **observable** similarities, but this would omit any correspondences that depend upon abstract similarities between the source and target.<sup>180</sup>

Hesse’s second criterion—which assumes that the relevance relation is a causal relation—is also “too restrictive” (Bartha 2010, 43). It “rules out analogical arguments where there is no causal knowledge of the source domain” (Bartha 2010, 43).<sup>181</sup> For Bartha, Hesse’s evidence of the properties’ “tendency to co-occurrence” should not be based solely on causal relations; rather, relevance can include other relations, such as formal or statistical correlations (Bartha 2010, 43). As an example, Bartha cites Ben Franklin’s journal entry, in which he recounts the inspiration for his experiments on electricity. Recall that in this example, Franklin lists the many similarities between the source (fluid electricity found in a

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<sup>177</sup> Bartha also takes exception to what he calls the “similarity-identity reduction” (Bartha 2010, 41). This reduction is found as an assumption in Hesse’s first criterion. It assumes that the similarity relation between properties corresponding horizontally is in degrees of similarity. That is to say, the two properties are either dissimilar or weakly/strongly similar.

<sup>178</sup> The types of scientific models supported by analogy vary greatly; as does the way philosophers of science understand them. Frigg and Hartman (2009) list at least twenty-one categories of models found in science. See Frigg, Roman and Stephan Hartmann, “Models in Science”, *The Stanford Encyclopedia of Philosophy (Summer 2009 Edition)*, Edward N. Zalta (ed.), URL = <<http://plato.stanford.edu/archives/sum2009/entries/models-science/>>. In light of such variety, Bartha believes Hesse’s account is “too restrictive” in key areas (Bartha 2010, 40-45).

<sup>179</sup> For a detailed treatment of computational theories, see Bartha (2010) Chapter 3.

<sup>180</sup> An example of an abstract correspondence might be causal relations. See Clement, C. A. and D. Gentner (1988), pg. 412.

<sup>181</sup> As we shall see, Bartha’s own account includes a variety of types of analogical arguments; only one of which has a causal vertical relation.

laboratory) and a target (lightning). Similarities such as emitting light, quick motion, conductivity among metals, smelling like sulfur, and destroying animals are not known to be causally related in the source; but there is “a strong correlation” between them (Bartha 2010, 44). Even so, Franklin’s experiment, and his appeal to analogy, would not be considered an instance of analogical reasoning by Hesse’s account.<sup>182</sup>

Hesse’s final criterion, in Bartha’s view, is also problematic. Hesse states that the “essential properties and causal relations of the source” must not be present in the negative analogy (Hesse 1966, 91). On the surface, this seems persuasive. After all, if a key property is part of the negative analogy, then the argument is significantly undermined. Hesse, however, cannot account for how one determines which similarities or differences are essential except in causal terms because determining what is essential requires some consideration of context in order to make sense of the properties brought to bear on the reasoning (Bartha 2010, 44). Context is critical to the creation and analysis of analogical inferences because it makes judgments of what properties are essential or relevant possible. In fact, determinations of which properties are relevant will vary from argument to argument; relevance hinges upon the property comprising the hypothetical analogy and how the properties listed in the source domain are related to it. Although Hesse, drawing on Black’s interactive account of metaphor, recognizes that analogical reasoning is meant to communicate, she overlooks a primary contributor: context (Hesse 1966, 164).<sup>183</sup> Thus, what may be relevant for one argument may not be relevant for another, even if both arguments rely on the same source and target.<sup>184</sup>

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<sup>182</sup> In his diary, Franklin wrote, “But since they agree in all the particulars wherein we can already compare them, is it not probable they agree likewise in this [attraction by points]? Let the experiment be made” (Franklin 1941, 344).

<sup>183</sup> Recall that metaphors and analogies are parts of language of a particular community. Understanding these tools of language, one should be familiar with the community’s usage. The problem, as we shall see, is that the shared assumptions of a community often hinder critique of these language tools.

<sup>184</sup> I thank Paul Bartha for his comments on a previous version of this chapter.

Finally, Bartha also notes a more general “counterintuitive consequence” that follows from Hesse’s account (Bartha 2010, 44). Normally, the strength of an analogical argument increases with the identification of additional relevant similarities. Hesse’s account, however, leaves open the possibility that, as the number of the essential properties in the positive analogy increases, so too does the chance for “an essential disanalogy” (Bartha 2010, 44). Without providing a context for determining whether a property is essential, each addition to the positive analogy increases the overall number of essential properties and in turn, “the risk of an essential disanalogy increases” (Bartha 2010, 44). An increased chance of a disanalogy threatens the cogency of the argument, which directly opposes our intuitions about an increasing positive analogy. For example,

An analogy with fluid flow was extremely influential in developing the theory of heat conduction. Once it was discovered that the heat was not conserved, however, the analogy became unacceptable (according to Hesse) because conservation was so central to the theory of fluid flow.

(Bartha 2010, 44)

According to Bartha, when a theory of analogy bases the cogency of its arguments on overall similarity between the source and target, then all conclusions are either warranted or not warranted (Bartha 2010, 93). As Bartha observes, “overall similarity[...] is an unreliable guide to the plausibility of an analogical argument” (Bartha 2010, 93). Instead, he suggests that the key to assessing the plausibility of an analogical argument is accomplished via the *prior association*, the relationship between the source domain and the hypothetical analogy that comprises the vertical relation (Bartha 2010, 94). As in the case of the *heat as fluid* metaphor, when more properties of the source are recognized as essential, the chance of discovering a relevant difference increases as well.<sup>185</sup>

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<sup>185</sup> While I agree with Bartha that Hesse’s method of requiring an account of similarity prior to establishing the context of the analogical argument will not yield the ability to properly evaluate analogical arguments, I think the

As part of her account, Hesse provides normative criteria for analogical arguments. Surprisingly, however, she does not draw on her criteria in cases of justification, such as whether to accept a hypothesis established by analogy or a competing hypothesis that has no analogy underwriting it (Bartha 2010, 45).<sup>186</sup> In fact, as Bartha points out, the challenge Hesse takes up is to find “a justification for every analogical argument that conforms” to the schematic form of a tabular representation; yet, the magnitude of the task is too grand (Bartha 2010, 45).<sup>187</sup> Although Hesse succeeded in reopening the discussion of analogical reasoning in science, her account of analogy is inadequate. First, she limits correspondences to what is directly observable, omitting higher-level, abstract correspondences between members of the target and source. Second, she only considers causal analogies, which as we will see with Bartha’s account fails to include other types of analogical inferences. Finally, it lacks the resources to inform choices between competing hypotheses, even when one hypothesis is grounded on analogy.

### §3.3 Bartha’s Articulation Model

Cognizant of the shortcomings of Hesse’s account, Bartha proposes a normative account of analogical arguments that he calls the Articulation Model.<sup>188</sup> Bartha’s account

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drive to increase of similarities (which might contribute to the defeat of the argument in question) is actually a good thing. As we shall see, one of the problems with Bartha’s rhetorical device is that a community may not have the resources to cultivate critics capable of a robust critique of its metaphors. In creating an environment of robust critique, a community will need members able to present, and more importantly scrutinize, the possible similarities and differences between a source and target. Being able to challenge what a critic and advocate may assume to be relevant is an important feature of robust critique. I thank Alison Wylie for bringing this problem to my attention.

<sup>186</sup> Bartha notes “Hesse is rather pessimistic about the prospects for providing a philosophical foundation for the argument from analogy” (Bartha 2010, 45). Bartha’s comments are based on the statements made by the Campbellian in Hesse’s hypothetical dialogue; see (Hesse 1966, pp. 55-56).

<sup>187</sup> According to Bartha, Hesse relies on Hempel’s theory of confirmation to choose between a hypothesis supported by analogy and one that is not (Bartha 2010, 45). This strategy is “exceptionally weak” as it requires treating analogy as a single-step induction which is “harder to justify than the original argument” (Bartha 2010, 46).

Bartha also addresses Hesse (1974) in which she considers an a priori justification for analogy (Bartha 2010, 46). This attempt, like the empirically grounded one, neglects the variety of analogical arguments and the contexts that makes them distinct from one another.

<sup>188</sup> Bartha focuses on the arguments found in science and mathematics, offering three reasons for doing so. First, there are numerous, clear-cut examples of analogical arguments to be found in math and science; second, the relevance relations are best understood (and are often the focus) in science; and last, not all of the analogical

differs from Hesse's account by including considerations of context when assessing whether the given similarities and differences are relevant. As we shall see, the context of an argument is important for the evaluation of an argument, which relies on what Bartha calls the prior association to provide an account of which features are relevant to that posited in the conclusion. Bartha defines the prior association as "the vertical relation in the source domain that is the focus of the analogical argument" (Bartha 2010, 25). In the tabular representation of the analogical argument, the prior association is the relation that links the features identified in the source with those attributed to target in the conclusion of the argument (Bartha 2010, 8). The prior association figures prominently in Bartha's Articulation Model because it is this relation (e.g. causal, explanatory, statistical, etc.) that when it is attributed to the target analog, indicates which similarities/differences between the source and target domains are relevant. Instead of providing a top down account of relevance, like we see in Hesse's account, Bartha's Articulation Model relies on the vertical relation of each argument to indicate what the advocate considers relevant.

All analogical arguments can be written in the same tabular form, and yet they differ with respect to their prior association (Bartha 2010, 96). The direction of the prior association (the relationship between the similarities in the premises, P, and what is posited in the conclusion, Q) may be asymmetric, bi-directional, or simply correlative (Bartha 2010, 96).<sup>189</sup> For example, the analogical argument that Darwin develops in the *Origin* depends on an appeal to a causal prior association that is asymmetric. In this case, the members of the source domain—the processes of culling and roguing, the inheritability of traits, and the

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arguments found in math and science use multi-layer reasoning, such as that found in everyday reasoning (Bartha 2010, 9-10). Bartha applies his model to the most basic of analogical arguments with the hope of extending his account to the more difficult types later.

<sup>189</sup> Recall that the set of relevantly similar factors shared by the source and target is the positive analogy and what is in the conclusion is the hypothetical analogy (a factor in the neutral analogy that is the subject of the argument) (Bartha 2010, 14).

plants and animals undergoing artificial selection—are understood to be a causal process that operates in one direction.

Consider another example (Example 4.3) presented by Bartha:

The species *Hallucigenia sparsa*, which flourished about 530 million years ago, was discovered by Charles Walcott in the Burgess Shale of British Columbia. The animal has been described as a 'bizarre creature' with 'seven pairs of stiltlike legs, supporting an elongate trunk with a cylindrical cross section' (Conway Morris, 1977). Given that no animal like *H. sparsa* is known today, it was puzzling even to determine its method of locomotion.

(Bartha 2010, 95)

Bartha notes that some modern animals, such as the tripod fish and elasipodid holothurians, possessed "similar appendages" and as a result, were used as approximate analogs (Bartha 2010, 95). These animals moved rather slowly, leading scientists to claim that *H. sparsa* did not move quickly, if at all (Bartha 2010, 95).

For Bartha, the prior association in the case of *H. sparsa* differs from the causal one that is central to Darwin's selectionist analogy above in that it appeals to functionality. In the case of *H. sparsa* "the prior association is an explanation of the physical form P in terms of an alleged function Q (locomotion)" and the function Q (locomotion) entails the physical form P (Bartha 2010, 95-96). As Bartha notes, "(a) the appendages (in tripod fish) have a form *capable of performing locomotion*, and (b) the legs have the form that they do, at least in part, *because of (for the sake of) that function*" (Bartha 2010, 96).<sup>190</sup> Thus, the prior association in this case is bi-directional or symmetric (Bartha 2010, 97).<sup>191</sup>

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<sup>190</sup> Emphasis in the original.

<sup>191</sup> Another example of an analogical argument with a symmetrical prior association is that concerning "finished artifact[s] that ensure that evidence of wear could be produced by only one pattern of use" (Wylie 2002, 145). Michael Sampson discusses the reasoning used to determine the use of "Humpies...a distinctive flaked-stone tool class recovered from the surface of restricted localities within the Tulare Lake Basin" (Sampson 1991, 193). The use of the tool—whether for "woodworking, hide skinning, or bone working"—entails the particular wear markings, while the wear markings are used to identify the type of tool found by archaeologists (Sampson 1991, 200). The particular markings on the humpies suggest that these tools were used for wood and bone working (Sampson 1991, 201).

If the direction of the prior association is asymmetric, as it is in the case of Darwin's analogy, a further question must be asked: "Is the logical relation between the positive analogy and the hypothetical analogy deductive or inductive?" (Bartha 2010, 97). Bartha refers to the logical relation between the positive and hypothetical analogy as the mode of the prior association. The direction of the prior association coupled with its mode (e.g. whether an asymmetric relation is a logical entailment or not) results in the following categories of analogical arguments.<sup>192</sup>

1. Predictive Analogies (P implies Q). In this category, the positive analogy implies the hypothetical. It can either be deductive as in a mathematical analogy or it can be inductive as with a statistical analogy.
2. Explanatory Analogies (Q implies P). In this category, the hypothetical analogy implies the positive analogy (it explains the positive analogy.) As in the case of Predictive Analogies, these analogies can be either deductive or inductive.
3. Functional Analogies (P implies Q and Q implies P). In this case, the relation runs in both directions. There is no asymmetry and therefore, these types of analogies are inductive only.
4. Correlative Analogies (P, Q). Neither P nor Q implies the other. The relation is a statistical correlation and is therefore, inductive.

(Bartha 2010, 96-97)

Once the mode of the prior association is established, on Bartha's account, arguments from analogy can be evaluated in light of their "potential for generalization" (Bartha 2010, 25). Bartha describes the potential for generalization as finding the "common pattern instantiated in both the source and the target domains" (Bartha 2010, 105). An argument's

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<sup>192</sup> Although only four types of argument are listed, the first two categories, the predictive and explanatory analogies, are further distinguished by their mode. That is to say, an asymmetric prior association can either be understood in terms of deduction or induction, making the four categories actually six (Bartha 2010, 96-97).

potential for generalization depends upon whether the common pattern found in the source can be extended to the target domain (Bartha 2010, 98).

The presence of a prior association and whether it can be generalized form the basis for determining whether an analogical argument is plausible. Informally, Bartha presents a three-step process for assessing plausibility:

1. Elaborate the prior association using the canonical forms and the conditions associated with each form.
2. Determine the relevant factors, both primary and secondary.
3. Assess the argument's potential for generalization.

(Bartha 2010, 102-107)<sup>193</sup>

Throughout these three steps of Bartha's Articulation Model, the context necessary for the evaluation of the argument is provided by Bartha's rhetorical device: the interactions between "an enthusiastic advocate" and a "polite but moderately skeptical critic" (Bartha 2010, 102).<sup>194</sup>

The interactions between the advocate and critic are motivated by the critic's desire for the advocate to be explicit in stating relevant similarities and the advocate's preference for being economical (Bartha 2010, 102). In order to satisfy the critic, the advocate must list every relevant property of the argument (Bartha 2010, 102). This provides an opportunity for the critic to detect a weakness in the argument by increasing the chance of identifying a relevant dissimilarity (Bartha 2010, 102). The critic's need for explicitness, in turn, is tempered by the advocate's preference for economy because the advocate not only seeks to limit the number of potential defeaters, but he/she also recognizes that some differences

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<sup>193</sup> Bartha provides a more extensive, formal account using set theoretic notation to describe the principles applied during the steps. For my purposes, however, the reader need only be familiar with the informal version.

<sup>194</sup> According to Bartha, "the rhetorical device is to imagine that the analogical reasoning is presented by an enthusiastic advocate to a polite but moderately skeptical interlocutor, the critic" (Bartha 2010, 5). For the argument to be considered plausible, it must successfully withstand the examination of the critic. Bartha's rhetorical device "helps to set a standard of justification that can be varied to reflect the demands of different settings," and the background knowledge and experiences of the advocate and critic can be brought to bear in the analysis of the argument (Bartha 2010, 5).

between the source and target are irrelevant (Bartha 2010, 102). An advantage of implementing the rhetorical device of advocate/critic is that the different epistemic and methodological contexts of the community are brought to bear on the determinations of plausibility (Bartha 2010, 5). In other words, the background knowledge of the advocate/critic may vary as well as the community's standard for justification.<sup>195</sup> The device of advocate/critic is not only a mechanism for evaluating analogical arguments, it can contribute to the creation of arguments as well. For example, an advocate might ask what a critic would require to accept a particular argument.<sup>196</sup> The debate between an advocate/critic on the merits of an analogical argument makes explicit the standards and knowledge of the participants, which can be subject to critique.

Throughout his explication of the Articulation Model, Bartha makes use of the advocate/critic device. For example, in the first step, he characterizes the selection of the prior association as a negotiation between the competing motivations of the advocate and critic (Bartha 2010, 103). Each type of prior association—whether functional or correlative—has “preconditions” or standards that are peculiar to it (Bartha 2010, 103). The motivations of explicitness and economy shared by the advocate and critic are held in check by the conditions for each type of prior association. For example, deductive prior associations require assessment of inferential validity and causal prior associations require considerations of possible “defeating conditions” (Bartha 2010, 103). Other constraints temper the motivations of the advocate and critic as well. For instance, the prior association attributed to the target domain can only be as strong as that which is attributed to the source; Bartha refers to this constraint as the upper bound thesis (Bartha 2010, 103).

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<sup>195</sup> Bartha's Articulation model is dynamic—as scientific inquiry changes over time, so does critique. It also connects it with Holyoak and Thagard's discussion on the logic of scientific discovery, blurring the distinction between discovery and justification found in the Received View. Thanks to Carole Lee for her comments in an earlier draft.

<sup>196</sup> This device also need not occur between individuals; as Bartha suggests, “you can be your own critic” (Bartha 2010, 5).

The second step in Bartha's development of the Articulation Model involves identifying those factors in the source and target domains that are relevant to the focus of the argument (Bartha 2010, 103-104). The interactions between advocate and critic highlight "two main problems" associated with considerations of relevance (Bartha 2010, 104). The first includes the critic's worry that "the positive analogy might be overstated" and the advocate's worry that "the negative analogy might be overstated" (Bartha 2010, 104). The second problem is comprised of the critic's worry that "the negative analogy will be understated" and the advocate's worry that "the positive analogy might be understated" (Bartha 2010, 104). Since the analysis of an analogical argument depends heavily upon a determination of features that are relevant to the conclusion, the argument must be stated accurately (Bartha 2010, 104). However, unlike Hesse's account, these claims of relevance do not depend upon "objective facts about relevance;" instead, the argument must genuinely "reflect both parties' knowledge of the two domains" (Bartha 2010, 104).<sup>197</sup>

The final step in Bartha's Articulation Model involves appraising the argument's potential for generalization, or whether a "common pattern" exists between the prior association of the source and the target (Bartha 2010, 105). In this step, two principles apply to the constituents of the positive and negative analogies. These principles—which Bartha refers to as the overlap principle and the no-critical-difference principle—stipulate that the source and target must share some positive factors and that a relevant negative factor does not exist (Bartha 2010, 101 & 105). There is no reason to accept a conclusion about the target in cases where the source and target do not share at least some similarities, or where the existence of a relevant difference defeats an analogical argument. If these two principles are satisfied, then the prior association can be generalized to the target, offering what Bartha

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<sup>197</sup> Note that there is, in the case of implicit relevance, a requirement of honesty posed to the advocate; the advocate must include all relevant factors in the argument (Bartha 2010, 104).

calls *prima facie* plausibility (Bartha 2010, 105).<sup>198</sup> Arguably, any account of analogical reasoning must explain how to determine whether “a given analogical argument establishes its conclusion as *prima facie* plausible” (Bartha 2010, 91).

Bartha frames the justification of an analogical argument in terms of plausibility, distinguishing his Articulation Model from the simplistic model assumed by the Received View and from Hesse’s more sophisticated account. Focusing on the prior association of an argument as the basis for classifying and assessing whether it can be generalized to the target, Bartha sidesteps the demand for a purely abstract account of relevance. Instead, the virtue of Bartha’s account is he makes the assessment of relevance dependent upon the generalizability of the prior association and context.

Bartha’s rhetorical device is intended to exploit the epistemic diversity of the community. If the community is not diverse, however, it will not have the resources to deal with the problem of ingrained analogy. Recall that the problem of ingrained analogy occurs when the analogies associated with metaphors are taken for granted and become self-reinforcing. On Bartha’s account, the plausibility of analogical arguments depends upon the background knowledge of the advocate and critic, to include the framework assumptions of metaphors and models used within the community. However, members of non-diverse and experientially homogenous communities will not be in the position to generate the robust critique needed for the problem of ingrained analogy. Advocates and critics, who share substantial experience and standpoints, may fail to recognize key points of disanalogy or to challenge the assumptions of the relevance of the similarities, and may even permit relevant differences to go unnoticed. Nowhere is this more apparent than in science that is guided by metaphor.

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<sup>198</sup> Bartha notes that there are two conceptions of plausibility. The first is the “probabilistic conception”—that is, there is a “degree of belief” in the truth of the argument’s conclusion and thus, in the truth of the hypothesis (Bartha 2010, 15). It is found in the works of Salmon (1990) and Polya (1954). The second conception is the one Bartha wishes to focus on: the modal conception (Bartha 2010, 19). To refer to an argument as *prima facie* plausible means that “provide[s] reasons to think the hypothesis might be true” (Bartha 2010, 18).

### §3.4 The Broad and Narrow Roles for Analogy

As we saw in the first chapter, the analogies at work in scientific practice play a number of different roles throughout the process. When first presenting his Articulation Model, Bartha recognizes two roles for analogy noting “it is important here to distinguish between the broad role of analogy, in providing an underlying metaphor for a sustained research program, and the narrow role of grounding an individual analogical argument” (Bartha 2010, 12).<sup>199</sup> Citing the research of Nancy Leys Stepan, Bartha grants that “metaphors can provide models and images that shape our perception of phenomena, and they can establish a grip that is difficult to shake” (Bartha 2010, 12-13). Stepan draws on the use of the *race-gender as distinct species* metaphor in 19<sup>th</sup> century science of human difference to highlight the “cultural sources of scientific analogies, their role in scientific reasoning, their normative consequences, and the process by which they change” (Stepan 1986, 262). The power of a metaphor to guide research and influence scientific practice arises from the interactions between the broad and narrow roles for analogy. As Bartha points out,

Many current models of analogical reasoning are based on the idea that good analogical reasoning aims at maximizing the overall degree of similarity between two systems. Such models have very few resources to halt the self-perpetuating tendency that Stepan is worried about, since that tendency is regarded as the essence of analogical reasoning.

(Bartha 2010, 12)

One of the strengths of Bartha’s Articulation Model is that it “does not generally take past successes of an analogy into account: each analogical argument is evaluated on its own merits”; “maximal overall similarity” does not provide justification for the argument (Bartha

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<sup>199</sup> In fairness, he intends to restrict his account to the narrow role for analogy; he undertakes how an argument may be plausible.

2010, 12). In addition, he holds that his rhetorical device makes the context of the analogical argument explicit as a mechanism for critical assessment. Even so, the steps for adjudicating the individual analogical arguments, as presented by Bartha, may not be in the position to mobilize effective critique, especially if the advocate and critic come from a homogeneous community.

When the analogies associated with a metaphor are taken for granted, most members of the community assume those analogies express 'facts' about the source or target, which results in the problem of ingrained analogy. For example, scientists may assume the analogies are supported by evidence, when they are actually supported by a priori theory/assumption. If everyone in a community shares these assumptions, then scientific communities do not have members capable of robust critique; Bartha's rhetorical device is rendered ineffectual. In such cases, both the advocate and critic may not have the necessary cognitive/conceptual/experiential resources to recognize when widely held assumptions are being taken for granted.

The problem of ingrained analogy occurs when the correspondences associated with a metaphor contain errors and the members of the community assume those correspondences are fact. While Bartha's Articulation Model does not rely on the past successes of the metaphor or the psychological drive for maximal similarity, it cannot address the filters through which the entire community views the source or target.<sup>200</sup> Members of a scientific community enact a myriad of decisions, such as pursuing a particular research program, designing specific experiments, adopting measurement standards, and interpreting data—all of which are informed by the framework assumptions of the guiding metaphor. Given the

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<sup>200</sup> For the time being, I am tabling the possibility that the critiques tendered by some of the community (e.g. those who are marginalized within the community) may fall on deaf ears. In such cases, the entire community need not be unaware; those who are in a position to fulfill the duties of Bartha's critic and advocate are prevented from doing so.

implicit influence of the metaphor, even the best-intentioned critic may not be critical enough, or in the right ways, to escape the influence of the metaphor.

The Articulation Model has many merits, but I suggest that it lacks the resources to address “the self-perpetuating tendency” motivated by an ingrained analogy (Bartha 2010, 11). While Bartha addresses the narrow role of analogy, his Articulation Model is limited when it comes to the evaluation of analogy’s broader role, especially in circumstances where a community lacks the necessary diversity to produce effective critics. In the next chapter, I return to my account of metaphor-driven science and show how the problem of ingrained analogy becomes self-reinforcing, which includes both the broad and the narrow roles for analogy (i.e. the narrow role, an analogical argument and the broad role, the scientific metaphor). Using examples found in 19<sup>th</sup> century race science and brain organization research, I describe how the self-reinforcing nature of an ingrained analogy plays out at the level of individual researcher and program. I contend that an account of analogy—if it is to reduce the likelihood of an ingrained analogy—must address both roles.

## Chapter Four

### Ingrained Analogy and Science

The problem of ingrained analogy is not strictly a problem associated with reasoning from individual arguments. It has a social dimension, which includes evaluating both the aptness of the metaphor (broad role) as well as assessing the plausibility of the analogical arguments (narrow role) generated by the metaphor. If a community is not in a position to provide a robust critique of the assumptions grounding its metaphors, then the interchanges between the broad and narrow roles create an environment for a metaphor to take hold and become self-reinforcing.

Historian, Nancy Leys Stepan, describes how the power of a metaphor-driven practice results in an ingrained analogy, drawing on examples from 19<sup>th</sup> century science on human difference. In this chapter, I present two cases that illustrate ingrained analogy affecting scientific practice at the level of the individual researcher and the scientific community. The first case is Stephen Jay Gould's analysis of Samuel George Morton's 19<sup>th</sup> century research on human intelligence. I suggest Morton relied on a metaphor that was consistent with his polygenist views and established a hierarchy of human intelligence based on the average cranial capacity of different races. The second case, taken from brain organization research (BOR), includes an analysis provided by Rebecca Jordan-Young (2010). This case is broader in scope than the first; instead of focusing solely on the work of an individual researcher, Jordan-Young's empirical study offers an insight into the community dynamics of BOR, particularly of the communal practices associated with knowledge production that contribute to an ingrained analogy's staying power. These cases serve not only to illustrate

how analogies can become ingrained in scientific practice guided by metaphor, but each also suggests possible avenues for reducing the occurrence of an ingrained analogy as well.

#### **§4.1 The Problem of Ingrained Analogy and Its Effects**

In “*Race and Gender: The Role of Analogy in Science*,” Stepan believes that her examples from 19th century research on race and human difference demonstrate a need for “a critical theory of metaphor in science” (Stepan 1986, 277). Such an account is necessary, according to Stepan, because scientific metaphors are extremely influential, even though their influence is largely unrecognized by the community.<sup>201</sup> The *race/gender as distinct species* metaphor used in 19th century science on human difference offers an example of a highly influential metaphor. During this time, Stepan notes “gender was found to be remarkably analogous to race, such that the scientist could use racial difference to explain gender difference, and vice versa” (Stepan 1986, 263). For example, in the 19th century “it was claimed that women’s low brain weights and deficient brain structures were analogous to those of lower races, and their inferior intellectualities explained on this basis” (Stepan 1986, 263). The linking of gender to race was based on both physical and psychological similarities, which ranged from having “a narrow, childlike, and delicate skull ... protruding ... apelike, jutting jaws of lower races” to being “innately impulsive, emotional, imitative rather than original, and incapable of the abstract reasoning found in white men” (Stepan 1986, 263).<sup>202</sup>

Interestingly, the analogy also ran in the reverse direction: non-white races were found to be like women. For example, scientist Carl Vogt states, “the mature male of many lower races resembled in his ‘pendulous’ belly a Caucasian woman who had had many children”

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<sup>201</sup> Stepan uses metaphor and analogy interchangeably in her paper. If Stepan, however, believes that metaphor operates in a manner similar to a Kuhnian paradigm, then analogy is best understood as the original comparison, which motivates the positive and negative analogies associated with a metaphor. Thus, the difference between metaphor and analogy should be preserved.

<sup>202</sup> Stepan cites the work of Haller and Haller (1974), Allan (1869), Cleland (1870), Ellis (1894), and Spencer (1875-6).

(Stepan 1986, 263).<sup>203</sup> At times, non-white races and white females were classified as distinct biological categories, or belonging to categories so distinct as to be incapable of normal reproduction with other races or gender. For example, females testing the boundaries of their cultural stereotype were described as being “in danger of degenerating into psychosexual hybrids” (Stepan 1986, 263-264).<sup>204</sup>

The illustrations of scientific texts of the 19th century also attested to the power of the metaphor, which supported a hierarchy of races. For example, Figures 3 and 4 represent the implicit beliefs of 19th century scientists, who distinguished populations of race, gender, and even a lack of intellect as separate species. These illustrations stressed the physical differences of traits, such as the shape of the jaw and size of the skull, in order to demonstrate that racial groups were separate species.

Figure 3, below, first appears in Nott and Gliddon (1868) and is later cited in Gould (1981) as being representative of personal biases at work in science.<sup>205</sup> For Gould, the illustrations in Figure X are an exaggeration of physical traits in order to support the assumed hierarchy of race. Gould states “The chimpanzee skull is falsely inflated, and the Negro jaw extended, to give the impression that blacks might even rank lower than apes” (Gould 1981, 32). Interestingly, Josiah Clark Nott, seems to anticipate Gould’s claim, stating

It will doubtless be objected by some that extreme examples are here selected; and this is candidly admitted: yet, each animal type has a centre around which it fluctuates—and such a head as the Greek is never seen on a Negro, nor such a head as that of the Negro on a Greek.

(Nott and Gliddon 1868, 460)

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<sup>203</sup> Originally described in Carl Vogt, *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, & Roberts, 1864), p. 81.

<sup>204</sup> This applied particularly to women who supported the suffrage movement. See Weir (1895); also cited in Stepan (1986) p. 264.

<sup>205</sup> Nott and Gliddon (1868) is a collection of papers that is dedicated to the work and memory of Samuel George Morton. It includes papers by Morton’s peers (Louis Aggasiz, William Usher, Henry S. Patterson, Josiah Clark Nott, and George R. Gliddon) as well as the unpublished papers of Morton.

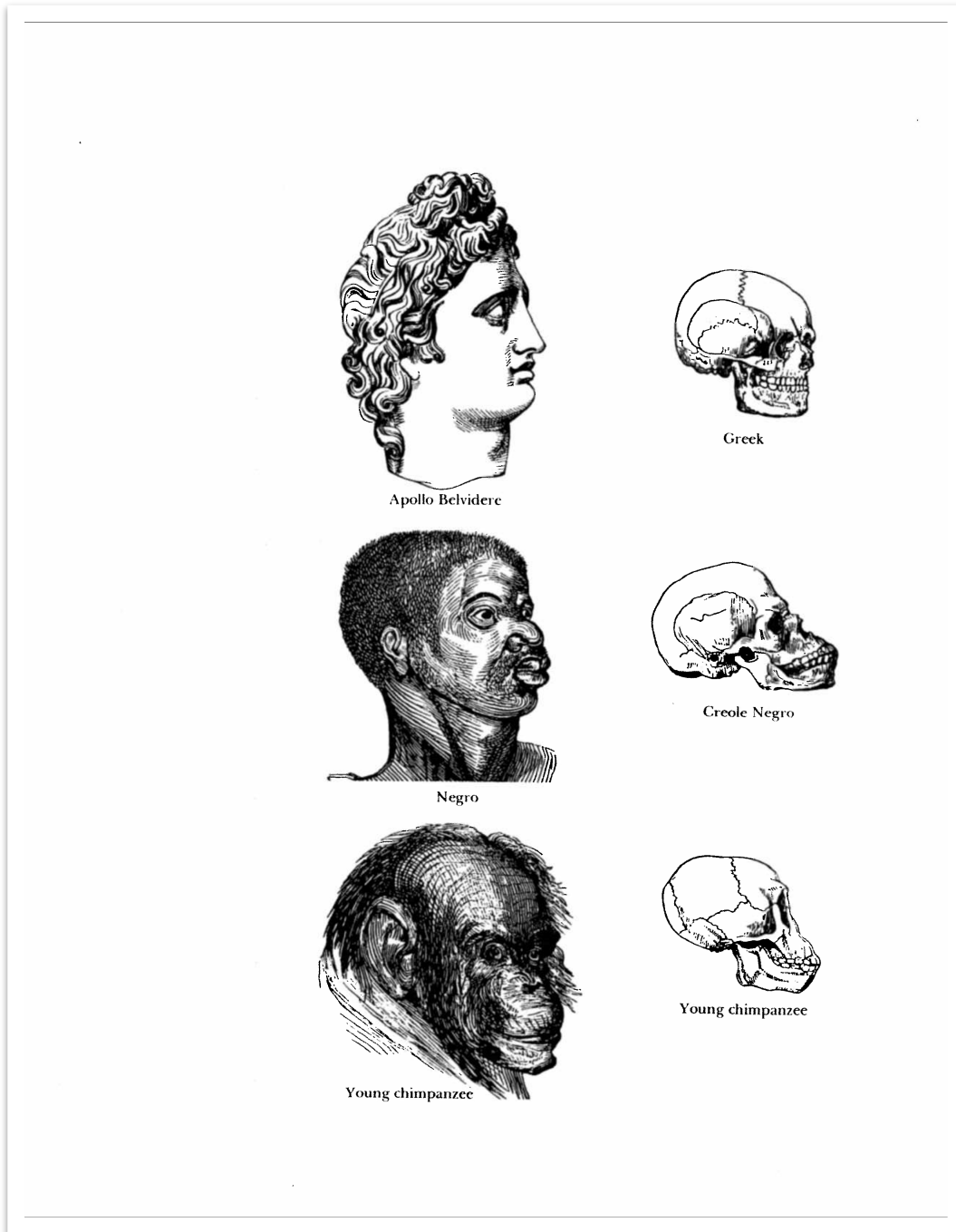


Figure 3. Nott's comparison of skulls and jaws in 1868.<sup>206</sup>

<sup>206</sup> See Nott and Gliddon (1868) p. 458 for original appearance.

In spite of Nott's response, Gould's critique of the illustrations is especially fitting given the later comments made by Nott in the same paper. Nott states,

A man must be blind not to be struck by similitudes between some of the lower races of mankind, viewed as connecting links in the animal kingdom; nor can it be rationally affirmed, that the Orang-Outan and Chimpanzee are more widely separated from certain African and Oceanic Negroes than are the latter from the Teutonic or Pelasgic types.

(Nott and Gliddon 1868, 457)<sup>207</sup>

According to Nott, people of non-white races more closely resemble primates than Caucasians. Interestingly, Nott never questions whether the sketches of the heads radically differ from the actual skulls, or whether the Grecian skull was sketched from looking at a statue.

The illustrations of skulls in Figure 4, below, serve a similar purpose as those in Figure 3. When comparing the sketches of the Negro, Idiot, and Chimpanzee skulls, Carl Vogt states "We need only place the skulls of the Negro, chimpanzee and idiot side by side, to show that the idiot holds in every respect an intermediate place between them" (Vogt 1864, 198). Arguing that the idiot is less human and more animal-like, Vogt also implicitly establishes that the Negro is closer in jaw shape and suture closures to an animal than the skull of a person from another race. He claims that the idiot is an intermediate, so they must be connected. In addition, by omitting the skulls of other races from the comparison, Vogt secures a link between Negroes and Chimpanzees.

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<sup>207</sup> Other illustrations include comparisons between Negroes, Orang-Outans, and Chimpanzees. The facial features in these illustrations reflect more prejudice than those supplied here, see (Nott and Gliddon 1868, 459). I have included this particular illustration in the appendix.

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Fig. 68. Negro skull, side view.

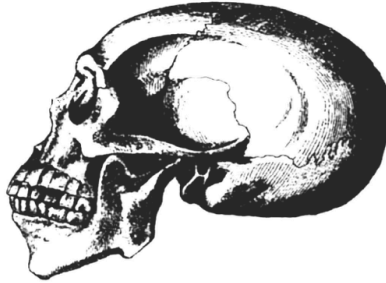


Fig. 69. Idiot skull, side view.



Fig. 72. Chimpanzee skull, side view.



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Figure 4. Vogt's comparison of skulls and jaw shapes of Negro, Idiot, and Chimpanzee.<sup>208</sup>

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<sup>208</sup> See Vogt (1864), p. 196-198 for discussion and figures.

The *race/gender-species* metaphor played an influential role in the modeling practice of 19<sup>th</sup> century science on human difference. While appealing to observations made by Stephen Toulmin, Stepan identifies the origin of an ingrained analogy:

The nature of the objects being studied (e.g., organic versus nonorganic), the social (e.g., class) structure of the scientific community studying them, and the history of the discipline or field concerned all play their part in the emergence of certain analogies rather than others and in their 'success' or failure.

(Stepan 1986, 265)<sup>209</sup>

As regards the larger community, Stepan is quick to point out “In the case of the scientific study of human difference, the analogies used by scientists in the late eighteenth century, when human variation began to be studied systematically, were products of long-standing, long-familiar, culturally endorsed metaphors” (Stepan 1986, 265). Thus, the impetus for research on human difference rests squarely in the norms and assumptions of the larger societies in which the scientist lived. Well-established prior to the science, “the metaphorical system provided the ‘lenses’ through which people experienced and ‘saw’ the differences between the classes, races, and sexes...” (Stepan 1986, 265).

When working from the *race/gender-species* metaphor, 19<sup>th</sup> century scientists on human difference encountered little resistance to their research because the analogies underwriting the metaphor were consistent with “cultural expectations” (Stepan 1986, 266). Stepan also notes the language of the analogies “became ‘naturalized’ in the language of science” and over time, the privileged members of the community became desensitized to the “metaphorical nature” of the comparison (Stepan 1986, 266). The analogies of the metaphor became firmly entrenched in the minds of the members of the community. No longer a source of novel comparison, the analogies associated with the metaphor were

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<sup>209</sup> Cited from Toulmin (1982), pp. 93-111.

consistent with the racial biases of the community, which ultimately resulted in the death of the metaphor.

Contrary to what one might think, calling a metaphor ‘dead’ does not describe its fall from popular use. A metaphor dies when its assumptions become so “deeply entrenched, hardly noticed, and thus, effortlessly used...in our thought” that we forget we are using it non-literally (Kovecses 2002, ix).<sup>210</sup> It does not matter how frequently the metaphor is used, but whether the metaphorical connections between source and target (the similarities and differences) escape notice and/or are taken for granted. While entrenched, the connections implied by the metaphor still play an active role in reasoning, which poses serious consequences for science.

“The foot of a mountain”, “the slow march of time”, and “the legs of a table” are dead metaphors; yet, these dead metaphors have less impact than those found in science. Consider the use of the *race/gender as a distinct species* metaphor used in 19th century science on human difference. The assumption of difference (especially when one considers separate species) not only motivated research questions, it also influenced measurement techniques and the design of experiments, and it even affected how data was ultimately interpreted. As a result of that particular metaphor, 19th century researchers assumed differences, both behavioral and physical, from the outset. Stepan notes that

Through an intertwined and overlapping series of analogies, involving often quite complex comparison, identifications, cross-references, and evoked associations, a variety of ‘differences’—physical, and psychical, class, and national—were brought together in a biosocial science of human variation. By analogy with the so-called lower races women, the sexually deviate, the criminal, the urban poor, and the insane were in one way or another constructed as biological ‘races apart’ whose differences from the white male, and likenesses to each other, ‘explained’ their different and lower position in the social hierarchy.

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<sup>210</sup> For Max Black, a dead metaphor escapes notice because it “no longer has a pregnant metaphorical use” (Black 1993, 25).

(Stepan 1986, 264)

The number of perceived differences and similarities between different races of people inspired a surprisingly diverse array of measurement practices. As Stepan describes,

It was measurements of the skull, brain weights, and brain convolutions that gave apparent precision to the analogies between anthropoid apes, lower races, women, criminal types, lower classes, and the child. It was race scientists who provided the new technologies of measurement—the calipers, cephalometers, craniometers, craniophores, craniostats, and parietal goniometers.

(Stepan 1986, 266)

These new measurement techniques were utilized in experiments, which ultimately supported the expectations of the scientists. In fact, as Stepan explains,

Because a metaphor or analogy does not directly present a pre-existing nature but instead helps ‘construct’ that nature, the metaphor generates data that conform to it, and accommodates data that are in apparent contradiction to it, so that nature is seen via the metaphor and the metaphor becomes part of the logic of science itself.

(Stepan 1986, 274)

In science, the two analogues of the *race-gender* metaphor interact—to use Max Black’s terminology—enabling scientists to use the properties of one analog to learn about the other and vice versa (Stepan 1986, 270).<sup>211</sup> Thus, women, who were analogous to lower races, were found to be similar in brain size, brain weight, in the shape and size of jaw, and in temperament (Stepan 1986, 270). In like manner, the assumptions that underpin the *race as distinct species* metaphor framed explanations for the inferiority of non-white races. Once scientists were able to confirm the connections via prediction and experiment, “the

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<sup>211</sup> See Black (1954).

similarities...were a reality of nature, somehow ‘in’ the individuals studied” (Stepan 1986, 267).

In *The Myth of Metaphor*, Colin Turbayne describes the switch from a metaphorical understanding of the world to a literal one. He writes

The history of science may be treated from the point of view that it records attempts to place metaphysical disguises upon the faces of process and procedure...to do this is not just to cross two different sorts; it is to confuse them. It is to mistake, for example, the theory for the fact, the procedure for the process, the myth for the history, the model for the thing, and the metaphor for the literal truth.

(Turbayne 1971, 4)

For Turbayne, this shift is dangerous because it is done unconsciously. When the metaphor becomes commonplace to the point that its use is no longer recognized as metaphorical, those acting under the influence of the metaphor are being misled, or in Turbayne’s terms “victimized” (Turbayne 1971, 5). As Stepan’s research suggests, scientists relying on a metaphor ‘saw’ the world in a particular way, which affected key features of scientific practice, such as methods for measurement, design of experiments, and the interpretation of data.

#### **§ 4.2 The Basic Structure of a Powerful Problem**

If we accept Stepan’s description of the pervasiveness of metaphor in scientific practice, then the propensity for the metaphor to mislead a community seems all but impossible to prevent. Without some form of intervention, something akin to a Kuhnian paradigm shift, a metaphor that is deeply entrenched will continue to exert its influence on the community undetected (Stepan 1986, 272).<sup>212</sup> This poses an epistemic danger for scientific communities; they mistake metaphorical speech for descriptions of reality and

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<sup>212</sup> To counter the influence of a dead metaphor, Turbayne suggests proposing a new metaphor (Turbayne 1971, 5). I will consider, and subsequently reject this strategy, in the next chapter.

develop their research programs under the influence of the metaphor.<sup>213</sup> In this section, I explain how an ingrained analogy takes hold and implicitly influences scientific practice at two levels: the individual scientist and the community.

Recall the broader, metaphorical conception of scientific practice I presented in Chapter One. The diagram, below, depicts the recursive exchange between a scientific metaphor and the arguments it generates. Supporting the metaphor are the positive, negative, and the neutral analogies between a source and a target. When relying on these analogies, scientists generate the first formulations of analogical arguments that will support either a particular hypothesis or a model. Working from the neutral analogy, scientists will develop expectations about the data from their experiments, and, as we will see in the case of Morton's research, they will often unconsciously rely on the content of the metaphor to interpret their data.<sup>214</sup> Any information gained as a result of these arguments will then be incorporated into the positive and/or negative analogies that underpin the metaphor. In this way, a new version of the metaphor will generate new arguments (and the information contained in the positive and negative analogies will be used as the premises for new arguments). Scientists will continue to exploit a particular metaphor until the community no longer considers it productive.

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<sup>213</sup> Interestingly, Stepan notes that Thomas Kuhn "paid little attention to the role of social, political, or economic factors in the generation of new metaphors" (Stepan 1986, 276).

<sup>214</sup> One feature of the neutral analogy under consideration is often the conclusion of an argument and is referred to as the hypothetical analogy, see Bartha (2010, 14).

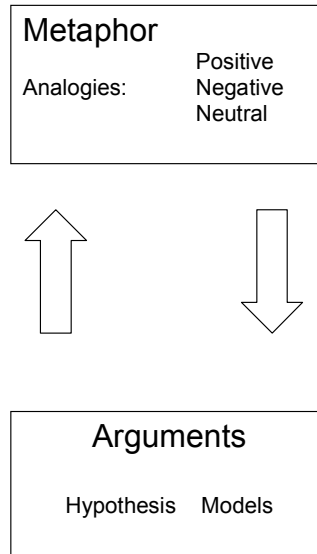


Diagram 3. The Metaphorical View of Science

The problem of ingrained analogy arises when the information in the positive and negative analogies of the conceptual metaphor are taken as fact and, in turn, figure as premises of subsequent arguments. This poses a serious epistemic threat when the assumptions of similarity and difference that comprise the premises is misleading.<sup>215</sup> For example, consider the following tabular representation of an analogical argument.

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<sup>215</sup> In such cases, the analogies are in the position to mislead scientists and scientific communities. However, in light of what Turbayne claims—confusing metaphorical speech with reality poses a metaphysical threat as well. I am inclined to save this problem for a later project.

Source	Target
P1	P1*
P2	P2*
P3	P3*
P4	P4*
Conclusion	Hypothetical Analogy

Argument 6. Tabular Representation of an Analogical Argument


When the analogies of a metaphor are taken literally, the similarities and differences between the analogues are believed to be facts and when scientists create arguments, those facts comprise the premises, or horizontal correspondences of the tabular representation. Suppose the second horizontal correspondence, that between P2 and P2\*, is the result of an ingrained analogy.<sup>216</sup> Whether that particular horizontal relation denotes a similarity or difference, it functions as a fact in the argument. This is problematic; not only is the correspondence misleading, but the scientist relied on this “fact” to design their experiment and interpret data. This, however, is not the only way that an ingrained analogy can mislead scientists.

As we saw from Bartha’s Articulation Model presented in Chapter Three, the relevance of the similarities and the differences in an individual argument is a function of the vertical relation in the tabular representation. If a horizontal correspondence posits incorrect

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<sup>216</sup> For this example, I am limiting the effect of the ingrained analogy to only one horizontal relation for simplicity’s sake. It is possible that misleading correspondences could appear in multiple horizontal lines on the tabular representation.

information, then that information influences the vertical relation of the argument. Since the vertical relation of the source provides the relevance relation for the argument, the entire argument is affected.



Source	Target
P1	P1*
P2	P2*
P3	P3*
P4	P4*
Conclusion	Hypothetical Analogy

#### Argument 7. Relevance and the Tabular Representation of an Analogical Argument

In summary, there are a few ways in which an ingrained analogy can mislead. First, scientists working within a framework of the metaphor may assume the similarities to be relevant when they are not, second, relevant differences between the analogues may proceed unrecognized, and finally, the members of the source domain may not be linked to the conclusion in the way the argument suggests.<sup>217</sup> Ensuring that the information in the horizontal correspondences accurately represents the world is critical since the direction of the prior association (the vertical relation) determines how different analogical arguments are classified (Bartha 2010, 96).<sup>218</sup> An ingrained analogy can therefore result in the misclassification of the analogical argument and thus, determinations of the plausibility of the argument will also be affected. The following sections illustrate how the problem of ingrained analogy influences actual scientific practice. Because both the practice of the

<sup>217</sup> The direction of the prior association is critical to Bartha's Articulation Model (Bartha 2010, 96). An ingrained analogy may result in the misclassification of the analogical argument and thus, the plausibility of the argument will also be affected.

<sup>218</sup> Recall that the prior association of an analogical can be predictive, explanatory, functional, or correlative (Bartha 2010, 96-97). Misinformation in the horizontal correspondences may also introduce error into the vertical relation, causing the advocate and critic to appeal to different standards of plausibility.

individual scientist and that of the community are susceptible to the problem of ingrained analogy, a theory of analogy should be able to address both.

#### **§ 4.3 19<sup>th</sup> Century Race Science: The Research of Samuel George Morton**

For the largely white and elite male scientific community of the 19<sup>th</sup> century, the inferiority of non-white races was readily observed; yet, it still required an explanation. Motivated by their interactions with populations long believed to be inferior, scientists of human variation pursued a research program designed to establish a link between physical difference and intellectual difference. One example of the risks posed by an ingrained analogy noted by Stepan is the research of Dr. Samuel George Morton. Described by Stephen Jay Gould as a prominent physician and “self-styled objective empiricist,” Morton is well known for his work in craniometry, the measuring of the bones of the skull (Gould 1978, 503).<sup>219</sup> Morton’s extensive collection of human skulls—which approached 1,000 specimens of various races at the time of his death—afforded him the opportunity to propose a link between cranial capacity and human intelligence (Gould 1978, 503). His research on the link between race and intelligence exemplifies how an ingrained analogy implicitly affects the research practice of an individual scientist, especially with respect to interpreting experimental results. In the next section, I will present the case of BOR, a case that exemplifies the risks of ingrained analogy at the level of community dynamics.

Morton’s reputation rested largely on his efforts to objectively measure the volume of approximately 600 human skulls; later compiling his research into tables like the one below.

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<sup>219</sup> Morton began his career as a physician and naturalist, but “his work with skulls gave [him] a special standing” (Fabian 2010, 10-13). According to the *History of Physical Anthropology Vol. 1* (1997), Morton’s account of polygenism was an integral part of the foundation of the American School of Anthropology (*History of Physical Anthropology*: 1, 65).

Table 5. Morton's final summary of cranial capacity by race (34).

Races and Families	N	Cranial capacity (in <sup>3</sup> )			
		Largest	Smallest	Mean	Mean
<i>Modern Caucasian Group</i>					
Teutonic Family					
Germans	18	114	70	90	} 92
English	5	105	91	96	
Anglo-Americans	7	97	82	90	
Pelasgic Family	10	94	75	84	
Celtic Family	6	97	78	87	
Indostanic Family	32	91	67	80	
Semitic Family	3	98	84	80	
Nilotic Family	17	96	66	80	
<i>Ancient Caucasian Group</i>					
Pelasgic Family	18	97	74	88	
Nilotic Family	55	96	68	80	
<i>Mongolian Group</i>					
Chinese Family	6	91	70	82	
<i>Malay Group</i>					
Malayan Family	20	97	68	86	} 85
Polynesian Family	3	84	82	83	
<i>American Group</i>					
Toltec Family					
Peruvians	155	101	58	75	} 79
Mexicans	22	92	67	79	
Barbarous Tribes	161	104	70	84	
<i>Negro Group</i>					
Native African Family	62	99	65	83	} 83
American-born Negroes	12	89	73	82	
Hottentot Family	3	83	68	75	
Australians	8	83	63	75	

Table 1. Morton's final summary presented in Gould (1978).<sup>220</sup>

Morton's table provided a comparison of the cranial capacities of different races and supported his conclusions in his three major works, *Crania Americana*, *Crania Aegyptiaca*, and a 1849 summary of his entire collection, titled "Observations on the size of the brain in various races and families of man." According to Gould, "these tables were frequently reprinted during the 19<sup>th</sup> century and became a linchpin in the anthropometric arguments about human racial differences" (Gould 1978, 504).<sup>221</sup>

<sup>220</sup> This table appears in Morton (1849), p. VIII.

<sup>221</sup> According to historian Ann Fabian, the price of Morton's book, *Crania Americana*, was \$20; a sum that prohibited most academics from purchasing the book. The book, however, did find an audience amongst those seeking to defend the institution of slavery (Fabian 2010, 81-83).

Working from Gould’s analysis, I propose that Morton relied a metaphor, which suggested human races were analogous to species, and the differences in intelligence amongst species could be extended to mean differences in intelligence amongst races.<sup>222</sup> The metaphor, which linked species with race, relied on the presumption of relevant similarities between races and species. In the rest of the section, I demonstrate that Morton’s research was influenced by this metaphor, which led to the errors presented by Gould.

Historically, biological classifications of humans, like the one below from Linnaeus (1806), provided physical descriptions of the members of each race.

Racial Subspecies	Phenotypic Traits	Associated Behaviors
<i>Europeus albus</i>	Fair-skinned, Light Colored Hair, Blue or Green eyes	Clever, Easily Governed, Flexible, Wears Form-Fitting Clothing
<i>Asiaticus luridus</i>	Yellow-skinned, Black Hair	Melancholy, Severe, Conceited, Wears Loose Clothing
<i>Americanus rubescus</i>	Red-skinned, Black Hair, No Facial Hair	Stubborn, Easy to Anger, Difficult to Civilize
<i>Africanus niger</i>	Dark-Skinned, Curly Hair, Dark Eyes	Cunning, Passive, Inattentive, Impulsive

Table 2. Linnaeus’ Proposed Racial Classification of *Homo sapiens*<sup>223</sup>

<sup>222</sup> Morton ascribed to a type of biblical polygenism, which maintained that the separate races were created by God in different geographical locations with each location suited to the race originating in it. Such an account explained the physical differences of race as well as the lack of time for Noah’s sons to repopulate the earth (Fabian 2010, 83).

<sup>223</sup> Linnaeus, Carolus. *Systema Naturae*. Trans. William Turton. 13<sup>th</sup> ed. London: Lackington, Allen, 1806. pp. 20-22.

Of course, physical traits were not the only observable difference. Linnaeus also included descriptions of behavioral traits associated with each race. For example, according to Linnaeus' classification, *Americanus rubescus* (the American Indian) was stubborn, easy to anger, and very difficult to civilize (Linnaeus 1806, 20-22). For Morton, the physical and behavioral distinctions delineating the different races were no different from the physical and behavioral differences delineating different species. This supported an initial formulation of a metaphor, *races as distinct species*, which motivated Morton's research on human skulls.<sup>224</sup>

In his analysis, Gould describes Morton's practice as a "dimly perceived finagling, doctoring, and massaging" of the data in the interest of truth, Gould introduces five/six types of errors appearing in Morton's research:

1. Favorable inconsistencies and shifting criteria,
2. Procedural omissions,

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<sup>220</sup> In his analysis of Morton's research, Gould poses a challenge to my claim that Morton relied on a *races as distinct species* metaphor: the fact that species was commonly defined by their inability to interbreed (Gould 1978, 503). During the 19<sup>th</sup> century, the definition of species included the inability of distinct species to interbreed (or if they did, the offspring were either sterile or not viable). If Morton accepted the fact that individuals from different races were able to produce viable offspring, then it would not seem rational for him to use a metaphor that contradicted this point. Morton, however, doubted this distinguishing feature of species.

Instead of asking whether the definition was a challenge to his polygenist assumptions, Morton objected to the traditional definition of species in two ways. First, he pointed to the reduced ability of mixed race slaves (Creole peoples) to give birth to viable offspring. According to Morton,

As respects crosses between Negroes, Indians, and white persons, on the Panama isthmus; a passage was indicated to me by Mr. Conrad: —"The character of the half-castes is, if possible, worse than the Negroes. These people have all the vices and none of the virtues of their parents. They are weak in body, and more liable to disease than either the whites or other races. It seems that as long as pure blood is added to the half-castes proper, when they only intermarry with their own colour, they have many children, but these do not live to grow up...

(Nott and Gliddon *et al.* 1854, 726 *fn* 548)

Morton points to additional support, noting,

Hombrohn and Jacquinot have asserted on their own authority, as well as upon that of others, that this law of infertility [among people of different race in the United States] holds also with the cross of the European on the Hottentot and Australian.

(Nott and Gliddon *et al.* 1854, 398)

Morton's second strategy was to suggest that in the case of crosses that did yield offspring, the offspring were hybrids; not unlike the mule or the hinny that results from horse/donkey crosses.<sup>224</sup> According to Morton, some crosses between humans of different races resulted in individuals that were "rare and deficient in fertility," which amounted to cases of hybridization among humans (Gould 1978, 503). Ultimately, Morton argued that the occurrence of hybrids is not detrimental to his theory, but rather provided a good reason for revising the traditional definition of species (Gould 1978, 503).

3. Convenient omissions,
4. Subjectivity directed toward prior prejudice,
5. Slips, and
6. Miscalculations (Gould 1978, 504).<sup>225</sup>

Throughout his analysis, Gould is careful to point out that these errors are not consciously done (Gould 1981, 54).<sup>226</sup> Instead, he notes, "...Morton's summaries are a patchwork of fudging and finagling in the clear interest of a priori convictions" (Gould 1981, 54). Gould attributes Morton's research practice to a "prior prejudice," one strong enough to affect the "measuring of bones and toting of sums," yet not consciously recognized (Gould 1981, 56). I suggest that the "prior prejudice" cited by Gould results from the implicit influence of the metaphor, *race as distinct species*.

Evidence attesting to the influence of the metaphor on Morton's practice appears in Gould's first category, "favorable inconsistencies and shifting criteria," which includes Morton's failure to distinguish between male and female skulls when taking measurements of samples from each race (Gould 1978, 508). This error affected his data because Morton failed to take into account the difference in stature between the sexes (Gould 1978, 505). Large individuals are more likely to have larger skulls, and males in general are larger than females. Some of the non-Caucasian sub-samples were comprised only of female skulls, which provided a lower than average measurement for that group (Gould 1978, 505). Another point of contention was how Morton created his categories. Morton grouped the North American tribes (who happened to have a higher average cranial capacity than the Teutons and Anglo Saxons) with South American tribes. The South American tribes had significantly smaller skulls than the North American tribes and the skulls from South

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<sup>225</sup> The list of Morton's errors differs in Gould (1981) from Gould (1978). Miscalculations and convenient omissions are combined in Gould (1981) and a new error, "Subjectivity directed toward prior prejudice," is introduced (Gould 1981, 68).

<sup>226</sup> Gould states that he "find[s] no evidence of conscious fraud", and that Morton was transparent with his methods, making detection of fraud quite easy (Gould 1978, 54).

America comprised a larger portion of the sample (Gould 1978, 505-6).<sup>227</sup> Because he relied on a metaphor that stressed difference by race, Morton had an expectation that the skulls of non-Caucasian groups would be smaller overall than the Caucasian group. As a result of this expectation, his particular grouping ignored differences of stature and lowered the overall average for American Indians.

Interestingly, assumptions about the average skull size of particular groups also appear to favor the measurements of the Caucasian sample. Morton intentionally removed smaller Hindu skulls from the Caucasian sample because this group was known to have smaller than average skulls (Gould 1978, 508). Morton's decision to omit some measurements from the average of Caucasian skulls and his inclusion of smaller, female South American skulls in the Indian sample illustrates how a dominant metaphor can unconsciously influence research. I suggest the *race as distinct species* metaphor motivated an expectation of difference and Morton, in keeping with the expectation, assumed an average size for Caucasian skulls (one that would be larger than skulls of other races).

In the category described as "procedural omissions," Gould draws attention to the fact that Morton failed to establish a link between cranial capacity and intelligence (Gould 1978, 508). Gould notes that Morton "never considered alternate hypotheses, though his own data almost cried out for a different interpretation" (Gould 1981, 68). As we saw in the case of errors arising from the selective recording and inclusion of data, Morton did not consider differences in cranial capacity due to sex or stature. Without an account linking the physical measurements to intelligence, the features attributed to the source domain may not be relevant to the conclusion. Morton failed to provide a reason for thinking that the size of bones relates in any way to intelligence. As such, the argument remains a simple

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<sup>227</sup> It is unclear if Morton grouped the North American and South American skulls together because he believed them to be related. This seems doubtful since the difference in stature and other traits might have supported a contrary position given his polygenist beliefs. Even so, Morton's allowing a majority of female skulls to be included in the sample, when those were known to be smaller, is inconsistent with the methods used to calculate the average for the Caucasian group.

enumeration of perceived similarities and provides no support for why those similarities are linked to the conclusion in some way.

So long as Morton's measurements supported the presumption of difference suggested by the metaphor, he was unable to recognize evidence that would counter the assumptions of the metaphor and thus, was convinced of his success. By assuming a link between cranial size and intelligence, Morton could include samples of small stature (Incan skulls in the Indian sample), exclude small stature skulls from the Caucasian sample (omitting the data from Hindu skulls), and rely on the comparison of three female Hottentot skulls with an all male Caucasian sample to establish white intelligence (Gould 1981, 68).<sup>228</sup> In the early 19<sup>th</sup> century, differences in intelligence were apparent, and thus assumed, in science. Accounts from researchers abroad corroborated these beliefs further entrenching these biases within the scientific community. Conducting his research under the influence of the metaphor, Morton was not able to entertain an alternate explanation for the differences in cranial capacity, nor did he question his assumptions linking cranial capacity with intelligence.

"Convenient omissions," Gould's third category, includes skulls that Morton intentionally left out of the group calculations.<sup>229</sup> Although intact and able to provide a measurement, Morton believed these skulls were too large to be representative of its racial category and so he decided not include them in his calculations. For example, "Morton excluded a large Chinese skull and an Eskimo sub-sample in the 1849 tabulation of Mongolian capacity, thus reducing the grand mean below the Caucasian average" (Gould

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<sup>228</sup> Because Morton worked with a metaphor that linked race with species, he was unable to consider the difference gender might make in relation to skull size. He expected members of non-Caucasian races to have a lower average and expected members of the Caucasian group to have the highest average. Thus, it was easy to use smaller skulls in one case and removing them in the other without considering the affect of gender.

<sup>229</sup> Although this category is similar to Gould's first category, "favorable inconsistencies and shifting criteria," the difference is that in this category Morton arbitrarily selects which skulls are included in a given category and in the first, Morton chooses which sub-groups are included in the larger category. Admittedly, these two errors are quite similar.

1978, 509).<sup>230</sup> In each instance, Gould notes that the omission works “in Morton’s favor” (Gould 1981, 69). Bearing in mind the transparency with which Morton reported his calculations, it is not plausible that he deliberately intended to mislead the community; Gould points out, Morton’s tables figured prominently in his published works. Instead, a better explanation is that the metaphor prompted certain expectations, and Morton relied on those expectations when determining which skulls were included in a sample.

Gould’s fourth category of error, “Subjectivity directed toward prior prejudice,” demonstrates how the metaphor influenced Morton in spite of his attempts to use an objective method for measurement. As noted earlier, Morton gained a reputation of introducing objectivity into the science of human difference (Gould 1981, 51-52). After all, measurements of the cranial capacity of skulls appear on the surface to be objectively quantifiable. Initially, Morton’s methods included using the same seed to measure cranial capacity, measuring the volume of the seed by the gradations of an aluminum tube, and taking multiple measures.<sup>231</sup> In fact, Morton was so conscientious about having objective measurements he discontinued the use of seed, opting instead for steel shot. Morton also decided that he should be the only one to perform the measurements after he discovered previous measurements taken by an assistant were imprecise.<sup>232</sup> Morton’s reputation was bolstered by the changes made to his methodology. The differences between the measures using seed and shot, however, revealed another bias: there were “degrees of discrepancy” matching Morton’s “*a priori* assumptions” (Gould 1981, 68). When compared with the measurements attained by using shot, the use of seed (which varied depending on how much packing was done) had the largest discrepancy for blacks, a smaller discrepancy for Indians, and the smallest for whites (Gould 1981, 68). When using seed, not only did the biases of the

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<sup>230</sup> In his final summary of 1849, Morton neglects to include the measurements of three Eskimo skulls even though those measurements appeared in his earlier work, *Crania Americana* (Gould 1978, 508).

<sup>231</sup> Morton describes his methods in great detail in his book, *Crania Americana*, see pages 249-268.

<sup>232</sup> Morton reports these changes in the *Proceedings of the Academy of Natural Sciences of Philadelphia*, Vol. 1, See Morton (1841), pp. 6-8.

researcher enter into the measurements, the biases were consistent with his theory. Unfortunately, although Morton intended the use of steel shot to be an objective measure, the proportional differences found when using seed remained unchanged. Gould notes “blacks fared poorest and whites best when the results could be biased toward an expected result” (Gould 1981, 68). Although Morton sought an objective way to measure cranial capacity, he reproduced, in empirical terms, the expectations of the metaphor. The metaphor assumed differences in behavior and size between species and this was extended to the separate races.

I consider Gould’s last two categories, “slips” and “miscalculations,” together. According to Gould, these two categories include the errors of recording, calculation, and failure to proof read one’s work. During his calculations, Morton “rounded the Negroid Egyptian average down to 79, rather than up to 80” (Gould 1981, 69). Morton also rounded up the averages of German and Anglo-Saxon averages to 90 when those averages were 88 and 89 respectively (Gould 1981, 69). Gould also notes Morton “reported his Indian mean incorrectly, as falling between black and whites, rather than at par with blacks (Gould 1978, 509). Morton also described two black skulls as being the smallest in his collection when “several Inca crania were smaller by his own tabulated measure” (Gould 1978, 509). In short, the errors themselves did not raise any red flags because the mistakes were consistent with the metaphor (Gould 1978, 508). Simply put, Morton failed to recognize errors in his calculations so long as the measurements corresponded with his expectations.

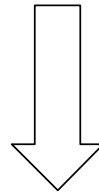
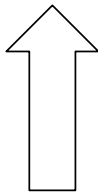
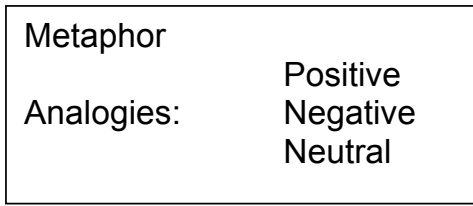
Throughout his research on human skulls, Morton’s errors indicate his reliance on certain assumptions, which were implicit and emphasized differences between human races that were similar to the differences attributed to species. The following diagram illustrates how the assumptions—the differences between races that are believed to be real—directed

Morton's practice, especially with respect to how he designed his experiment and collected data.

The assumptions of the metaphor comprise the premises of a tabular representation and included Morton's reasons for omitting the measurements some from the samples or for including the measurements of other skulls in a sample. These assumptions were taken as fact in the individual argument, which posited a conclusion supporting the difference in intelligence amongst members of different races given the average cranial capacity.<sup>233</sup> Believing that the differences in people of different races approximate the differences between members of different species, Morton conducted his measurements and collected his data through the filter of the metaphor. Although he was committed to the ideal of objectivity, he failed to realize it because he remained unaware of the powerful influence of his metaphor.

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<sup>233</sup> It is not clear whether Morton believes that the average measurement for a particular group would approximate an average measure of intelligence, or how intelligence and measurement relate.



Source: Species	Target: Races
P1 Hybrids/Inability to Breed	P1* Hybrids/Inability to Breed
P2 Observable differences in behavior (possible hierarchy)	P2* Observable difference in behavior (possible hierarchy)
.	.
.	.
Pn	Pn*
Conclusion (Difference in brain size amongst species.)	Hypothetical Analogy (Difference in brain size amongst races.)

Diagram 4. Morton's Argument Affected by Ingrained Analogy

Not only was Morton not aware of the influence of the metaphor on his practice, his errors reinforced its biases. The tables of data collected by Morton supported a metaphor that inspired research in a wide range of disciplines. Within the discipline of human difference research, the metaphor motivated the design of new experiments, new terminology, as well as the technology needed to measure the perceived differences between race/gender and the socially dominant, white male.<sup>234</sup> In the new arguments generated by the metaphor, similarities residing in the metaphor were no longer considered metaphorical

<sup>234</sup> Stepan notes the various disciplines that arose, such as craniometry and phrenology, and the new techniques/equipment used for measurement (Stepan 1986, 269-271).

or non-literal. Instead, this information influenced the horizontal relations of the analogical mapping and the vertical relation of relevance. At times, the entire metaphor was incorporated within the individual arguments. As a result, scientists saw similarity and difference where there was none, “and their interpretations of human difference and similarity were widely accepted, partly because of their fundamental congruence with cultural expectations” (Stepan 1986, 266).

Firmly entrenched within the empirical results of individual research programs, ingrained analogies are difficult to dislodge. This is largely due to two reasons: first, the analogies were taken for granted within the community and second, the self-reinforcing process that occurs between the metaphor and the individual arguments. By leaving out certain results or anomalous cases, Morton, who was a member of the human difference community, filled in the positive and negative analogies of the metaphor with misinformation. This resulted in a modified metaphor that informed subsequent research. The power of the metaphor prevented scientists from recognizing, collecting, or even entertaining evidence that did not fit their preconceptions. The community did not question the plausibility of the metaphor, nor was Morton’s research immediately brought into question. Unfortunately, the effects of the ingrained analogy that informed 19<sup>th</sup> century research on human difference were not limited to its epistemic consequences. As Stepan points out, “The analogies were used by scientists to justify resistance to efforts at social change on the part of women and ‘lower races,’[...]” (Stepan 1986, 275). In addition, to the detriment of women and minorities, the influence of the metaphor dominated both popular and scientific thinking for well over a century, which poses an interesting question: how to end the cycle of an ingrained analogy? (Stepan 1986, 275).

#### § 4.4 Brain Organization Research: Science and the “Male” or “Female” Brain

While the research of Samuel George Morton exemplifies how the problem of ingrained analogy works at the level of individual scientific practice, recent analyses of brain organization research (BOR) illustrate the effects of an ingrained analogy on scientific practice at the level of the community. Rebecca Jordan-Young’s (2010) analysis of BOR reveals how some communal practices contribute to the self-reinforcing nature of an ingrained analogy by encouraging scientists to maximize the similarities between the analogues of a model when researching sexed behavior in humans as a result of the influence of prenatal hormones on the brain.<sup>235</sup> Using Jordan-Young’s different analyses of BOR, I have identified errors at the level of the community that closely parallel the errors identified by Gould in his analysis of Morton’s research. For example, Jordan-Young’s meta-analysis of BOR literature reveal the community’s unconscious downplaying of differences (what she refers to as “bullet-proofing the theory”) between past and current models of BOR (Jordan-Young 2010, 141). In addition, Jordan-Young notes the community’s failure to consider contexts outside the dominant metaphor as alternative explanations for reported differences in human sexual behavior (Jordan-Young 2010, 143 & 267).<sup>236</sup> In this section, I introduce BOR, and argue that the two practices identified by Jordan-Young—the downplaying of differences between models and the failure to consider context as an explanation—result in an environment conducive to the problem of ingrained analogy. Further, I suggest that as a result of these practices, the BOR community was not in a position to cultivate members capable of the type of critique needed to address the problem of ingrained analogy.

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<sup>235</sup> Jordan-Young is careful to point out that other critics have identified “methodological weaknesses that call the results of particular studies into question (Longino-Doell 1983; Schmidt and Clement 1990; Byne and Parsons 1993; Fausto-Sterling 1985 and 2000)” (Jordan-Young 2010, 2). She distinguishes her research from these individual accounts by describing her work as a “rigorous and comprehensive *synthesis* of the studies” analogous to a meta-analysis, if BOR was less “diverse” (Jordan-Young 2010, 2-3).

<sup>236</sup> Jordan-Young points out that the sex hormones, which are the focus of BOR, are a “philosophically invested way to conceptualize substances” given androgens means “that which creates men” and estrogens mean “that which induces estrus” (Jordan-Young 2010, 16). In short, the focus of BOR is difference.

BOR posits the differentiation of human brains, by gender or sexual orientation, given a brain's exposure to prenatal hormones. For advocates of BOR, "prenatal hormone exposures cause sexual differentiation of the brain—that is, early hormones create permanent masculine or feminine patterns of desire, personality, temperament, and cognition" (Jordan-Young 2010, xi).<sup>237</sup> As a result of the influence of prenatal hormones, brain organization researchers infer differences between male and female brains, which often include 'gay' and 'straight' brains as well.

Like the assumptions of the metaphor that informed 19<sup>th</sup> century race science, the BOR metaphor—*prenatal sex hormones as an organizer of gender*—presupposes the concepts of masculinity and femininity, which are comfortable and familiar to the perceived social experiences of larger society. For example, in *Delusions of Gender* Cordelia Fine (2010) cites anecdotal evidence that illustrates people who claim to 'witness' gender difference. Fine confides, "When I tell parents that I'm writing a book about gender, the most common response I get is an anecdote about how they tried gender-neutral parenting, and it simply didn't work" (Fine 2010, 189). According to Fine, most parents who attempt to raise their baby in a gender neutral way immediately resort to biological explanations once they observe their child engaging in stereotypical, gendered behavior (Fine 2010, 189-191). In a similar vein, Jordan-Young also describes the anecdotal evidence given by parents who make comparisons between their sons and daughters. Jordan-Young recounts her mother's observation that such claims often come from parents of small families, and "with just a couple of children, gender looms large—it's the most obvious explanation for every difference you see between them" (Jordan-Young 2010, xiii). Although notions of what counts as masculine or feminine behavior have changed over time, these explanations are comfortable and pervasive in larger society.

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<sup>237</sup> Prenatal exposure can also prompt "behavioral dispositions" as a person ages (Jordan-Young 2010, xi). This means that some behaviors may not be observed until a person is an adult.

Claims about the innate differences between men and women appear in the news, often preceded by headlines asking why women are not equally represented in certain professions or academic fields. When asked this type of question, former Harvard President Larry Summers cited the “innate differences” between men and women as a possible reason for why women were absent from the “upper echelons” of academia, especially in the science, technology, engineering, and mathematics (STEM) fields (Hemel 2005). In relatively recent *New York Times* article, “Where the Women Are: Biology” (Nov. 4 2011), economist Paula E. Stephens claims that the increasing numbers of women in biology (as opposed to other sciences) occurs because “women historically have been interested in subjects that were less math intensive and that had the goals of helping people” (Drew 2011, ED21).<sup>238</sup> Other explanations for the lack of female representation in certain professions, like philosophy, sometimes include the appeal that women tend to avoid the “aggressive style” often associated with the discipline.<sup>239</sup> Unfortunately, these assumptions are not limited solely to the popular culture.

These bio-essentialist claims found in popular science texts and articles in the news claim legitimacy through an appeal to brain organization science. BOR spans approximately 48 years, and includes the use of “sophisticated” technology (Jordan-Young 2010, 2-6).<sup>240</sup> Originating in 1959,

[brain organization theory] rests on a very simple idea: the brain is a sort of accessory reproductive organ. Males and females don’t just need different genitals in order to have sex, or different gonads that make the eggs and sperm necessary for conception. Males and females also need different brains so they are predisposed to complementary sexual desires and behaviors that lead to reproduction.

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<sup>238</sup> As an economist, Stephan notes that women going into the life science limit their chances to earn decent salaries (Drew 2011, ED21). It is worth noting that Stephan frames her position as one of personal choice, which omits any external contexts that might direct females into those particular jobs.

<sup>239</sup> Philosopher Linda Martín Alcoff considers, and handily challenges this explanation, in her *New York Times* “Opinionator” piece Alcoff (2013).

<sup>240</sup> In fact, Jordan-Young conducts her analysis on three hundred plus “studies conducted from the late 1960’s through 2008” (Jordan-Young 2010, 2).

(Jordan-Young 2010, 21)

Using Jordan-Young's analysis of BOR, I have identified methodological errors made by the community, which could result in the problem of an ingrained analogy. What about the BOR metaphor makes it likely for its assumptions to become ingrained? A fundamental assumption of the BOR metaphor, *prenatal sex hormones as an organizer of gender*, is the belief that differences in human behavior occur along gendered lines. Not only does the influence of prenatal hormones "organize" the brain, which results in distinct physical and behavioral traits, but the hormones themselves are also characterized as masculine and feminine.<sup>241</sup>

According to Jordan-Young, the researchers of brain organization theory treat masculinity and femininity as "stable" concepts that transcend time and culture (Jordan-Young 2010, 109). In fact, Jordan-Young notes of her interviews with the actual researchers, "nearly all scientists conducting brain organization research treat masculinity and femininity as commonsense ideas that don't require explicit definitions" (Jordan-Young 2010, 109).

Jordan-Young writes,

from the late 1960s until around 1980, brain organization researchers relied on a model of human sexuality that sharply divided masculine and feminine sexual natures. Their model exhaustively divided aspects of sexual desire and expression into male and female 'forms': initiating versus receptive, versatile versus conservative, genitally focused versus diffuse.

(Jordan-Young 2010, 112)<sup>242</sup>

Early BOR defined feminine sexuality as "romantic, dependent, receptive, slow to waken, and only weakly physical" (Jordan-Young 2010, 113). In the case of women, sex was

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<sup>241</sup> See *fn.* 29.

<sup>242</sup> In fact, Jordan-Young divides the studies of BOR into "two chronological periods when one or the other model of sexuality was dominant amongst scientists" (Jordan-Young 2010, 114). BOR in the 1960's to the 1980's used a model in which a bi-polar model, which had male and female sexuality on opposite ends of a spectrum. Behavior was more/less male or female (androgyny was in the middle).

a “means for fulfilling desires for love and motherhood,” and not as its own end (Jordan-Young 2010, 113). This particular conception of female stands in stark contrast to its male counterpart, which depicts male sexuality as “active and energetic, initiating, dominant, penetrating, frequent, intense, and genitally focused” (Jordan-Young 2010, 113). According to BOR researchers, men considered sex as “its own end, unsentimental and undiluted by romance” (Jordan-Young 2010, 113).<sup>243</sup>

The primary characteristic distinguishing between feminine and masculine types of behavior in BOR is the sexual desire for the opposite sex. Often taken for granted by researchers, Jordan-Young notes the definitions of male and female sexuality are not explicitly stated in the research (Jordan-Young 2010, 114). In her analysis, she gleans some operational definitions by examining the existing “research reports and the materials used in conducting these studies—especially the actual questions asked and the rules for evaluating answers” (Jordan-Young 2010, 114). For example, initial studies in 1968 on women with congenital adrenal hyperplasia (CAH) measured the subject’s “eroticism and sexual practices” (Jordan-Young 2010, 115).<sup>244</sup> Individuals with CAH experience an “overproduction of androgens, including testosterone and dihydrotestosterone” (Jordan-Young 2010, 114). Consistent with the expectations of researchers, women in this particular study exhibited “heavy virilization, sometimes uncorrected genital morphology, and [a] lack of feminine secondary sexual development” (Jordan-Young 2010, 115). Although the authors of the study admit that the subjects were not ideal because the influence of androgens was not limited solely to the “prenatal period,” this study is often cited (even today) as “evidence that prenatal hormones shape adult sexuality” (Jordan-Young 2010, 115).

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<sup>243</sup> Jordan-Young also notes “certain activities, especially masturbation and having multiple partners, or sex outside of marriage, were unequivocally coded as masculine” (Jordan-Young 2010, 113).

<sup>244</sup> The subjects of this study “had come to age prior to the introduction of cortisone therapy,” which used in infancy “suppress[ed] the overproduction of androgens” (Jordan-Young 2010, 115).

The definitions are from the outset epistemically problematic. Not only are the prenatal hormones assumed to be gendered and linked with gendered behavior (which is defined by a particular culture and time), the definitions of masculine and feminine sexuality have shifted and this shift was not acknowledged by the BOR community. For example, in the 1968 paper, researchers described the arousal of the late treated women with CAH in masculine terms, noting the subjects were “not sentimental...which leads to a romantic longing for the loved one alone and which will, in his absence, require waiting for his return” (Ehrhardt, Evers, and Money 1968, 120). Instead of being passive and patiently waiting for one’s partner, subjects with CAH adopt the masculine behavior of masturbation (Jordan-Young 2010, 116). Ehrhardt *et al.* attributed other masculine traits to the subjects, such as versatility in sexual positions, freedom with respect to initiating sex, and an “erotic response to perceptual material” (Ehrhardt, Evers, and Money 1968, 121). Interestingly, researchers noted that the behaviors of women with CAH were not strictly masculine because the subjects also exhibited the female trait of having a “steady” sexual partner, and not multiple ones (Jordan-Young 2010, 117). Yet, this difference is not explored further.

Having stable concepts, especially those playing a key role in the assumptions of the metaphor, is critical given the particular experimental methodology used by BOR. As we shall see shortly, the individual experiments of BOR rely on each other for support, which requires the terms used throughout different studies to be consistent. The stability of masculinity and femininity in BOR, however, is arguable, especially when confronted with “a cursory familiarity with the history of sexuality—or recent social history more generally” in the United States.<sup>245</sup> According to Jordan-Young, the “sexual revolution, second-wave

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<sup>245</sup> Jordan-Young limits her discussion primarily to Western Europe and the United States and so, the challenges that she considers will be more of time than that of culture. Of course, BOR scientists did not need to be aware of contemporary social policies. Jordan-Young points to research by Laqueur (1992) and van der Meer (1997) who note that European women during the Renaissance were believed to lack control with respect to acting on sexual desires and men who were concerned with fashion and appearance were not thought effeminate, but a “womanizer” (Jordan-Young 2010, 110).

feminism, and the gay and lesbian liberation movement” challenge the stability of these fundamental concepts; yet, the evolution of masculinity and femininity are not attended to in the research. Since the inception of BOR, what was initially thought to be feminine (often understood as behaviors associated with female sexuality) has undergone key changes, developing from a narrow description of feminine behavior to one that includes behaviors previously believed to be masculine (Jordan-Young 2010, 110).<sup>246</sup> The introduction of the birth control pill and the changing societal attitudes regarding female sexuality affected the classification of certain sexual behaviors (like masturbation, genital arousal, and sex with multiple partners), which were no longer thought of as strictly masculine, but became an acknowledged part of female sexuality (Jordan-Young 2010, 114).

The assumptions about what count as strictly feminine and masculine sexual behavior implicitly guide scientific practice in BOR, and researchers have unwittingly relied on outdated assumptions in spite of the changes that have occurred within larger society as a result of the sexual revolution. For example, Ehrhardt *et al.* (1968) was submitted to “the *Johns Hopkins Medical Journal* in December 1967, a few months after the ‘summer of love’ launched the sexual revolution into mainstream consciousness” (Jordan-Young 2010, 117).<sup>247</sup> Yet, the antiquated assumptions of difference associated with the organizing metaphor of BOR permeated research and proceeded unnoticed not only in the research of the day, but more recent studies also continued to cite Ehrhardt *et al.* (1968) at least until the 1980’s (Jordan-Young 2010, 118).<sup>248</sup>

After the 1980’s, BOR researchers adopted a more sophisticated conception of gender, switching from a one-dimensional “Bi-Polar Model of gender” to what is called the

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<sup>246</sup> Jordan-Young observes, “interestingly, no aspects that were originally considered feminine ever became part of the ‘normal’ masculine repertoire” (Jordan-Young 2010, 110).

<sup>247</sup> The paper was also written post- Masters and Johnson (1966), “which gave wide publicity to their findings that the physiology of arousal and orgasm is extremely similar in men and women” (Jordan-Young 2010, 117).

<sup>248</sup> Jordan-Young cites nineteen studies that relied on Ehrhardt *et al.* (Jordan-Young 2010, 118).

“Orthogonal Model of gender” (Jordan-Young 2010, 119-132). Previously, the Bi-Polar Model depicted gendered behavior as poles at the opposite ends of a continuum.

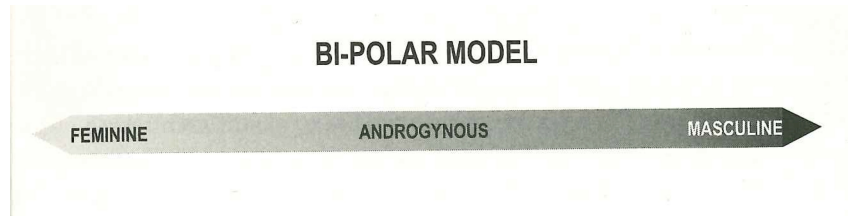


Diagram 5. The Bi-Polar Model of gender (Jordan-Young 2010, 119)

According to this model, “femininity can be demonstrated by the presence of certain traits specifically defined as ‘feminine,’ but also by the *absence* of traits defined as ‘masculine’” (Jordan-Young 2010, 119).<sup>249</sup> Studies on sexual behavior that rely on this model contain a “master element that conveys all (or at least the most important) information about masculine and feminine sexuality”: that women desire men and men desire women (Jordan-Young 2010, 119).

After 1980, however, characteristics previously believed to be indicative of masculine sexual behavior, like “high libido, multiple partners, and more varied and frequent sexual activities,” were also included in the domain of feminine sexual behavior (Jordan-Young 2010, 132). Animal studies, which demonstrated that “normal male and female animals regularly display both so-called male and female sexual behavior, but in different proportions,” challenged the limitations of the previous, one-dimensional model (Jordan-Young 2010, 132).

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<sup>249</sup> Jordan-Young also notes that this model fails to capture someone who is “either unfeminine *and* masculine” as well as someone who is “*both* feminine and masculine” (Jordan-Young 2010, 119).

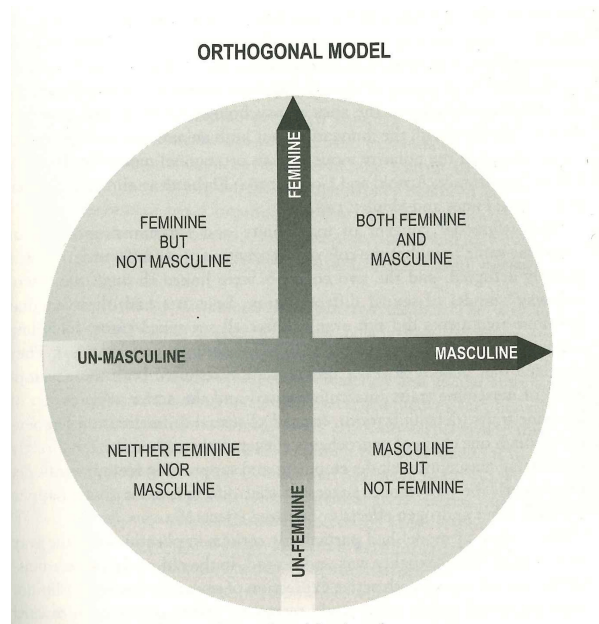


Diagram 6. The Orthogonal Model of gender (Jordan-Young 2010, 132-134)

The orthogonal model, however, accommodated this new information; while the concepts of masculine and feminine remain distinct, “the properties of masculinity and femininity may be combined or absent in any individual” (Jordan-young 2010, 133). According to Jordan-Young, “the change of model had particularly serious implications for the way in which feminine sexuality was understood” (Jordan-Young 201, 134). Instead of being a domain that was essentially the absence of masculine behaviors, female sexuality became “a domain *with content*—not an absence of traits that men had, but an expression of traits that were typical of ‘healthy’ and ‘normal’ women” (Jordan-Young 2010, 134).

Normally, a conceptual switch from one model to another is explicitly recognized as a refinement in the theory, and the switch from the Bi-Polar Model to the Orthogonal Model in BOR—although a long time in coming—appears to be a refinement of theory. Yet, brain organization researchers in general do not note the switch from one model to the other in the literature, which fails to acknowledge any reference to disparate concepts associated with the models. For example, a study conducted under the framework of the later model

may refer to 'feminine' behavior and mean something quite different from the same term used in a study that relies on the earlier model.

Jordan-Young describes the community's omission of model development from the literature as "bullet-proofing the theory" (Jordan-Young 2010, 141). The community's lack of transparency with respect to the switch from one model to the other in BOR poses epistemic problems. Jordan-Young notes "newer and more complex conceptualizations of feminine sexuality may be an improvement in abstract theory, but they make it difficult to grasp the meaning of research underlying brain organization theory" (Jordan-Young 2010, 141). Depending on the model used, terms like feminine may mean different things, and there is often no indication of intended meaning in the literature (Jordan-Young 2010, 141).<sup>250</sup> Failing to recognize the differences between the two models prevents direct comparison of BOR studies and hides whether "the theory is supported, on the whole, by the evidence (Jordan-Young 2010, 141). For example, Money and Matthews (1982) relied on a broader interpretation of feminine sexual behavior, "one that included sexual arousal" (Jordan-Young 2010, 135). Yet, earlier studies associated with John Money employ terms underwritten by the one-dimensional model, which characterizes feminine behavior as passive.<sup>251</sup> The women in the 1982 study were found to exhibit typical feminine sexual behavior, but this differs radically from the feminine sexual behavior assumed by the earlier studies.

After Money and Matthews (1982) appeared, sexual arousal was no longer included as an indicator for distinctly masculine or feminine sexual behavior. As Jordan-Young describes, "scientists *no longer referred to the previous findings* of 'masculinized' or 'feminized' arousal patterns...even in review or background sections" (Jordan-Young 2010, 136). This is puzzling given that there was no explicit rejection or explanation for the switch.

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<sup>250</sup> At times, some of the studies of BOR "directly contradict on another" (Jordan-Young 2010, 141).

<sup>251</sup> These studies include Money (1965), Ehrhardt *et al.* (1968), Masica *et al.* (1971), and Money and Daléry (1976).

Yet, as Jordan-Young suggests, dropping the previous use of arousal “signals a change in scientific thinking,” but it is a change that comes at a cost (Jordan-young 2010, 136). Of course, no longer using the categories associated with the earlier model made sense, inasmuch as they did not accurately depict sexual behaviors, but silently dropping the categories from use without explanation masks the problems associated with the previous model, giving the impression that the theory is supported by more evidence than it really is.

Meyer-Bahlburg *et al.* (1984) offer another example of research categories that were lost in the course of switching from one model to the other. This particular study, and all subsequent ones, hides a shift in the use of descriptions of sexual development, such as delays/“absence of *precocity*” (Jordan-Young 2010, 136).<sup>252</sup> According to the earlier model, attaining sexual milestones early was part of masculine sexual behavior (Jordan-Young 2010, 136). Yet, as the studies continued and the community switched from the one-dimensional model to the orthogonal model, some categories connected with gender were dropped, which exaggerates the support for the current model.

Jordan-Young notes “over time, as sexual-revolution ideas about women’s sexuality became increasingly mainstream, scientist’s definitions changed without their even realizing it” (Jordan-Young 2010, 142). In fact, the “level of departure from earlier definitions was dangerously close to making brain organization literally unfalsifiable” (Jordan-Young 2010, 142). Further, “because scientists have not noticed the changes and confronted them directly, they have both reinforced the notion that ‘masculine’ and ‘feminine’ sexuality are universal, timeless constructs and created an illusion of a seamless line of evidence supporting human sexuality as hardwired by hormones” (Jordan-Young 2010, 142). The unconscious masking of difference of the concepts assumed by all models is a key mechanism through which an analogy comes to be ingrained.

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<sup>252</sup> These terms refer to sexual milestones, such as having a boyfriend/girlfriend, dating, and the “initiation of sexual behavior” (Jordan-Young 2010, 136).

Jordan-Young notes that the extent to which the concepts found in some studies/papers remain in the community's consciousness of BOR while others disappear is telling (Jordan-Young 2010, 143). For example, Jordan-Young describes a study, Kester *et al.* (1980), involving men exposed to masculinizing hormones. According to this study, men who were exposed to Diethylstilbestrol (DES) actually "reported *earlier* initiation of sexual activities" (Jordan-Young 2010, 138). Although this particular study was consistent with the one-dimensional model's expectations, being a contemporary study, it should not have included sexual milestones. This study is continually cited as support in contemporary literature reviews even though milestones have been dropped from the research (Jordan-Young 2010, 138).

Using older studies to support the new model poses epistemic problems. First, it avoids a direct comparison of models and hides the shift of framework concepts connected with the introduction of the new, orthogonal model (Jordan-Young 2010, 143). Citing older studies, while at the same time appealing to the new model, amounts to "selective reference and reinterpretation of previous research" (Jordan-Young 2010, 143). Unless one conducts an analysis on the scale of Jordan-Young's, the differences between the two models proceed undetected. The practice "makes brain organization research appear seamless, coherent, and ever-advancing—rather than characterized by many dead ends, reevaluations, and continuing contradictions" (Jordan-Young 2010, 143). On the surface, BOR is the result of consensus and the categories/definitions used appear stable (Jordan-Young 2010, 143).

The lack of transparency in BOR literature, given the switch from the one-dimensional model of gender to the orthogonal model, is prevalent. Jordan-Young states "brain organization researchers almost never address the fundamental questions involved" (Jordan-Young 2010, 148). For example, a majority of studies focusing on sexual orientation "either have not defined sexual orientation at all, or have used vague and contradictory

definitions that often do not agree with the measures scientists have used” (Jordan-Young 2010, 146).<sup>253</sup> Even when scientists offer a definition, it is done in manner that downplays any possible criticism. According to Jordan-Young, this manner of defining terms gives the “impression that measures are mundane and predictable rather than plucked from a wide range of alternatives” (Jordan-Young 2010, 146). By treating the measures and definitions as commonplace or stable, BOR scientists block potential critique, making it easier for the assumptions of the metaphor to become ingrained.<sup>254</sup>

Given that the studies of BOR rely on each other in order to support a given theory, the influence of an ingrained analogy poses a serious epistemic threat. As we saw in the earlier discussion of Stepan, the assumptions of similarity that underpin a scientific metaphor induce agreement amongst members of the community and suggest support of a theory where there is none. Unfortunately, the ingrained analogy of gendered difference (and the psychological drive to maximize similarity between the analogues of the model) is also preserved through the literature trends of BOR.

Notably, all the studies supporting BOR are “special” in the sense that “each study of how prenatal hormones sexually ‘organize’ the human brain is a *quasi experiment*” (Jordan-Young 2010, 3). Because of the limits associated with using human subjects, BOR “scientists must piece together evidence from animal studies, and from individual human quasi experiments that are by definition partial and uncontrolled” (Jordan-Young 2010, 3). The reliance on quasi experiments has important consequences for generating robust critique within the BOR community. Given that the “interpretation of every quasi experiment depends on carefully placing that study within the overall body of evidence,”

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<sup>253</sup> Jordan-Young notes that of the studies which link prenatal hormone exposure to sexual behavior (studies from 1967-1995 appearing in journals with peer-review) “roughly 51 percent focused primarily on sexual orientation” (Jordan-Young 2010, 318 *fn* 2). Of these studies, one third provided an explicit definition, approximately one third indicated an operational definition (given the measures used in the study), and the remaining third offered no definition at all. For further information, she refers the reader to Jordan-Young 2000, Chapter Seven.

<sup>254</sup> Jordan-Young refers to the “‘quiet’ disagreements” among BOR scientists as “profound” (Jordan-Young 2010, 146).

evaluation/analysis of BOR theories rely on “how well the studies fit together” (Jordan-Young 2010, 3). Fit, of course, implies a homogeneity/commensurability of terms, categories, and assessments amongst the studies. If the community masks or glosses over the differences between distinct studies, it not only hinders the community’s ability to challenge the assumptions of the metaphor, *sex hormones as an organizer of gender*, it also creates an environment that is conducive to the problem of ingrained analogy.

The epistemic strength of BOR studies is enhanced as a result of how they support each other (Jordan-Young 2010, 45). “If you can’t actually do an experiment to see if X causes Y, then you consider all the quasi experiments about the effect of X on Y” (Jordan-Young 2010, 45). If the categories or measures are not commensurable, i.e. the definition for femininity is different between models, then the studies do not support each other in the way the community believes. Without sufficient support, the knowledge claims made by the community may seem sensible, but they lack credibility. The differences between the research guided by BOR’s earlier model and the research conducted using the newer one are not apparent in the literature. Thus, the assumptions of the older model can become entrenched in the community, affecting the design of future research and the collection of data.<sup>255</sup>

The downplaying of difference between the two models in the BOR literature is not the only area that provides a foothold for the problem of ingrained analogy. A second area, described by Jordan-Young, is the failure of the BOR community to consider the “important elements of the social and biological context of development” (Jordan-Young 2010, 237).

According to Jordan-Young, BOR scientists focus exclusively on prenatal hormones “without

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<sup>255</sup> When the community downplays any differences between the models, it is difficult to challenge the assumptions associated with dominant metaphor. For example in BOR, terms such as ‘masculine’ and ‘feminine’ changed when the community switched from the one-dimensional model to the orthogonal one. The community, however, was not made aware of the shift in meaning because older studies were used to support newer ones, which gave the appearance of stable concepts. When concepts are believed by the community to be stable, the community is less likely to challenge them, even when those terms are unclear or questionable. Throughout the use of both models, ‘Masculine’ and ‘Feminine’ have never been carefully articulated, and instead have been left to interpretation in the BOR literature.

seriously entertaining other explanations that are at least as plausible” (Jordan-Young 2010, 238). The failure of the BOR community to entertain competing explanations prevents any serious critique of the dominant metaphor.

Generally speaking, one aim of scientific methodology is to design experiments that “neutralize the expectations of scientific observers” (Jordan-Young 2010, 238). As we saw in the discussion of Morton, the expectations of a researcher may lead to errors when collecting and reporting the data. When scientists study the effects of a particular treatment/surgery on animal subjects, they typically set up a control group (Jordan-Young 2010, 238). Jordan-Young notes that such experiments are designed to “obscure which animals had the intervention so that when they are monitored for subsequent behavior or physical changes, the observers won’t unconsciously see what they expect” (Jordan-Young 2010, 238). In addition, the randomization of “experimental and control groups” serves to constrain “other relevant factors,” ensuring that those factors are distributed throughout both groups and are not subject to “what the scientist already knows or believes about what causes the outcome” (Jordan-Young 2010, 239).<sup>256</sup>

Because BOR involves the use of human subjects, most if not all of BOR experiments are quasi experiments instead of randomly controlled trials, and as we shall see, quasi experiments are particularly vulnerable to the introduction of a research expectations. Brain organization researchers investigating the sexual behavior of girls with CAH cannot use a control group that would be subjected to treatment similar to that experienced by the experimental group (Jordan-Young 2010, 239).<sup>257</sup> The ethical implications of using human

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<sup>256</sup> Of course, the larger the number of the participants is better in such cases.

<sup>257</sup> Jordan-Young notes the treatment for girls with CAH often includes “medical monitoring [...] medical exams every three months or so, including close genital inspection and testing for size and flexibility; repeated questioning about gender identity and behavior (such as whether the child feels like a boy or a girls and why, and what sorts of toys they enjoy); query of the parents regarding the child’s masculinity or femininity; informing parents and the girls themselves that they may or may not be fertile, but that they can certainly adopt children; and so on” (Jordan-Young 2010, 239). It would not be ethical for girls without CAH to undergo similar treatments.

subjects are reason to avoid such direct, and invasive, experimentation.<sup>258</sup> Instead, the studies on girls/women with CAH are often compared to healthy individuals (sometimes a sibling) or perhaps in comparison with other groups, who have undergone similar (but not identical) surgeries/treatment (Jordan-Young 2010, 240-255). Jordan-Young notes, however, that the trauma of the treatment—which is a compelling reason for not using a standard experimental design—could provide an alternative explanation (context) for some of the behaviors under investigation. Unfortunately, the BOR community does not entertain these alternative explanations because they exist outside of the dominant metaphor (Jordan-Young 2010, 239-240).

BOR's reliance on quasi-experiments creates an environment suited for the problem of ingrained analogy. This is not to say that we should not rely on this particular methodology, but researchers must realize that quasi experiments may not be able to neutralize the expectations of the dominant metaphor. Quasi experiments do not give researchers the ability to control for all relevant causal factors and when this form of experimentation is combined with the psychological drive to maximize similarity, the community fails to consider other, possible explanations of the behaviors under investigation. In BOR, researchers expect women with CAH (the presence of a prenatal androgen) to exhibit more masculine sexual behavior, such as being less likely to maintain long, stable relationships; exhibiting a preference for lesbianism as opposed to penile-vaginal intercourse; and having a less conservative attitude toward masturbation and sexual positions (Jordan-Young 2010, 240-244). In other words, BOR experiments (which are designed in light of the expectations motivated by the metaphor) focus on these areas of sexual behavior, giving weight to them

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<sup>258</sup> Jordan-Young notes that subjecting healthy girls to such treatments (even when using placebos and pseudo surgeries) imposes “negative effects on the girls involved, as well as their parents” (Jordan-Young 2010, 239). For example, there might be psychological harm and a sense of violation with respect to the invasive examinations (Jordan-Young 2010, 239). Such treatments “might affect [participants’] relationships with their bodies, the medical system, their peers, and even their parents” (Jordan-Young 2010, 239).

as explanations while neglecting the possible impact of the social and biological context in which the women with CAH are situated.<sup>259</sup>

Because the community is so influenced by its metaphor, the medical context of being a woman with CAH, in particular, is completely overlooked by BOR studies. Researchers do not consider whether the corrective surgeries performed on women with CAH (and the effects of those surgeries) might affect their sexual behavior. Jordan-Young notes that the majority of girls and women with CAH have undergone at least one, and in most cases several, surgeries performed to correct the physical appearance and functioning of their genitals.<sup>260</sup> The effects of such surgeries are “nerve damage” and “impaired clitoral sensation or no sensation at all” (Jordan-Young 2010, 241). Women with CAH often “have a vaginal opening that is not adequately large or flexible to permit heterosexual intercourse,” and thus, experience “pain during vaginal penetration” (Jordan-Young 2010, 241). While BOR researchers expect the sexual behavior of women with CAH to approximate that of the “bisexual or lesbian,” it is reasonable to question whether the resulting surgeries and treatment play a role in influencing the subject’s sexual behavior. Pain and the inability to experience pleasure in the expected ways could very well motivate non-standard sexual behavior.

The BOR reliance on quasi-experimentation also poses problems when the terms/categories used by the researchers are not commensurable with those understood by the subjects. Some CAH studies rely on the reports of the subjects, which in itself is not a bad thing. However, if the researchers and subjects do not share similar definitions with respect to the categories delimiting sexual behavior, then the subjects’ responses do not adequately

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<sup>259</sup> Recall that one of the assumptions of the metaphor is that the brain is “organized” as male or female given the influence of sex hormones. Thus, researchers design experiments as a result of this assumption.

<sup>260</sup> Jordan-Young describes these surgeries as both “normalizing” and done “to correct problems from earlier surgeries including stenosis, fistulas, and incontinence” (Jordan-Young 2010, 240). Some surgeries are either a total removal of the clitoris (clitoridectomy) or a “reduction of the clitoris that involves removal of erectile tissue” (Jordan-Young 2010, 240).

answer the questions researchers pose. For example, masturbation means something different for women with CAH than it does for the researchers. In a study comparing women with diabetes and women with CAH, the “women with CAH ‘commonly spoke about masturbation as a medical procedure rather than primarily as a sexual activity’” (Jordan-Young 2010, 243).<sup>261</sup> Women with CAH “associated masturbation with [the] dilation required to keep the vagina open, or as a way to understand the medical interventions they had received” (Jordan-Young 2010, 243). In addition, the experiences of girls with CAH (whose medical examinations occur regularly about every 3-4 months) differ greatly from those of a typical girl who may not see the gynecologist, or have an exam of one’s genitals, until much later in life and with less frequency. Under constant scrutiny, women with CAH do not consider their genitals as a source of sexual release/gratification, but instead think in terms of manipulations needed in order to preserve the ability to undergo penile-vaginal penetration (Jordan-Young 2010, 243). As Jordan-Young notes, “women with CAH may more often think of masturbation as vaginal penetration, perhaps because this has been so explicitly prioritized in the way they have been medially ‘prepared’ for sexual interaction” (Jordan-Young 2010, 243). Given the radically different experiences of women with CAH, it comes as no surprise that there is a divide between what researchers mean by masturbation and what the subjects understand masturbation to be.

The corrective surgeries performed on women with CAH as well as the BOR clinicians’ goal to preserve ‘normal, female’ sexual function for women with CAH attests to the power of the BOR metaphor and its ability to hinder the community’s consideration of alternative explanations. Katrina Karkazis (2008) charges BOR clinicians with being “so deeply invested in the idea that genitals can and should be ‘fixed’ that they fail to consider how their treatment does, in spite of their good intentions, harm their patients” (Jordan-Young 2010,

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<sup>261</sup> See May *et al.* (1996), page 484.

244). The need to normalize the appearance is not merely a cultural assumption, but it is also endorsed by a metaphor that posits gendered difference. The genitals of women with CAH do not resemble 'normal' female genitalia. Under the bi-directional model, "not female" is the equivalent of male. Even when appealing to the newer, orthogonal model, women with CAH fare no better: the sexual behaviors of women with CAH are not interpreted as female.

For brain organization researchers, women with CAH are considered as something other than female, and thus, these women are expected to undergo treatment to be 'normal.' Unfortunately, the community has not recognized the severity of its treatments, nor has it considered the extent to which such experiences may influence the subject's sexual behavior (Jordan-Young 2010, 245). So complete is the neglect of context, Jordan-Young notes

it is possible to read the brain organization studies of CAH without ever encountering the sobering data on the overwhelming pain and discomfort during sex; the absence of clitoral sensation; and the feelings of shame, bodily inadequacy, and unattractiveness that are common among women with this condition.

(Jordan-Young 2010, 246)

The neglect of considering alternative explanations—even ones caused by the BOR community!—attests to the power of the BOR metaphor and the community's implicit desire to maximize overall similarity across the research.<sup>262</sup> The gendered assumptions of the metaphor frame researchers' expectations with respect to the anatomy and behavior of women with CAH, and it limits what explanations researchers entertain.

Although it is not reasonable to expect scientists to examine all variables involved in BOR, Jordan-Young notes "it is not acceptable in science to ignore variables that we already know are relevant, especially if these variables have a good chance of affecting the outcome as much as or more than the variables in which we are interested" (Jordan-Young 2010,

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<sup>262</sup> In the future, I want to explore whether quasi experiments are more susceptible to the problem of ingrained analogy given that strength of a theory is due to support from different experiments.

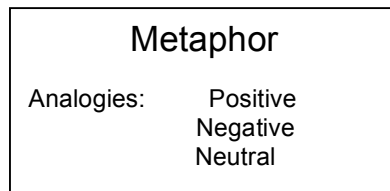
246). This is particularly critical for a discipline that relies on quasi experiments, where randomization of participants (and the variables) is not possible and the studies depend upon one another for support (Jordan-Young 2010, 246).<sup>263</sup> Brain organization researchers conduct experiments through the lens of gender while at the same time attempting to maximize similarity by designing studies intended to provide overall epistemic support.

The analyses of BOR found in Jordan-Young (2010) differ from the research conducted by Morton because it serves to demonstrate how the analogies of a metaphor become self-reinforcing within normal community practice. The BOR community's desire to satisfy the assumptions of its metaphor influences, or filters, the researchers' expectations. The overall practice of the BOR community becomes one of maximizing the similarity between the source and target, which prevents a robust critique of the assumptions linked with the metaphor. Diagram 7, below, depicts the problem of ingrained analogy at the level of community.

In the diagram, the analogical arguments guide the design of experiments conducted by individual scientists/research groups in the BOR community. The scientists not only rely on these arguments and the body of research to design their experiments. Not only do the assumptions of the metaphor implicitly influence the way that researchers interpret the resulting data, but there is a drive for all research to be consistent with each other. Thus, the data is collected not only through the lens of the metaphor, but also with the intent to maximize epistemic support. The data is then incorporated into the appropriate analogies associated with the metaphor, and the process continues.

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<sup>263</sup> At this point, I have only scratched the surface with respect to BOR. I have relied primarily on CAH studies and even then, I did not discuss the details of parental reporting and the priming of expectations, decisions based on what the subject and/or family knew about the fertility of the subject, gender schemas in our culture, etc. I hope, however, that what I have covered is enough to argue for communal practices that permit, or even encourage, the problem of ingrained analogy.



Source	Target	Source	Target	Source	Target
P1	P1*	P1	P1*	P1	P1*
P2	P2*	P2	P2*	P2	P2*
P3	P3*	P3	P3*	P3	P3*
P4	P4*	P4	P4*	P4	P4*
Conclusion	Hypothetical Analogy	Conclusion	Hypothetical Analogy	Conclusion	Hypothetical Analogy

Source	Target	Source	Target	Source	Target	Source	Target
P1	P1*	P1	P1*	P1	P1*	P1	P1*
P2	P2*	P2	P2*	P2	P2*	P2	P2*
P3	P3*	P3	P3*	P3	P3*	P3	P3*
P4	P4*	P4	P4*	P4	P4*	P4	P4*
Conclusion	Hypothetical Analogy	Conclusion	Hypothetical Analogy	Conclusion	Hypothetical Analogy	Conclusion	Hypothetical Analogy

Diagram 7. Scientific Practice Guided by Metaphor (Community)

As we have seen, one problem with the explanations of sexual behavior in BOR is that they preclude the consideration of alternative and highly contextual explanations; the research on girls/women with CAH is only one example. Other errors at the level of community include the lack of transparency in the literature with respect to the switch in models and the reliance on putatively stable concepts. Also troubling is the reliance on quasi experiments. Although this type of experiment is not problematic of itself, the BOR community does not exercise care in its use. For one thing, they should be aware of the tendency to design experiments with the goal of supporting previous research, and they should ensure terms used in these studies are in fact stable. Members of the BOR community

should also take care to ensure a consideration of alternative explanations for sexual behavior.

BOR explanations have become commonplace in the popular science literature. With titles like *The Female Brain (or The Male Brain)* and *Teaching the Female Brain: How Girls Learn Math and Science*, popular science books legitimize the perceived differences by appealing to the epistemic authority of science.<sup>264</sup> Not unlike the case of 19<sup>th</sup> century science on human difference, the BOR metaphor not only poses epistemic risks, but the explanations based on those metaphors also pose social and political risks as well. If science determines that professional preferences are based on gender, then there is little reason for the government to ensure that professional opportunities are available for females, and even less reason to question whether features of society in general play a role in restricting the availability of opportunities. Some practices of the BOR community—notably the lack of transparency given changes in models and the methods of quasi experiments, contribute to the likelihood of the problem of ingrained analogy. A robust critique of the assumptions of gender underwriting the BOR metaphor never materialized as a result of these practices. Morton's research and BOR demonstrate that the influence of the metaphor not only occurs at the level of the individual scientist, but at the level of community as well. As such, a normative account of analogy should have the resources to deal with the nuances of both cases.

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<sup>264</sup> Placement also plays a part of the legitimization of BOR. Copies of *The Female Brain* and *The Male Brain* were on the shelves at the Seattle Science Center gift shop.

## Chapter Five

### Transformative Criticism and Scientific Practice:

### Strategies for Developing a Robust Social Critique of Metaphor

The cases presented in the previous chapter—Morton’s 19<sup>th</sup> century research on human intelligence and contemporary brain organization research (BOR)—demonstrate that scientists and the communities in which they are situated desperately need to develop strategies for challenging the implicit influence of their metaphors. While previous accounts of analogical reasoning, like those presented by Mary Hesse and Paul Bartha, have focused primarily on the structure and credibility of individual arguments from analogy, I suggest that controlling the implicit influence of a metaphor that guides scientific research also requires an analysis of community dynamics/resources.<sup>265</sup>

Understanding a metaphor requires having access to the conceptual and hermeneutic resources made available by the language in which the metaphor developed. This includes having a working knowledge of the “system of associated commonplaces” of the source and target (Black 1962, 40).<sup>266</sup> While scientists introduce metaphors derived from the larger social community in which the scientist resides, they intend their metaphors to be “intelligible” and “understood” in particular ways (Hesse 1966, 164-165). Recall that scientific metaphors differ from literary ones because scientific metaphors are constrained

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<sup>265</sup> Bartha (2010) offers an elegant treatment for determining the plausibility of analogical arguments and my work will not be a reiteration of what is achieved there. Instead, I intend to build upon his work by extending the analysis to the larger context of practice, although some overlap will naturally occur.

<sup>266</sup> For Black, it is not necessary to understand the precise definition of the source/target. Instead, to successfully understand a metaphor, one need only be familiar with the way the particular words are used in one’s society (Black 1962, 40). Black believes that the common usage of the source and target must be “readily and freely evoked” (Black 1962, 40). Thus, a metaphor in one community will not necessarily make sense in another community (Black 1962, 40).

by the aims of the discipline and the features of the phenomena under investigation.<sup>267</sup> Thus, understanding the associations emphasized by a scientific metaphor requires grasping the context in which the community uses its metaphor. Lynn Hankinson-Nelson notes,

Scientific communities serve as obvious examples of epistemological communities, with bodies of theory, accepted procedures, questions, and projects defining such communities; and membership being a function of education in and allegiance to community-specific knowledge, standards, and practices.

(Hankinson Nelson 1993, 334-335)

The aims of the community, its research projects, and the phenomena not only constrain the metaphor, but they also serve an additional function: defining communal boundaries. Depending on the project, what comprises a particular “scientific community” will include groupings of different sizes, such as small laboratory groups, larger sub-fields, and/or an entire discipline.<sup>268</sup> Thus, an account intended to cultivate a robust critique of scientific metaphor should be applicable to communities of various sizes.<sup>269</sup>

The strategies that I propose in this chapter are predicated on a conclusion drawn in the previous chapter: an uncritical attitude toward the use of metaphor allows for the possibility of an ingrained analogy. The practices (and even the social make up) of scientific communities can hinder the performance of Bartha’s rhetorical device. Recall that the problem of ingrained analogy arises when the assumptions that ground a scientific metaphor become entrenched (or are understood as literal statements) within the community. These entrenched assumptions comprise the premises of the arguments generated by the metaphor and implicitly influence subsequent scientific practices—like the design of experiments and/or the interpretation of data. In the case of ingrained analogies, the results

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<sup>267</sup> What determines the source analog is defined by the community through its research projects and the particular features of the phenomenon that interest the community. Literary metaphors are not constrained. Novel associations are valued over what might be thought as epistemically promising associations.

<sup>268</sup> For an introduction to the origins of ‘scientific community’, see Jacobs (2002).

<sup>269</sup> The constraints demarcate scientific communities from non-scientific ones, but in cases of interdisciplinary sciences, different metaphors can flag the different communities working on similar problems.

of the arguments/experiments are often incorporated back into the positive and negative analogies of the metaphor to be used to direct future research. In this way, a metaphor tends to be self-reinforcing, and its entrenched assumptions difficult to detect by the community. When the assumptions associated with the metaphor are consistent with the expectations of larger society and the scientific community lacks the epistemic resources for recognizing and critiquing those assumptions, the problem of ingrained analogy is likely to occur.

As we saw in the previous chapter, the metaphors—*race as species* and *prenatal hormones as brain organizer*—embody an understanding consistent with communal expectations, which threatens the effectiveness of Bartha’s advocate-critic device. As members of a larger society and the scientific community, the advocate and critic may both share the assumptions contained in the analogies of the metaphor, making a robust critique unlikely. In this chapter, I examine the social dynamics of communities with an eye toward maximizing the effectiveness of the interactions between Bartha’s advocate and critic. In order to develop a robust critique of their metaphors, communities should adopt strategies based on a refined account of Helen Longino’s transformative criticism. Although there will be costs associated with adopting these strategies, I believe that by doing so, scientific communities can reduce the likelihood of the problem of ingrained analogy and indirectly become more socially responsible.

### **§5.1 Communities, Social Epistemology, and Cognitive Science**

As we have seen in Chapter Three, earlier analyses of analogical reasoning appeal primarily to individuals and the role they play in the analysis; even the rhetorical device developed by Bartha seems especially tailored to the individual member of a community.<sup>270</sup>

Those advocating such accounts assume that an individual’s background knowledge and

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<sup>270</sup> Informal logic texts too, such as Copi (1968) and its subsequent editions, are geared toward developing the analysis skills of the individual.

experience function as a possible source of criticism for analogical arguments. What takes place at the level of the community, however, can have profound effects on how individuals approach the critique of metaphor, and even whether members are aware of the metaphor's influence on their research practice. For example, members of a community may lack the diverse background knowledge and practical experience needed to develop a robust critique of a metaphor, which might hinder the performance of Bartha's rhetorical device. It may also be the case that even when members have the necessary diversity of knowledge and experience, their critiques may not receive uptake within the community.

Other reasons for addressing community-level dynamics are suggested by recent research in psychology and social epistemology. For example, in "How Scientists Think in the Real World," psychologist Kevin Dunbar examines the "reasoning strategies" of individual scientists, noting that analogical reasoning plays a key role in "generating hypotheses, designing experiments, and interpreting data" (Dunbar 2000, 52).<sup>271</sup> In fact, Dunbar's research suggests that the research aims of a scientist (whether developing a hypothesis, creating an experiment, or interpreting data) determine the role that analogies play in practice (Dunbar 2000, 54).<sup>272</sup> However, his research also reveals an unsettling feature of individual scientists' use of analogical reasoning: scientists are unlikely to remember their use of analogy at any given point in their research (Dunbar 2000, 55).<sup>273</sup>

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<sup>271</sup> See Dunbar (2000) pp. 53-54. He also extends the results of his research to the improvement of scientific education by suggesting a set of "rules" for improving the professional development of future scientists (Dunbar, 57).

<sup>272</sup> When we consider the research of Holyoak and Thagard, we see that the inverse—that analogies affect the research aims of scientists—is true as well. See Holyoak and Thagard (1995), especially pp. 186-191 and Stepan (1986).

<sup>273</sup> Dunbar notes,

When we go back and ask the scientists to remember how they generated a new concept or solved a problem at a meeting that we recorded, they have little memory of how it occurred, particularly with regard to analogies. It appears that the scientists remember the results of their reasoning rather than the small steps that they made or the different analogies that they used to make a discovery. As a result, scientists tend to undervalue the role of analogies from the same domains or to other related organisms.

(Dunbar 2000, 55)

According to Dunbar’s research, individual scientists fail to recall when they rely on analogical reasoning (Dunbar 2000, 55). While I do not suggest that a community would be able to recall its use of analogy when a member does not, I do believe that community-level dynamics might serve to counteract the limitations of individual reasoning; a community might incorporate strategies that promote and/or encourage the awareness of analogical reasoning. Of course, it is not merely the scientist forgetting his/her particular use of analogy that signals the need to develop strategies for use within communities. Another motivation is what social epistemologists refer to as the Independence Thesis.<sup>274</sup>

The Independence Thesis is the specification of an insight that is central to accounts of scientific communities in terms of distributed cognition, such as those posited by Philip Kitcher and Paul Thagard.<sup>275</sup> Mayo-Wilson *et al.* note that “rational individuals can form irrational groups and [...] conversely, rational groups might be composed of irrational individuals” (Mayo-Wilson *et al.*, 653-654). In light of this thesis, Mayo-Wilson *et al.* state “the methodological prescriptions for scientific communities and those for individual scientists are logically independent” in the context of making decisions (Mayo-Wilson *et al.*, 653-654).<sup>276</sup> For example,

Groups composed of a random assortment of problem solvers may outperform a group of the individually best problem solvers (Hong and Page 2001, 2004). Unreliable individuals might pool their information in such a way as to create reliable groups (Surowiecki

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<sup>274</sup> According to Mayo-Wilson *et al.*, “The underlying spirit of the Independence Thesis has motivated the creation of a new field—social epistemology—which focuses explicitly on the epistemic properties of groups rather than individuals (Goldman 1999)” (Mayo-Wilson *et al.*, 654).

<sup>275</sup> See Kitcher (2003 & 2011) and Thagard (2012).

<sup>276</sup> Mayo-Wilson *et al.* note

[...]this thesis has been defended in various forms. Individuals who desire credit more than truth may divide “cognitive labor” across competing research programs better than do truth-seeking scientists, thereby inadvertently improving the scientific community’s chances of discovering truths (Goldman 1992; Kitcher 1993; Strevens 2003). Individuals who refuse to abandon their favored theory, even in light of strong evidence against it, may help to make sure good theories are not prematurely abandoned by the broader scientific community (Feyerabend 1965, 1968; Popper 1975; Kuhn 1977; Hull 1988; Zollman 2010).

(Mayo-Wilson *et al.*, 654)

2004; Goodin 2006). Groups in which a significant amount of information is ignored might do better than groups in which information flows freely because an appropriate amount of diversity can be maintained in the absence of information (Zollman 2007, 2010).

(Mayo-Wilson *et al.*, 654)

Mayo-Wilson *et al.* claim that the Independence Thesis has two “important implications for history and philosophy of science (Mayo-Wilson *et al.*, 656). First, historical and sociological researchers should not focus exclusively on the individual or the community (Mayo-Wilson *et al.*, 656). Instead they must consider the contributions of both in order to provide an accurate account of how science produces knowledge. Second, “a normative theory that evaluates scientific methodology by focusing on a single scientist will not necessarily ‘scale up’ to a theory about groups” nor will methodologies appropriate for groups be successful for an individual working in isolation (Mayo-Wilson *et al.*, 656). In other words, strategies for dealing with the problem of ingrained analogy should be designed so as to be applicable to each level of use. As we have seen, Bartha provides a method for analyzing analogy at the level of individual interactions, but his device of advocate and critic will not be effective if the community lacks the epistemic resources to motivate a robust critique of its metaphors. In light of the findings reported by Kevin Dunbar, that researchers rarely recall their own use of analogy, as well as the implications of the Independence Thesis, I propose a set of strategies that address the capacity of metaphors to influence collective thinking, both at the level of individual research and community practice.

## **§5.2 Prior Accounts for Critiquing Scientific Metaphors**

Recall from Chapter 3, that a metaphor is a domain, which is comprised of the sets of similarities and differences between a source and target. These sets, known as the positive and negative analogies of the metaphor, figure in the premises of individual analogical

arguments that support a particular model or hypothesis. For example, the metaphor that linked molecules with billiard balls trades on certain relevant similarities and the lack of relevant differences between the two analogues. Of course, molecules and billiard balls differ in several ways. For example, billiard balls are often different colors and marked by numbers, where molecules are not. Such differences are crucial to questions about color, but if scientists are interested in the behavior of gas molecules, then color is unimportant.

When the behavior of molecules is the focus of research then other similarities take precedence. For example, Newton's laws of motion describe the collisions of billiard balls. If scientists infer that gas molecules behave analogously to billiard balls, then Newton's laws of motion can be used to describe the behavior of gas molecules as well. When claims about the similarities and differences in the sets of analogies of a metaphor are erroneous, however, scientists whose research program is informed by the metaphor can be misled; a metaphor that assumes color is relevant to the movement of billiard balls may not motivate many research projects that focus on motion.<sup>277</sup> In this way, scientists, and perhaps even larger society, become "unconscious victims of the metaphor" (Turbayne 1971, 5).

In the literature, two authors argue for adopting a critical stance toward metaphorical speech. The first, tendered by Colin Turbayne, suggests introducing a new metaphor in order to challenge the assumptions of the original metaphor. Ideally, the new metaphor serves as competition by positing new relations intended to supplant ones previously held.<sup>278</sup> The second approach, offered by Doren Recker, invokes a Humean strategy that involves critically scrutinizing the similarities by searching for relevant dissimilarities between the target and the source analogues. Recker believes that by locating a relevant difference the metaphor is weakened, which prompts the community to search for other

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<sup>277</sup> While most members of the community would drop research on the motion of gas molecules, there may be a few, those on the fringe of the community who still pursue research in spite of the difference.

<sup>278</sup> I recognize that there may be more than one competing metaphor supported by the community. However, I will focus on one for simplicity's sake.

metaphors. The approaches endorsed by Turbayne and Recker support adopting a critical stance at the community-level that challenges the analogies underwriting the metaphor. However, as we shall see, neither approach is sufficient for dealing with the problem of ingrained analogy.

In *The Myth of Metaphor*, Turbayne makes a crucial distinction between relying on a metaphor from being used by one. He observes,

[those who use a metaphor] represent the facts of one sort *as if* they belong to another, the latter [those who take a metaphor literally] is to claim that they actually belong. While the former adds nothing obviously to the actual process, the latter involves the addition of features that are the products of speculation or invention instead of discovery. It thus involves the insinuation of metaphysics.

(Turbayne 1971, 4)

According to Turbayne, when scientists accept the analogies associated with a metaphor as accurate, literal comparisons, they ascribe non-existent features and relations of the analogy to the target. In other words, scientists that adopt a metaphor uncritically, or treat a model as though it was the phenomenon modeled, put themselves in the position of being abused by the metaphor (Turbayne 1971, 26). As Turbayne notes,

The victim of metaphor accepts one way of sorting or bundling or allocating facts as the only way to sort, bundle, or allocate them. The victim not only has a special view of the world but regards it as the only view, or rather, he confuses a special view of the world with the world.

(Turbayne 1971, 27)

Gould's analysis of Morton's 19<sup>th</sup> century race science illustrates how an individual scientist failed to consider alternative views, even in light of contrary evidence. Morton, however, was not the only one affected by the ingrained analogy of this particular metaphor. As I have noted, the *race/species* metaphor implied racial differences in intelligence,

temperament, and aptitude, and legitimated unjust social policies against racial groups, such as Black slavery and Indian Removal.<sup>279</sup>

To counteract a metaphor's negative effects and avoid being misled, Turbayne warns that individuals relying on metaphor must recognize the potential dangers that arise when the correspondences implied by a metaphor are taken literally (Turbayne 1971, 21). As support, Turbayne references Max Black's interactive view of metaphor, and according to Black, those relying on metaphor should not gloss over "the shifts in attitude that regularly result from the use of metaphorical language" (Black 1954-5, 289).<sup>280</sup> When one first encounters a metaphor, its non-literal similarities capture the mind's attention and suggests novel connections. Summarizing Black's view, Turbayne notes, "an effective metaphor...acts as a *screen* through which we look at the world; or it *filters* the facts, suppressing some and emphasizing others" (Turbayne 1971, 21). Once ingrained, however, the novel connections introduced by the metaphor become commonplace, and the content of the metaphor is taken as fact (Turbayne 1971, 21-22). As in the case of Morton's research, the "facts" associated with a metaphor direct research by drawing attention to and directing attention away from particular features highlighted by the metaphor.

Turbayne maintains that merely recognizing the potential danger of taking a metaphor literally is not sufficient to prevent being misled by it. As Turbayne explains, one cannot simply re-allocate the "facts" to the proper metaphysical categories because "we never know exactly what the facts are. We are always victims of adding some interpretation" (Turbayne 1971, 64-65). Thus, instead of trying to fix the existing metaphor, Turbayne suggests "the

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<sup>279</sup> Historian Ann Fabian discusses the repercussions of the 19th century skull collections, noting that "the skulls have become embattled objects, but in the long run it might be easier to rebury the collected skulls than to rid our culture of the racist ideas that some extracted from collected skulls" (Fabian 2010, 220).

<sup>280</sup> In fact, the change in attitude that occurs with the use of metaphor makes it such a powerful tool of language. For example, "A wolf is (conventionally) a hateful and alarming object; so, to call a man a wolf is to imply that he too is hateful and alarming" (Black 1954, 289). Black's shift in attitude is similar to the mental leap proposed by Keith Holyoak and Paul Thagard. The mental leap is "like a spark that jumps across a gap, an idea from the source analog is carried over to the target. The two analogs may initially seem unrelated, but the act of making an analogy creates new connections between them" (Holyoak and Thagard 1995, 7).

best way to avoid being victimized by a metaphor worn-out by overuse is to show that it is expendable” (Turbayne 1971, 65). In other words, Turbayne thinks it is better to test the relations of an existing metaphor by contrasting them with the relations of another, in a continual effort to “search for the best possible metaphor” (Turbayne 1971, 65). The practice of introducing a competing metaphor is understood here to open up a new way of looking at the world, and this puts the community in the position of selecting a better metaphor (Turbayne 1971, 66).<sup>281</sup>

Although Turbayne’s account offers a solution applicable both at the individual and community level, the cost of such an approach may be a tendency to discard metaphors before their fruitful implications have been fully explored. If only one feature of the metaphor is discovered to be problematic, the community may be able to correct it. For instance, a community might realize that a perceived difference is actually a similarity, or vice versa. Recall the metaphor linking the movement of billiard balls with that of gas molecules, the community in this case might revise its information about a source, and thus realize that the color of billiard balls has no bearing on their movement. Just because a source analogue is more familiar to the community than a target analogue, however, it does not mean that we are justified in our knowledge about the source or that we know all there is to know about a source; knowledge is after all contingent. A simple correction to a community’s beliefs about a particular source could motivate scientists to construct different arguments and pursue different questions regarding the behavior of gas molecules.

Another reason for not discarding a metaphor is that a new source analogue might not be available, and as Kuhn describes, scientists are reluctant to abandon their paradigms.<sup>282</sup>

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<sup>281</sup> Turbayne states, “Now that we know we are choosing between different metaphors, tests rarely resorted to reveal themselves. But it would be premature to apply them without first putting the metaphors to work within a specific subject matter where they can be used to produce different theories capable of being tested by those canons customarily used to test any scientific theory” (Turbayne 1971, 70).

<sup>282</sup> Kuhn states, “what scientists never do when confronted by even severe and prolonged anomalies. Though they may begin to lose faith and then to consider alternatives, they do not renounce the paradigm that has led them into

Even in the event a competitor is found, it may share the same problematic features as the current one. When a metaphor is first introduced, members of a scientific community are not likely to know whether a particular metaphor will hinder or aid epistemic progress. As Irving Copi notes, “to draw an analogy between two or more entities is to indicate one or more respects in which they are similar” (Copi 1978, 379).<sup>283</sup> Members of a community may lack the epistemic diversity necessary for developing a new metaphor, especially if the community views the research problem in a narrow context. In this case, even if the community does arrive at a new metaphor, who is to say that it will not be liable to the same inaccuracies as the first? As we have seen, metaphors, and the analogies underpinning them, are products of a particular community.

Like Turbayne, Doren Recker believes that once an analogy has become ingrained, re-inserting “like” back into the metaphorical expression does not solve the problem (Recker 2004, 217). In fact, according to Recker, this strategy fails to overcome the human psychological tendency to equate the figurative correspondences posited by the metaphor and the way the world actually is (Recker 2004, 217).<sup>284</sup> As a solution, he proposes a strategy originating with David Hume, which calls for an active search for the dissimilarities between the source and target (Recker 2004, 217).<sup>285</sup> According to Recker, highlighting the differences between the source and the target greatly weakens likelihood of accepting

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crisis. They do not, that is, treat anomalies as counterinstances, though in the vocabulary of philosophy of science that is what they are . . . Once it has achieved the status of paradigm, a scientific theory is declared invalid only if an alternate candidate is available to take its place. No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature” (Kuhn 1962, 77). Thanks to Paul Bartha for pointing this out.

<sup>283</sup> As Holyoak and Thagard note “although making an analogy requires a leap, that leap need not be blind or random. In all its manifestations, analogy is guided by the basic constraints of similarity, structure, and purpose...” (Holyoak and Thagard 1995, 7).

<sup>284</sup> As Recker notes, “part of the problem [of being misled by an analogy] is *psychological* rather than logical, concerned with how we do think as opposed to how we *should* think” (Recker 2004, 217).

<sup>285</sup> Hume offers an analysis in his Treatise 1.3.12, see Hume (1896). According to Hume, “in the probability deriv’d from analogy, ‘tis the resemblance only, which is affected. Without some degree of resemblance, as well as union, ‘tis impossible there be any reasoning: but as this resemblance admits of many different degrees, the reasoning becomes proportionably more or less firm and certain” (Hume 1896, 142).

unwarranted extensions of the metaphor; thus, preventing the analogy from becoming ingrained (Recker 2004, 217). The search for dissimilarities mobilizes the critique of the metaphor in two ways: first, it serves to heighten the awareness of the non-literal content of the metaphor and second, it may yield a relevant difference, a feature that may void the analogy altogether. The difficulty, according to Recker, is breaking the psychological influence exerted by an ingrained analogy (Recker 2004, 217).<sup>286</sup>

Recker's search for dissimilarities, like Turbayne's introduction of competing metaphors, fails to achieve robust critique, especially when members of the community are so epistemically comfortable with their metaphor that they cannot conceive of differences between the source and target. This is not difficult to imagine when one recalls that metaphors and analogies make use of what is familiar in order to understand what is lesser known. Communities that are homogeneous—ones whose members have similar backgrounds and practical experiences—will not have the resources to achieve a robust critique capable of destabilizing a deeply ingrained analogy. Instead, the differences presented by individuals (who are immersed in the values and traditions of a community) may have little to offer in critiquing the assumptions that ground a community-endorsed metaphor.

Although Turbayne and Recker posit slightly different solutions for dealing with problematic metaphors, their accounts share an important similarity: the need for communal awareness of metaphorical speech.<sup>287</sup> As we have seen, however, the introduction of a new metaphor as Turbayne suggests and the search for dissimilarities between source and target

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<sup>286</sup> Recker (2010) details the psychological grip maintained by the persistent *nature as a machine* metaphor. Although this metaphor has been scientifically refuted, it continues to influence public opinion. According to Recker, those under the sway of the metaphor will not find any motivation to not rely on their default reasoning unless given some reason to do so (Recker 2010, 660-662). By introducing the differences between the analogs, one may be able to break the hold that intuitive reasoning maintains (Recker 2010, 660-662).

<sup>287</sup> Black (1962) states that awareness of the "extension of meaning" is required for an interactive metaphor to work, see pg. 39. To prevent an ingrained analogy, I suggest maintaining a communal awareness to prevent the metaphor from becoming entrenched.

as Recker proposes, cannot be counted on to generate a robust critique of the correspondences posited by a scientific metaphor. A robust critique requires something more: a method for introducing the much-needed epistemic novelty and diversity necessary for maximizing the critical interactions within a scientific community, especially when the community cannot do so on its own.

### 5.3 Endorsing Strategies for Effective Scientific Practice

A scientific metaphor—one that guides practice and acts as a filter for interpreting data—is the product of a particular community. As we have seen, for a metaphor to be effective, the community must recognize the similarities and differences between the source and target analogues, but the community should also be aware of the subtle psychological influence of these analogies. The metaphor directs research and highlights perceived similarities between the source and target. As Turbayne suggests, “there are other ways of viewing the world besides those that we inherit from the great sort-crossers of the past who, by their genius, hold us enthralled” (Turbayne 1971, 56). Yet, encouraging communities to critique that which they find comfortable or familiar is a formidable task.

First, consider the social aspect of Bartha’s rhetorical device: the interaction between the advocate and critic. According to Bartha, “The rhetorical device is to imagine that the analogical reasoning is presented by an enthusiastic advocate to a polite but moderately skeptical critic” (Bartha 2010, 5).<sup>288</sup> As described, the rhetorical device “helps to set a standard of justification,” which can be applied in various contexts (Bartha 2010, 5). To encourage a robust critique of metaphors used in scientific communities, I suggest thinking about Bartha’s rhetorical device in terms of the broader role that analogies play in these communities: the interactions between a metaphor and the arguments generated by it. In

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<sup>288</sup> The critic will be skeptical enough to not only verify the advocate’s statements of relevance, but also to be able to introduce competing claims or evidence within the bounds of rationality.

order to generate a robust critique of its metaphors, a community will need to exploit the tension created by the device—the “competing pressures” of being explicit and economical—which makes possible the critical presentation and analysis of a metaphor (Bartha 2010, 102).<sup>289</sup>

At different times, members of a scientific community will be involved in the assessment of an analogical argument as either a critic or an advocate. If they are members of the same community, the background knowledge of both critic and advocate will be similar with respect to previous scientific theory. Even so, individuals may differ with respect to other evaluative standards, such as “simplicity and competence” (Bartha 2010, 7). For example, members of the same community will share the same professional history, standards, and methods in a manner akin to Hankinson-Nelson’s epistemic communities or Thomas Kuhn’s scientific communities that work under a shared paradigm.<sup>290</sup> Of course, some differences—those of experience and values—shape how the critic and advocate utilize their background information in the evaluation of an analogical argument (Bartha 2010, 7). In order to develop a robust critique of the positive and negative analogies associated with a metaphor, scientific communities will need to adopt methods that enable them to promote and exploit the diverse background information of its members.

#### Examples where diverse members made a positive contribution

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<sup>289</sup> For Bartha, the critic wants the advocate to be explicit. This requires detailing what the advocate takes to be support for the analogy; of course, this gives the critic an opportunity to detect a weakness in the argument. In contrast, the advocate wants to be economical because only some factors will be relevant to the argument (Bartha 2010, 102). A robust critique will enable the critic to challenge what similarities and differences the advocate believes are relevant.

<sup>290</sup> See quote from Hankinson-Nelson (1993, 334-335) above, and Kuhn (1970). In addition, Kuhn (2000) discusses scientific communities, noting that

The analytic unit would be the practitioners of a given specialty, men bound together by common elements in their education and apprenticeship, aware of each other’s work, and characterized by the relative fullness of their professional communication and the relative unanimity of their professional judgment. In the mature sciences the members of such communities would ordinarily see themselves and be seen by others as the men exclusively responsible for a given subject matter and a given set of goals, including the training of their successors.

(Kuhn 2000, 147)

to scientific practice guided by metaphor can be found in a variety of fields, such as primatology, biology, and archaeology. In these fields, socially conscious, female scientists challenged the gendered assumptions of dominant metaphors guiding research, giving rise to a fuller, if not epistemically better, explanation.<sup>291</sup> The contributions made by feminist scientists raise an important question about what types of experience or background information should be maintained or promoted within a community. Given the variety of disciplines and research questions, articulating the specific experience and background knowledge that the members of a community will need in order to generate a robust critique will be difficult; generally speaking, criticism will come from those who have diverse practical experience or knowledge about the phenomenon under investigation or the similarities/differences in the positive and negative analogies. Although those in previously marginalized race/gender/class groups have made some key critical insights about race/gender/class metaphors, I do not want to make the claim that epistemic diversity tracks race or gender. In fact, there are examples where women scientists did not challenge the dominant metaphors of their discipline, and in some cases supported androcentric practices/explanations.<sup>292</sup> Instead, my strategies are intended to exploit epistemic diversity amongst a community's members and when this is not possible, the strategies will assist in finding relevant, critical sources.

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<sup>291</sup> Specific examples for primatology include Haraway (1989) and examples from biology include Martin (1996) and Okruhlik (1994). For examples in archaeology, see Wylie (2002) particularly the types of contributions she distinguishes as "content critiques," and for more on the feminist contribution to the critique of science in general, see Wylie (2012).

<sup>292</sup> For example, Tuana (1996) notes that female scientists adopted their feminist positions as a result of their "participation in feminist communities" (31) and Rosser (1986) stresses that a "substantial number of women" needed to be in the discipline prior to exposing the biases favoring males in science. Rosser (2002) also points out "As long as only a few women were scientists, they had to demonstrate or conform to the male view of the world in order to be successful and have their research meet the criteria for 'objectivity'" (229). Of course, it was not only pre-feminist scientists who preserved androcentric science. Kourany (2010) points out "women scientists have also made contributions indistinguishable from those of the men, contributions sometimes just as sexist and androcentric as those of the men" (65). Kourany also cites a study "analyzing 40 years of gender-difference research [...] in four journals of the American Psychological Association" which reveals that "males are still treated as the norm" against which females, both girls and women, are measured (2010, 65).

Kristen Intemann, cautions against assuming “that any epistemic community that includes a member of a historically oppressed group will have these epistemic advantages” (Intemann 2010, 789).<sup>293</sup> She also adds that “the mere presence of a member of an oppressed group will not be sufficient to achieve the sort of conscious, critical reflection that is required for achieving a standpoint” (Intemann 2010, 789). While contributions from particular socially defined situated knowers (in light of gender, sexual orientation, race, etc.) have often proven to be pivotal in opening up critiques along these lines, simply diversifying the membership of a community will not be enough to challenge the analogies associated with a metaphor. Other experiences—such as a hobby or familiarity with a subject not wholly related to the scientific discipline—may prove invaluable as well. What one knows about the target and source analogues (or even about particular properties associated with them) can add to the background knowledge of the critic and advocate, and the more diverse this practical experience is, the better.<sup>294</sup>

Since communities cannot anticipate what particular experience will be necessary in order to challenge the assumptions of their metaphors, I propose that scientific communities maximize the interactions between advocates and critics by adopting strategies based on a refinement of Helen Longino's account of transformative criticism. Not only will these strategies make the most of Bartha's rhetorical device in sufficiently diverse communities, they will also aid in recruiting and exploiting external sources of diversity, which is critical when the community lacks the resources for developing a robust critique of the assumptions associated with its metaphors.

Originally intended as a social account of scientific rationality—where communities that meet certain conditions achieve objectivity—Longino's account requires communities to

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<sup>293</sup> See also Solomon (2009).

<sup>294</sup> For example, Robert Lang holds degrees in engineering and physics (with a background in optics) and yet it was his expertise in origami that was sought by Lawrence Livermore National Laboratory to help design a telescope with a “football-field sized lens [that] could be folded into a small rocket” (O'Carroll 2012).

adopt norms of practice that promote a critical analysis of social values (Kourany 2010, 58 and Longino 2002, 129). According to Longino, “critical discursive actions are social processes of knowledge production. They determine what gets to remain in the public pool of information that counts as knowledge” (Longino 2002, 129).<sup>295</sup> Given that I seek to develop a critical attitude toward the use of metaphor within scientific communities, the critical discursive actions pointed out by Longino yield the type of analyses pertinent to my account; when applied to the communal process of critiquing metaphor, the strategies will motivate critique within the community as well as inform the community’s treatment of external critique.

As we have seen, the primary goal of preventing the problem of ingrained analogy requires that the critique of the assumptions associated with a metaphor be “epistemologically effective” (Longino 2002, 129). If knowledge is produced when it “survives criticism from multiple points of view,” as Longino claims, then a community must ensure that it is in a position to challenge the epistemic content of the analogies associated with the metaphor. Since an idealized conversation between epistemically homogeneous individuals cannot ensure a robust critique, the community is responsible for developing a critical attitude toward metaphor as part of its general practice. This becomes apparent once we consider Longino’s conditions for transformative criticism, which include public venues that promote critique, a sincere uptake of criticism within the community, an exercise of what Longino refers to as “tempered equality,” and public standards of evaluation (Longino 2002, 131).<sup>296</sup> According to Longino, communities that meet the requirements laid down by the above conditions will “assure the effectiveness of discursive interactions” (Longino 2002,

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<sup>295</sup> Referring to laboratory studies, Longino describes knowledge production as “an amalgam of heterogeneous acts” as opposed to a “particular kind of truth-producing activity guided by logic” (Longino 2002, 7). Many different considerations go into whether scientists pursue their research goals (Longino 2002, 7). Sources of funding, the network in which one’s research resides, and the nature of the phenomena under examination are examples of the context that often inform “research decisions” (Longino 2002, 7).

<sup>296</sup> Due to issues of clarity, I have switched the order of the final two conditions.

129). Of course, Longino recognizes that this assumes the community relies solely on its existing diversity, which has its limitations. Therefore, the strategies for ensuring that the relevant epistemic diversity to generate robust critique must address the need to be receptive whether the critique is made from within or external to the community. In the remainder of this section, I consider each of Longino's conditions in turn as a possible strategy for reducing the likelihood of an ingrained analogy.

The first condition introduced by Longino calls for the community to maintain "publicly recognized forums for the criticism of evidence, of methods, and of assumptions and reasoning" (Longino 2002, 129). According to Longino, this condition requires not only that criticism occur "in the same standard and public venues in which 'original research' is presented," but it also "should be given the same weight or nearly the same weight as is given to 'original research'" (Longino 2002, 129). Citing John Stuart Mill's justification for the public critique of ideas, Longino explains "criticism not only spurs evaluation and reevaluation of hypotheses, but also leads to the better appreciation of the grounds and of their consequences" (Longino 2002, 129).<sup>297</sup> Longino's condition is well illustrated by precisely the kinds of internal critical debate about the viability of Darwin's analogical argument in the *Origin* that I discussed in Chapter Two.<sup>298</sup>

Longino admits that some may think her condition for public forums is "trivial," pointing to the existence of discipline-specific journals and conferences already in place (Longino 2002, 129). She is quick to point out, however, that current trends to privatize or commercialize science run counter to what this condition requires. For example, corporate entities doing research for the sake of profit, patent, or national security are not likely to be forthcoming or transparent with their research. In addition, recent research on social bias in

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<sup>297</sup> For more information regarding Mill's position, see Chapter II in Mill (1986)—particularly pages 42-45 and 61-63.

<sup>298</sup> Recall that in his correspondence, Darwin admitted that his belief in the truth of his theory was increased given his ability to respond to his critics.

peer review demonstrates how, Longino's conditions notwithstanding, the peer review practices of journals are not fully objective. According to Carole Lee and Christian Schunn, the background beliefs of members within a particular community play an important role in "determining what research and claims get admitted into community-wide spaces such as journals and conferences" (Lee and Schunn 2011, 355). While these beliefs can negatively impact the peer review processes of proposed venues, standard notions of diversity do not offer the needed critique of social biases. In order to mitigate the biasing effects of background beliefs on peer review processes, Lee and Schunn suggest that Longino should "require a specific form of perspectival diversity: namely, a diversity of perspectives bearing on cultural norms and beliefs within and about the knowledge community" (Lee and Schunn 2011, 363). The perspectival diversity proposed by Lee and Schunn is intended to provide a critique of the personal beliefs and assumptions that inform the evaluative process found in peer review. As we have seen, the metaphors and analogies found in science are evaluated via the background information of the advocate and critic. One worry might be that the amount of epistemic diversity that happens to be in a given community may not be fully critical of the background assumptions that undergird a community's reliance on metaphor.

Current venues (like journal articles, presentations, and conferences) do not encourage or require analyses of the conceptual metaphors guiding scientific research. Instead, any critical focus in current venues has been directed toward evaluating individual bits of scholarship against a background of theory, while at the same time ignoring the conceptual underpinnings of these theories. Both Ruth Hubbard (1988) and Sandra Harding (1991) call attention to the common practice of stripping context found in the different venues of the community, such as presentations, papers, and discussions of research.<sup>299</sup> In these venues,

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<sup>299</sup> As Harding notes,

scientists gloss over key features that might influence their work (such as funding sources, sponsorship, or corporate relations) and the politics of the time (both within the larger society and amongst other research groups).

As we have seen in the case of Morton's research on human difference and the findings in BOR, scientists do not critically engage the analogical reasoning motivating their research. Recall that Dunbar reported that scientists are not likely to recall their use of analogy and even if they did, Karin Knorr-Cetina describes how scientific papers are devoid of the contextual features of the research (Knorr-Cetina 1983, 115-140). Creating venues for the critical analysis of metaphor, or at least ensuring a dedicated space for such critique within current venues (journals, presentations, and conferences) might motivate scientists to make transparent the detailed records of their research, and possibly even raise awareness to the use of metaphor and analogy in practice. This could be achieved by creating spaces for the discussion and critique of metaphor in scientific papers, journals, and conferences, or by creating independent venues focused specifically on the reasoning done by scientists and their communities.<sup>300</sup> Requiring scientists to make explicit their use of analogical reasoning might counter the psychological tendency to uncritically accept the assumptions of the metaphor.<sup>301</sup>

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Science generates capital in the form of information, ideas, and technologies that are used to administer, manage, and control the physical world and social relations. When human activity is divided in hierarchical ways, those who engage in 'ruling class' activity can have only a partial and distorted understanding of nature and social relations. For this reason, laboratory life especially is the wrong activity from which to try to describe and explain the causal relations of administering, managing, and controlling the physical world and social relations.  
(Harding 1991, 95)

The myth of a value-free methodology often attributed to physics should not be expected nor required of any science. Neither should an idealized narrative that is devoid of context be the framework of scientific papers and presentations, especially if we seek a robust critique of metaphor in science.

<sup>300</sup> Journals and narratives may provide some assistance. In my research, the journals of Benjamin Franklin, James Clerk Maxwell, and Charles Darwin offered some insight to the use of analogical reasoning by scientists. I would like to explore this area in a future paper.

<sup>301</sup> As Paul Bartha notes that "structuralist" accounts of analogical reasoning in cognitive science trade on a drive to "maximize overall similarity" between analogous domains (Bartha 2010, 32 & 195-197). See also Gentner (1983) and

Of course, it may be the case that the scientific community lacks the epistemic diversity to develop a critical discourse of the kind needed to address the problem of ingrained analogy. As we saw in Chapter Four, Samuel George Morton was somewhat transparent about his research; he made the results of his research and his methods available for analysis. Yet, even though Morton described his research methods, his reliance on the metaphor of *race as a distinct species* was not explicitly presented in his books and thus, not subject to scrutiny.<sup>302</sup> Even if Morton had submitted his metaphor for evaluation, the community in which he published his research, however, lacked the resources to pose a robust critique.<sup>303</sup>

Unlike Darwin's challenge to the religious status quo, Morton's research was embraced by American society and used as a justification for the oppressive policies of slavery and Indian removal.<sup>304</sup> Morton's research was widely accepted; it was only questioned by the Rev. John Bachman and noted abolitionists, such as Frederick Douglass.<sup>305</sup> Of course, venues for criticism, even if numerous, are ineffectual if the criticism is not taken seriously; as we shall see Frederick Douglass objected to Morton's research, but he was ignored. Yet, it is reasonable to ask if Morton's community had been more receptive to an external critique of

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Holyoak and Thagard (1995). As Dunbar suggests, scientists are not aware of their use of metaphor. Requiring scientists to account for their reliance on analogical reasoning in their papers, journals, and presentations, could serve two purposes: it would emphasize that the assumptions of the metaphor are not literal descriptions of the world and the assumptions would become a point of public critique.

<sup>302</sup> In an effort to be fair, I should note that Morton's assumptions regarding the creation/origination of distinct species was subject to debate. Though I believe the reader will agree that Morton did not faithfully consider the merit of his opposition, it made little difference because his opposition assumed the wholesale inferiority of non-white races as well. This, of course, demonstrates the homogeneity of background assumptions within the community that I believe are extremely problematic.

<sup>303</sup> The transparency of Morton's research enabled Stephen Jay Gould, and Jason Lewis *et al.*, to debate Morton's methods approximately 100 years after the original research. To his credit, Morton provided at the very least talking points about bias in scientific practice. Of course, it was not only Morton's transparency nor his tables of data that inspired the undertaking. Instead, Gould's modern belief in the intellectual equality of individuals of different races motivated his critique of Morton.

<sup>304</sup> I do not mean to imply that President Andrew Jackson was aware of Morton's research or that he was familiar with Morton's *Crania Americana*. Instead, I suggest that Morton's research did not provide a reason for halting the cruel treatment of American Indians and Black slaves at the time. Intelligence, according to Morton, varied amongst the different races, which made unjust policies permissible.

<sup>305</sup> Rev. Bachman supported monogenesis, but even this still supported a racial hierarchy. Douglass' address, "The Claims of the Negro Ethnologically Considered," was not issued until 1854 while Morton began publishing his results as early as 1839.

his polygenist assumptions, whether further challenges would have succeeded in demonstrating the weakness of his metaphor.

The creation and maintenance of venues for the critical discourse of metaphor and analogy would also prove useful in the case of brain organization research (BOR). Recall the part of Jordan-Young's analysis in the previous chapter, which illustrated how BOR literature failed to document an important shift from a binary model of sexual behavior to an orthogonal one. This omission created an appearance of support for brain organization theory, and obscured the community's reliance on assumptions that should have been an explicit target of critical discourse. For example, the community failed to document the changes in meaning of terms used by distinct models (the use of 'masculine' and 'feminine' appears consistent even though the terms had actually changed meaning with the introduction of the new model). In addition, the literature on research that supported the original binary model was included as support in articles on research using the orthogonal model, which poses a serious methodological problem for a discipline that relies on quasi-experiments (Jordan-Young 2010, 45). According to Jordan-Young, "quasi experiments derive their strength from their relationships with other studies," assuming that the studies "examine the same cause and effect" and the definitions and measures used in the studies are consistent with each other (Jordan-Young 2010, 42-55). Because the switch from the binary model to the orthogonal model was given little attention within the BOR community, the literature indicates a level of evidential support that is not warranted. As Jordan-Young points out, "scientists' definitions are usually left implicit" in BOR literature;

...scientists (those who conduct research related to the theory; those who review studies for journals, funding proposals, and other publications; and those who lean on the theory for broader arguments about sex differences or sexuality) have apparently never noticed these critical changes [when using research conducted under the binary model as support for the orthogonal model].

(Jordan-Young 2010, 114)

Reliance on one model as support for another quite different model is bad science, and had the BOR literature valued critical discussion of the model and the analogical reasoning supporting it the problem would have been detected. Subjecting the model switch to public critical analysis would not only motivate a discussion of the standards for justification related to quasi experiments, it would also generate a focused, critical discussion of the similarities and differences suggested by each model, showing which stable concepts, if any, could support BOR claims.

The BOR community might have benefitted from external critique as well. Instead, some of the research that might have posed a challenge to the dominant models (e.g. studies that focused on contextual explanations for behavior among women with CAH) received little uptake within the community.<sup>306</sup> Jordan-Young notes that researchers could “read the brain organization studies of CAH without ever encountering the sobering data [the competing explanations for sexual behavior referred to as context] on the overwhelming pain and discomfort during sex; the absence of clitoral sensation; and the feelings of shame, bodily inadequacy, and unattractiveness that are common among women with this condition” (Jordan-Young 2010, 246). In other words, “scientists doing brain organization studies tend[ed] to focus narrowly on the level of steroid hormone exposures alone, to the exclusion of virtually all other variables that affect development” (Jordan-Young 2010, 246). This is particularly troublesome given that contextual explanations associated with having and being treated for CAH contain non-hormonal reasons **known** to be relevant to behavior (Jordan-Young 2010, 246). When compared to the research of Morton and BOR, the public critique of Darwin’s theory of natural selection stands in stark contrast.<sup>307</sup>

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<sup>306</sup> According to BOR, women with Congenital Adrenal Hyperplasia (CAH) were subject to male hormones in while in utero, and thus exhibit unusual genitalia and male sexual behaviors.

<sup>307</sup> After the publication of the *Origin*, Darwin’s contemporaries challenged various aspects of his theory, including his analogy with artificial selection. Hull (1973) contains several reviews of the *Origin*. For example, the review from Thomas Vernon Wollaston—a Darwin supporter—admitted that it was possible for varieties to change by natural

Longino's second criterion requires communities to seriously consider and respond to the criticism that is made public through its venues. The community must not only create spaces for critical discourse, "but its beliefs and theories must [also] change over time in response to the critical discourse taking place within it" (Longino 2002, 129-130). The requirement for uptake imposes responsibilities on the members of the research community to engage in actively listening to criticism—while at the same time asking that the criticism be legitimate and "constructive" (Longino 2002, 130). Longino's second criterion requires a kind of responsiveness—the exchange of discourse—that Paul Bartha makes central to his rhetorical device, the interactions between an advocate and critic. Recall that the advocate is expected to specify explicitly the similarities and dissimilarities between the source and target so that the critic can focus on her search for relevant dissimilarities (Bartha 2010, 102). Both advocate and critic are obliged to be truthful and not to claim the existence of spurious similarities or dissimilarities (Bartha 2010, 104).

Darwin's 19<sup>th</sup> century debate on evolution is also an exemplar of Longino's second condition in action: the uptake of criticism within a community. In fact, Darwin responded to an array of criticism, some of which was directed at the similarities supporting Darwin's analogy between artificial and natural selection. For example, Richard Owen, Britain's leading comparative anatomist, believed that the case of the antlers of the deer in Windsor Forest was a failed test case for natural selection in action. Owen noted that the deer's antlers did not improve over time like the fiber or meat of domestic species (Hull 1973, 177

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selection, but doubted that it was likely for species (Hull 1973, 140). Another review, penned by François Jules Pictet, questioned whether the changes in artificial selection could actually take place in nature (Hull 1973, 145). More familiar responses, such as that tendered by Adam Sedgwick, focused on the permanency of species, the lack of a mechanism for change, and the lack of fossil evidence (Hull 1973, 161-163). Darwin's critics availed themselves of venues such as professional journals and society meetings—even a popular magazine, *The Spectator*, published Adam Sedgwick's objections (Hull 1973, 159<sup>fn\*</sup>). In his article, Sedgwick stressed that the existence of species was due to creation and although he did not go so far as to call Darwin an atheist, he believed Darwin's theory to be "intensely mischievous" (Hull 1973, 161).

& 200-201).<sup>308</sup> Darwin's critics also cited differences between artificial and natural selection they believed to be relevant. For example, some took issue with the amount of time required for gradual phenotypic change, arguing that the earth was not old enough for change capable of producing distinct species to take place.<sup>309</sup> Others, like William Whewell and Adam Sedgwick, believed that humans, who possessed a soul, were different from animals and not subject to evolution. Such differences were intended to challenge, if not directly block, Darwin's analogy.<sup>310</sup> Darwin took the reviews of the *Origin* seriously, and he responded to some of his critics in later editions.<sup>311</sup>

By contrast to Darwin's community, Morton's community lacked the requisite diversity for robust scrutiny of the assumptions associated with a metaphor. Although there were some venues for critical discourse, these proved to be inadequate in a community that focused only on methodological issues (not conceptual ones) within the community and ignored critique from those external to the community. Consider two challenges to Morton's theory. The first challenge came from the Rev. John Bachman, who took issue with Morton's position on the origins of humans.<sup>312</sup> Bachman believed that natural processes, like the climate or environment, might have caused the different physical traits associated with human races. Although Morton responded to Bachman's critique in his 1850 book, *Letter to the Rev. John Bachman, D.D., on the question of hybridity in animals considered in reference to*

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<sup>308</sup> According to Owen, the antlers of the deer in Windsor Forest should improve (become larger) with each successive generation, yet that is not the case (Hull 1973, 200). For Owen, variability implied unlimited variation and so pigeons, deer, etc. should demonstrate great changes (Hull 1973, 201).

<sup>309</sup> As regards time, Fleeming Jenkin challenges the amount of time needed, stating that Darwin's "view of past time is, we believe, wholly erroneous. So far as this world is concerned, past ages are far from countless; the ages to come numbered; no one age has resembled the past, nor will any future time repeat the past" (Hull 1973, 320).

<sup>310</sup> For an account of man's unique status, see the review from Adam Sedgwick (Hull 1973, particularly pages 164-165).

<sup>311</sup> In fact, a key difference between the first and sixth edition of the *Origin* is a chapter devoted to the criticism provided by St. George Jackson Mivart. In his book, *Genesis of Species* (1871), Mivart challenged the incremental development of complex structures, such as the eye. Mivart believed that the intermediate stages of eye would not offer a decided advantage in the survival and reproduction of an animal. In the sixth edition, Darwin responded by showing the possible development of the eye using different structures found in the animal kingdom. See Darwin (1872), Chapter Six.

<sup>312</sup> In his *Second Letter on Hybridity* (1850b), Bachman disagreed with Morton's polygenesis.

*the unity of the human species*, he merely reiterated his original view that climate or environment were not capable of producing the important physical differences amongst the various races of human beings (Hume 2008, 136-137).<sup>313</sup> In addition, Morton “cited examples from Lyell’s *Principles of Geology*” that supported the distinctness of species and races, then “proceeded to provide a number of examples from the animal kingdom, much as he had beginning with his original article” (Hume 2008, 137). In other words, Morton’s response was not a serious consideration of Bachman’s objection, but a restatement of his earlier claims.

The second challenge came from Frederick Douglass who in his 1854 address, “The Claims of the Negro Ethnologically Considered,” issued an objection to Morton’s polygenesis on moral grounds. Douglass states,

The evils most fostered by slavery and oppression, are precisely those which slaveholders and oppressors would transfer from their system to the inherent character of their victims [...] By making the enslaved a character fit only for slavery, they excuse themselves for refusing to make the slave a freeman. A wholesale method of accomplishing this result, is to overthrow the instinctive consciousness of the common brotherhood of man. For, let it be once granted that the human race are of multitudinous origin, naturally different in their moral, physical, and intellectual capacities, and at once you make plausible a demand for classes, grades and conditions, for different methods of culture, different moral, political, and religious institutions, and a chance is left for slavery, as a necessary institution [...] There is no doubt that Messrs. Nott, Gliddon, Morton, Smith and Agassiz were duly consulted by our slavery propagating statesmen.

(Douglass 1999, 287)

Douglass directly addressed Morton and his research in *Crania Americana*, attacking Morton’s attempt to classify Egyptians as non-Negro. Douglass notes that in *Crania*, Morton “repels the supposition the Egyptians were related to Negroes, by saying there is no mention

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<sup>313</sup> Morton believed these traits were unique to a specific geographical location and also “bred true and thus mattered in classification and development” (Hume 2008, 136).

made of *color* by the historian, in relating the marriage of Solomon with Pharaoh's daughter" (Douglass 1999, 289). In fact, Morton argued that the Sphinx, in spite of its facial appearance, was not patterned after the Egyptians (Morton 1839, 29).<sup>314</sup> Although Douglass is described by Philip Foner as "contribut[ing] considerably to the exposure of these pseudo-scientific theories," there was little uptake by Morton or any of the other polygenists (Douglass and Foner 1950, 282).

The challenges to Morton's theory illustrate with particular clarity how criticism and its uptake are vital to challenging entrenched assumptions of scientific metaphors. In order for Bartha's device of advocate and critic to be effective, the critical interlocutors must address all relevant criticisms, and when they do, they must do more than merely restate their original position. Morton did not take Bachman's objections seriously nor did he acknowledge that provided by Douglass. Although there was relevant epistemic diversity possessed by Douglass, he was a member of a marginalized race and considered to be an outsider by the scientific community. In cases where there is little critique generated from within the research community, communities incur a responsibility to be receptive to relevant critique coming from individuals outside of the community.

The BOR community, which lacks a relevantly critical perspective within the community, also fails to meet Longino's second condition. For example, Jordan-Young notes that the researchers on women with CAH have not engaged with "recent studies of sexual activity and function among women with CAH that do not entertain the brain organization hypothesis" (Jordan-Young 2010, 245). According to Jordan-Young, this research is not part of the brain organization metaphor, which assumes the androgenization of the brain due to hormone exposure, and thus, is not "cited in the brain organization literature" (Jordan-Young

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<sup>314</sup> Morton suggests that the Sphinx was a shrine created for the Negro populations in Egypt (Morton 1839, 29). Citing Cuvier, Morton establishes that Egyptians were in some way related to the Greeks and Romans, and therefore were Caucasian (Morton 1839, 31).

2010, 246). The very narrow scope of the BOR metaphor—which focuses solely on prenatal hormones—prevents BOR researchers from considering other explanations of putative sexual behaviors. As Jordan-Young describes, “scientists doing brain organization studies tend to focus narrowly on the level of steroid hormone exposures alone, to the exclusion of virtually all other variables that affect development” (Jordan-Young 2010, 246). Thus, researchers who include the experience involved with being treated for CAH (undergoing invasive procedures, exams, physical pain, etc.) in their explanations of non-standard, gendered behavior are not part of the BOR literature and never receive uptake.

Unlike the research conducted by Morton and the BOR community, Darwin not only listened to his critics, he responded to several of them, as did his supporters.<sup>315</sup> In addition, Darwin did not merely restate his theory; he directly addressed the concerns of his critics through a considerable amount of correspondence or in the additional five editions of the *Origin*.<sup>316</sup> Darwin did not limit his correspondence solely to men or scientists. Although his ideas about gender and social status were a product of his time, he corresponded with those considered outside of natural philosophy such as females who pursued scientific endeavors like botany, and of course, he made extensive use of the knowledge of breeders.

Longino’s third criterion, which she refers to as *tempered equality of intellectual authority*, is critical for reducing the likelihood of an ingrained analogy because it is a demand that a hypothesis be subject to a range of criticism while it also stresses not every member will be granted intellectual authority despite the “duties of inclusion and attention” (Longino 2002, 131-133). This criterion depends upon the “intellectual authority, not cognitive authority” of its members; the community grants some members weighted input

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<sup>315</sup> I do not claim that he responded to all. Some Darwin scholars believe that supporters, like Thomas Henry Huxley, did a large share of the public defense due to Darwin being ill at an early age (Ruse 2003, 1523). Ruse notes “Huxley preached evolution-as- world-view at working men’s clubs, from the podia during presidential addresses, and in debates with clerics—notably Samuel Wilberforce, Bishop of Oxford” (Ruse 2003, 1524).

<sup>316</sup> For an interesting contrast between Darwin’s description of gender in his publications and his personal letters, refer to the Darwin Correspondence Project, < <http://www.darwinproject.ac.uk/darwin-and-gender-introduction>>.

on matters when they have more knowledge/experience (Longino 2002, 133).<sup>317</sup> The norm of tempered equality is intended to ensure the critical resources of the community are maximized, while the critiques themselves are constrained by a requirement of being “relevant to their cognitive and practical aims” (Longino 2002, 133).<sup>318</sup> Longino’s third criterion recognizes the diversity of practical experience and knowledge as an epistemic resource within the community. Like the hypotheses or assumptions that are the subject of critical scrutiny on Longino’s account, this norm suggests that the analogies associated with a scientific metaphor could be subjected to “the broadest range of criticism” given tempered equality of intellectual authority (Longino 2002, 132).

Consider Darwin’s theory of natural selection, which depended upon evidence from various disciplines. Darwin collected information from a variety of cognitive sources, which included research by scientists, such as geologists and botanists, as well as animal husbandry, such as private breeders and hobbyists. Bert James Loewenberg notes “Darwin’s data was actually overwhelming. The *Origin of Species* was crammed with data and the evidence [...] The facts, often new and rare, were garnered from innumerable sources and covered virtually the entire range of natural history” (Loewenberg 1965, 10).

Natural philosophers, who brought to bear their radically different expertise on natural selection, published robust critical reviews of Darwin’s theory. For example, Adam Sedgwick and George Lyell specialized in geology, Richard Owen comparative anatomy, and Henry Fawcett was a political philosopher. As David Hull notes, “there seemed to be something about a classical education in mathematics that predisposed a person to find evolutionary theory decidedly unsatisfactory” (Hull 1973, 273). Hull includes reviews from

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<sup>317</sup> According to Longino, cognitive authority refers to the knowledge one has about a particular area or problem (Longino 2002, 133 *fn* 19). For example, one might be “cognitively authoritative with respect to matters in astrophysics but ignorant of cell biology” (Longino 2002, 133 *fn* 19). Intellectual authority refers to having skills that provides the competence to determine who has knowledge and who does not (Longino 2002, 133 *fn* 19).

<sup>318</sup> Longino writes that the critical assessment in knowledge-producing communities should be based on the “unforced assent to the substantive and logical principles used in them, rather than by properties, such as social or economic power, of those who are propounding [those hypotheses]” (Longino 2002, 132).

mathematicians, William Hopkins and his student Lord Kelvin, who gave negative reviews of Darwin's theory. The diversity capable of generating a robust critique of the assumptions associated with Darwin's metaphor is not a diversity of lived experience or perspectival diversity. It is a disciplinary, epistemic diversity that comes from practical experience with the phenomenon under investigation; a community cannot assume that the diversely situated experience/knowledge of its members will give rise to a critical standpoint on knowledge production.. This is made clear by contrasting the critiques of Darwin's theory with the feeble critical discourse of the research conducted by Morton and that of BOR.

The community that was the primary audience for Morton's research was not comprised of epistemically diverse members like that which was capable of generating a robust critique of the *Origin*. In fact, the two major theories guiding research on human difference in the 19th century assumed an intellectual superiority of Caucasians. Morton's theory of polygenesis was a fundamental assumption of the American School of Anthropology. According to Morton's theory, humans originated in different areas around the world, which gave rise to physical and behavioral differences amongst the people of different races. Morton's *race as a distinct species* metaphor implied a hierarchy used to justify social policies like slavery and Indian Removal (Baker 2008, 93).<sup>319</sup> Its rival theory, monogenesis, which implied a common human ancestry, also supported a racial hierarchy that assumed that Caucasians were the most evolved and thus the best suited for rule (Hume 2008, 141-142).<sup>320</sup> In fact with regard to mixed races, "both polygenists and monogenists tended to agree that mulattos or 'mixed bloods' were superior in intellect to, for example,

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<sup>319</sup> Baker notes that Morton "did not hesitate to provide evidence of Negro 'inferiority' to John C. Calhoun, the prominent proslavery secretary of state, to help him negotiate the annexation of Texas as a slave state" (Baker 2008, 94).

<sup>320</sup> According to Hume (2008) even after the evolution debate [...] many researchers saw the existing human races as products of natural selection having caused them to branch far back in the distant past. Some schemes even depicted Negroes as having branched off first" (Hume 2008, 144). Morton's rival, the Rev. John Bachman, objected to Morton's views on hybridity and polygenism. Yet, he did not "challenge slavery or the existing hierarchy of American society" (Hume 2008, 142).

‘pure’ Negroes or Indians” (Hume 2008, 141). Hume notes that the debates between Morton and Bachman “took place in the context of the crisis over slavery” (Hume 2008, 123). Whether members were polygenist or monogenist, the larger scientific community on human difference exhibited hyper-homogeneity with respect to the treatment of non-white categories. The community was not epistemically diverse and even though there were competing theories, both assumed a hierarchy of race.<sup>321</sup> The community was not in a position to provide a robust challenge to the assumptions grounding the *race as distinct species* metaphor, and the theory of hard heredity it endorsed.<sup>322</sup>

The BOR community does not fare any better than Morton’s; it too lacks the relevant cognitive diversity capable of generating a robust critique of its *prenatal hormone as brain organizer* metaphor. Longino’s condition of tempered equality requires that marginalized members should be included in the critical discourse insofar as they have the intellectual capacity and relevant expertise. This implies that the community is responsible for ensuring equal participation of those in the community without considering whether a member is

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<sup>321</sup> Notice how the race of 19<sup>th</sup> century researchers is intimately tied to their hands-on experience with the phenomena (questions of human difference). This gives the appearance that the relevant diversity required is perspectival and not cognitive. I suggest later that Douglass’ critique is not relevant solely because of his socially situated knowledge of being a black man, but that his experience of the different abilities of non-whites (which is relevant to questions of human intelligence) affords him cognitive diversity not found in Morton’s scientific community.

<sup>322</sup> The works of Morton and his students, Josiah Nott and George Gliddon, affected the research of Pierre Paul Broca, who founded the French anthropological society (Hume 2008, 148). Morton’s research reached England via his friendship with Francis Galton (Hume 2008, 146). Although the British equivalent of a monogenist, Galton appreciated some of the features of polygenism, such as

the relative worth of races, the importance of maintaining superior stocks, the importance of and difficulties associated with civilization, the association with particular characters and types that made up peoples and races, and the importance of finding methods of quantifying those hereditary qualities.  
(Hume 2008, 149)

Galton’s works *Hereditary Genius* (1869) and *Natural Inheritance* (1889) contained his theories of intelligence, which asserted the lack of “capacity that savage and barbarous races had” for becoming civilized (Hume 2008, 149). The hard heredity implied by Morton’s polygenism, implied a hard heredity of traits, also found support in Mendelism. According to Hume, the success of Mendelism was a “polygenist victory in two ways” (Hume 2008, 152). First, hard heredity connected traits to chromosomal units and not environment (Hume 2008, 152). Second, “many of the early supporters married the impoverished polygenist world view of race war and civilization to the eugenicists who further worried about race suicide” (Hume 2008, 152). Needless to say, Morton’s research had a profound effect on future science, and as we saw in the earlier discussion of Stepan, the science of polygenism had a profound effect on social policies.

popular, well respected, or has an academic pedigree. Such factors should not influence who should be allowed to enter into the critical discourse of the community. Yet, Longino's account gives little direction for enlisting novel sources of epistemic diversity when the community lacks it.

Like Morton's critical community, members of the BOR community share a fundamental assumption, which negatively affects the epistemic diversity of the community. According to Jordan-Young, the discipline known as BOR begins with John Money's research on David Reimer (John/Joan) who was born male in 1965, but was reassigned female after an accident that permanently damaged his genitals (Jordan-Young 2010, 5). Money believed that Reimer's young age, which was under 2 years at the time of the incident, would enable Reimer to be raised as a girl without problem (Jordan-Young 2010, 6). Given his experience with the Reimer case, Money began applying brain organization theory to humans in the 1960's.<sup>323</sup> Jordan-Young writes that Money was "one of the most important and prolific researchers" on the effects of hormones on human behavior, (Jordan-Young 2010, 6).

Money and his collaborator, Anke Ehrhardt, (and her later collaborator, Heino Meyer-Bahlburg) endorse what they call an interactionist account of BOR (Jordan-Young 2010, 7). This fundamental assumption is key to understanding the epistemic homogeneity of BOR. Interactionists, generally speaking, believe "in the importance of both biology and the rearing environment" as having a role in the sexual development of humans (Jordan-Young 2010, 7). Although the linear model created by Money and Ehrhardt should recognize the role of biology and environment factors, it downplays the role of environment and emphasizes the role of hormones. As Jordan-Young notes, the linear model "set the stage for decades of research on hormones and sexuality," yet it did not include "how physical and

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<sup>323</sup> Tragically, Reimer's case was played out in the public arena. Not only did his story appear in an extensive amount of medical literature, it became a BBC documentary, a *Rolling Stone* article, a popular book, and "a special science documentary for the PBS science series *NOVA*" (Jordan-Young 2010, 6). He committed suicide at the age of 38.

social variables actually *work in tandem*” (Jordan-Young 2010, 8).<sup>324</sup> The community’s commitment to an impoverished account of interactionism reflects the epistemic homogeneity of the BOR community.

Describing the BOR community, Jordan-Young notes that “opposing ‘interactionism’ is about as popular as being against ‘freedom,’ so brain organization researchers all call themselves interactionists, though they actually continue to employ the same model”—a model that downplays the potential effects of environment on human sexual behavior (Jordan-Young 2010, 8). When members call themselves interactionists, it gives the appearance of theoretical diversity, but in fact they do not consider alternative accounts. Because the community as a whole shares this fundamental assumption, it is not capable of generating robust critique. As support, Jordan-Young points out, “most brain organization studies do not include social variables in their models at all, let alone explore potential interactions between these and the hormones that they see as the main actors” (Jordan-Young 2010, 8).

Downplaying the social factors that might explain human sexual behavior is particularly problematic, and it is made even more problematic because the model endorsed by Money and Ehrhardt contained “old-fashioned” notions of masculine and feminine sexual behavior (Jordan-Young 2010, 35). In other words, the model functioned as a filter that resulted only in explanations linked to prenatal hormones that corresponded to outdated descriptions of gendered, sexual behavior. Responding to revised understandings, the community switched from the linear model to the orthogonal model in the 1980’s. Even so, the studies still reflected a key assumption of the metaphor introduced by Money, Ehrhardt, and colleagues: “sexual object choice has been the clear organizing principle of masculine

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<sup>324</sup> Jordan-Young cites the description given to BOR by Doell and Longino (1988). Doell and Longino described the linear model as a “black box with prenatal hormonal input and later behavioral output” (Doell and Longino 1988, 59). See Jordan-Young (2010), p. 8.

and feminine sexuality” and sexual object choice was influenced by exposure to prenatal hormones (Jordan-Young 2010, 131).

Jordan-Young points out that “the standard practice for brain organization studies [has been] to structure information on sexual arousal and behaviors” (Jordan-Young 2010, 131). The community’s uncritical adoption of the putative interactionist models provided by Money and Ehrhardt seems to be a case of privileging the research of those who initiated the discipline. Its citation and publication approximate Bartha’s description of a community’s drive to maximize the overall similarity between source and target. In addition, the influence of Money and Ehrhardt’s research has not only dominated the explanatory reasoning of the discipline, but it also potentially tainted the data. Jordan-Young notes that parents of children with intersex children are aware of the theories in BOR and have often consulted books or other materials on the subject authored by Money and Ehrhardt.<sup>325</sup> When the same parents are asked to provide data for research studies through questionnaires, their responses presuppose the theory.<sup>326</sup>

Beginning with BOR’s first model of masculinity/femininity, the community did not meet Longino’s condition of tempered equality of intellectual authority. In addition, members endorse an impoverished account of interactionism, which gave the appearance of considering alternative, environmental explanations, but actually prevented them from attending to relevant external critiques. Jordan-Young’s analysis of BOR literature shows that the community failed to consider external critique once the first model’s assumption of interactionism permeated the discipline.

The BOR community and Morton’s community in the 19th century entertained fundamental assumptions associated with the metaphor, and these assumptions were shared

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<sup>325</sup> Most notable is Money and Ehrhardt’s popular 1972 book, *Man and Women, Boy and Girl*. Sometimes, parents became aware of the information independently; others were made aware of resources by their medical practitioner (Jordan-young 2010, 248).

<sup>326</sup> Also part of the clinical practice was to inform parents of what to expect with regard to their intersex child’s behavior (Jordan-Young 2010, 248).

by most, if not all, of each community's members. Although Longino's social norms are intended to mobilize the diversity that already exists within a community, she fails to distinguish between different types of diversity and she neglects to consider what to do when a community lacks the relevant epistemic diversity capable of generating a robust critique of assumptions connected with scientific metaphors. In cases where the community in question lacks epistemic diversity, it may be necessary to invite critique from outside of the discipline. Of course, that is not to say that the community should make itself vulnerable to all forms of external critique. Longino's last condition will be useful in creating an environment that allows for a constrained external critique of a community's metaphors.

Longino's final condition for transformative criticism requires that communities have a set of "publicly recognized standards" that enable members of the community to non-arbitrarily evaluate "theories, hypotheses, and observational practices" (Longino 2002, 130). Members can refer to these standards for evaluation when they participate in critical discourse, and when they do, this ensures their criticism is "relevant to the goals of the inquiring community" (Longino 2002, 130). In addition, Longino intends not only for this norm to require communities to have publicly recognized standards for evaluating the integrity of their procedures, but that it also provides for standards for determining the adequacy of the community's critical discourse (Longino 2002, 130).

The task of assessing the adequacy of a community's critical discourse includes not only that of meeting Longino's aforementioned conditions for objectivity, but also some additional meta-level conditions, such as honest and transparent interactions between the critic and advocate.<sup>327</sup> In the absence of epistemically relevant critiques from within the community, the standards should include some guidance for the creation of epistemic

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<sup>327</sup> The first two meta-level conditions are ones identified and discussed previously, and are implied by the conditions given by Paul Bartha (2010) and Helen Longino (2002) respectively. I will now explore the last condition demanding the potential for novel critique via a trading zone.

“trading zones,” which would encourage internal-external critical discourse. Although I am borrowing this term from Peter Galison (1995, 1996, and 1997), who posited intellectual trading zones as points of communication between different sub-communities of a discipline, I understand trading zones as spaces where the internal-external transfer of critical practical experience associated with the phenomena and/or the correspondences stipulated by the metaphor occurs.

The communities engaged in 19th century research on human difference and contemporary BOR both lack the necessary epistemic diversity among its members to mobilize a robust critique of the assumptions that underpin the discipline’s metaphors. Unlike Darwin’s community, where members differed on key assumptions of the metaphor, Morton’s community and the BOR community could not muster a critique amongst its members. These are conditions that call for critical input from external sources.<sup>328</sup> In fact, both Morton’s community and the BOR community had opportunities for the uptake of external critique, but neither recognized legitimate external sources, even when these directly challenged assumptions articulated in the community’s metaphor.

As we saw in Chapter Two, Darwin had the good fortune to pen the *Origin* in the context of explicit debate about scientific methodology.<sup>329</sup> Darwin’s theory was compared to conceptions of good science by the respected minds of the day, which indicates a key point: Darwin’s theory was evaluated against a backdrop of the debates of what was the normative account of induction. The critical practice of Darwin’s community exemplifies Longino’s final condition insofar as it required public articulation of and critical engagement with the assumptions underwriting the metaphor. With regard to my refined condition for a set of standards of applicable to external critical discourse, it is debatable whether the community

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<sup>328</sup> I want to thank fellow graduate student, Jon Rosenberg, for his assistance in developing this point.

<sup>329</sup> In fact, many of his reviewers, both positive and negative, attempt to demonstrate Darwin’s adherence to the standards set down by John Herschel, John Stuart Mill, and William Whewell, see Hull (1973, 291).

permitted critique from outside of the community. As we have seen, Darwin's community met the conditions that require venues and the serious uptake of criticism. Although it is doubtful that the membership of the local community was diverse as regards their race and gender, epistemically diverse individuals existing outside of the community were allowed, and in some cases encouraged by Darwin himself, to participate in the critical discourse.

Although Darwin's community appears to be homogeneous, at least with regards to race and gender, it was able to produce a robust critique. In addition, not every participant was an insider to the community; Darwin relied on the reports of breeders, the correspondence with women interested in natural philosophy, and his co-author, Alfred Russel Wallace, who was initially thought of as an outsider in light of his socio-economic status. Darwin's community was able to generate a robust critique because the members were epistemically diverse. Natural philosophers who happened to be theologians or perhaps differed with respect to their ideas of what constituted good science were in the best position to provide criticism. From these peers, Darwin received some of the toughest objections to the assumptions of his metaphor.

Darwin's community differed from the cases of 19<sup>th</sup> century research on human difference and contemporary BOR because its members had the relevant diversity (a diversity about the fundamental assumptions about method, God, and evolution) to generate a robust critique the analogy.<sup>330</sup> Darwin's community, as an exemplar of robust critique, can offer some guidance for cases where a community is so cognitively homogeneous that it fails to generate a robust critique of its metaphors. It can also help to locate the cognitive trading

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<sup>330</sup> I am persuaded of this given how long it took for Darwin's theory to gain acceptance. I realize that I am dealing in hypotheticals, but I cannot help but wonder whether his theory would have gained acceptance more quickly if the community had been less homogeneous, if it had not been comprised of wealthy, landed gentlemen, who were members of the Church of England. As it was, Darwin's community did experience some diversity, which allowed for the critique of his analogy, and it turns out that this was enough for a somewhat robust critique. It is telling that those able to levy critiques at Darwin's theory were the same gentlemen (natural philosophers) that were predominantly theologians.

I would like to examine this particular point in a separate paper that offers an analysis of the intellectual trading zones of Darwin's community.

zones that make available relevant, novel critiques from epistemically diverse individuals or communities without making the community vulnerable to every criticism under the sun.

Because the communities of 19<sup>th</sup> century research on human difference and BOR failed to recognize the legitimacy of potential critique from external sources, I suggest amending Longino's conditions for transformative criticism so that communities make room for the introduction of relevant, external criticism. I draw on Galison's intellectual trading zones to describe the idealized space that communities need in order to import external critique into their critical discourse. Being receptive to the creation of trading zones would introduce an opportunity for novel critique in epistemically homogeneous communities, and it would ensure that outsiders with relevant experience would have a chance to be heard. While my refinement directly applies to Longino's condition for recognizing epistemic authorities within a community, it also has implications for the condition that requires shared public standards: external critiques may signal a need to subject the shared public standards to critical assessment. In such cases, those external to the community who can provide relevant critiques should receive uptake. Let's take a look at how this might play out in Morton's 19<sup>th</sup> century community involving human difference research and the community for BOR.

Had the members of Morton's community attended to the criticism submitted by Frederick Douglass, they would have had to justify the assumptions of racial difference associated with the metaphor that informed their research program. Recall that Douglass directly challenged the insistence of the community, with a counter-example of an intelligent, non-white person. Other examples, such as mixed-race families with viable offspring, could pose a direct challenge to the claims of Morton's polygenism, and if Morton were to respond as he did previously, by suggesting that scientists needed to revise their definition of species, he may have brought his reputation for objectivity into question by others who would insist

on standards of justification. The assumptions of the metaphor could also face external criticism concerning the link between intelligence and bone structure/cranial capacity. Recall Gould's objection that Morton never provided a justification for linking cranial capacity and intelligence, or even why a larger skull would indicate higher intelligence (Gould 1978, 508). What made the external critique relevant is demonstrated by Douglass' criticism; he had practical experience with the phenomenon that challenged the fundamental assumption of a hierarchy of race. In fact, Douglass' writings and speeches, in spite of a lack of public education, attest to his intelligence!

If we examine the case of BOR, we find that there are researchers in other disciplines whose research could be brought to bear on the assumptions of the metaphor, *prenatal hormones as a brain organizer*. Researchers focusing on the various contextual factors that influence the sexual behavior in women with CAH are the best resources for calling into question the assumptions that support gendered hormones as the only catalyst for sexed behavior. Given the wholesale endorsement of interactionism by the BOR community, its members should have been willing to consider alternative explanations, which suggest that both hormones and environment play a role in human sexual behavior. In addition, critical discourse of the flavor of interactionism that guides BOR would also do a great service toward challenging the assumption that only hormones can explain human sexual behavior. As we have seen, however, the BOR community ignores the environmental influences, focusing exclusively on the putative influence of prenatal hormones. Had the community been willing to consider external sources of relevant criticism, it may have been able to break the cycle of maximizing similarity and recognize the shortcomings of its metaphor.

Not every scientific community will have the relevant epistemic diversity to mobilize a robust critique of their metaphors and analogies.<sup>331</sup> Thus, I would add to Longino's norms a condition that requires scientific communities to be willing to consider relevant, external critique. Once a research community is receptive to the idea of external critique, it will be necessary to state a method for determining relevant, critical participation. After all, the community need not entertain all external criticism. Thus, I propose communities adopt a method of assessing the relevance of external critique that is similar to the constraints for the selection of a source analogue mentioned by Holyoak and Thagard in Chapter One. A community could limit their interactions with outsiders by posing constraints on the type of critique allowed. For example, communities might require outsiders to have relevant practical experience with the phenomena under investigation, to include the putative content of the positive, negative, and neutral analogies that comprise the metaphor. By allowing for the possibility of external critique, communities become receptive to criticism from outside the discipline, while at the same time they need not consider every criticism directed at the assumptions that underpin their metaphors.<sup>332</sup> Douglass' critique of Morton's research on human difference and the research of other disciplines that offer alternative explanations of human sexual behavior are examples where the experience with the phenomena was particularly relevant. Morton and the community adopted an elitist stance toward Douglass and completely ignored his objections; Douglass was after all not only an outsider to anthropology, but he was also part of the 'phenomena' under study. Had the community been receptive to external critique—even if not in the case of Douglass—there were abolitionists who could have pointed out the intellectual abilities of people from different races, as well as attest to the viability of mixed race offspring.

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<sup>331</sup> I think that a lack of critique of the metaphor within a community may be an indication that a community requires external assistance, and communities may be able to identify this if they ensure the venues needed for critique. Also, they may publish their initial commitments, such as the interactionism held by almost all members of BOR.

<sup>332</sup> Of course, a community, if it believes the external critique to be of value, need not ignore the criticism.

In the case of the BOR community, it seems that external critique could come from various avenues of practical experience with the phenomenon. For example, researchers in other disciplines who rely on interactionism could point out that BOR relies solely on hormonal explanations and excludes any explanatory appeal to environment. While the community's implicit switch from one model to the other that is absent from the literature may not have been noticed by individuals external to the discipline, it was noted by Jordan-Young. This brings up a key point. Some external critique may not be aimed directly at the phenomenon or the sets of similarities and differences supporting the metaphor. Instead, it may be that outsiders like philosophers, science technology studies scholars, historians, and critical theorists are best suited to present external critique of the methods, values, and assumptions of the community. So long as the critiques can be shown to be relevant to the community's research, the community should seriously consider it.

Longino's conditions for transformative criticism are intended to ensure "that theories and hypotheses accepted in the community will not incorporate the idiosyncratic biases (heuristic or social) of an individual or group" (Longino 2002, 134). As we can see in the table below, due to the homogeneity of the communities of 19th century research on human difference and BOR, both failed to meet a good share of Longino's conditions. In contrast, Darwin's community of natural philosophers met the conditions for standards involving critical discourse in the community. Had Darwin's community been less epistemically diverse, the results of his research would not have been subjected to a robust critique and he would not have had the confidence in the truth of his theory that he did.

	19 <sup>th</sup> Century Race (Morton)	BOR	Darwin's Natural Philosophy
Venues	Yes, but the epistemic homogeneity prevented critique.	No, epistemic homogeneity hindered the creation of venues.	Yes- full of critique.
Uptake	No	Non-starter	Yes, esp. in newer additions
Tempered Equality	No!	No!	Yes, to a degree. Darwin recognized the importance of non-scientific contributions to the discipline.
Standards	Some standards for methodology, but not in the case of critiquing the model/metaphor.	The omission of context from explanations of sexual behavior prevents external critique.	Yes, esp. as regards the assessment of metaphor and analogy. In fact, no external critique was necessary.

Table 3. Longino's Account of Transformative Criticism Within Three Scientific Communities (19th Century Race Science, BOR, and Darwin's Natural Philosophy)

The responsibilities of the advocate and critic as outlined by Bartha's rhetorical device are furthered by my refinement of Longino's account. The robust scrutiny of the metaphor puts a community in the best position to realize transformative criticism. The community must be receptive to criticism, to the point of seeking out dissent when initially there is none and ensuring legitimate space for such criticism (Longino 2002, 132). Criticism must be relevant and consistent with the standards adopted by the community (Longino 2002, 133). Most important, the community must be diverse with respect to values and background knowledge in order to provide the robust scrutiny needed to avoid an ingrained analogy. And the norms of practice in place within the community should ensure that diversity is not only encouraged, but also actively sought out through the creation of trading zones that foster relevant critique. Longino states

Insofar as a community satisfies these conditions, that is, insofar as a community maintains a means of disseminating and responding

to criticism, and is constituted of members who hold themselves answerable individually and collectively to a set of standards and reach consensus as a result of discursive interactions including all relevant perspectives and uninhibited by political power, it will qualify as a knowledge productive community.

(Longino 2002, 134)

To the degree that a scientific metaphor is the product of a given community, the four cognitive social norms proposed by Longino set out broad conditions for ensuring the possibility of social critique. Her conditions of transformative criticism can be used to proactively construct communities in such a way as to promote critical discourse.<sup>333</sup> But is that enough to address the implicit influence of an ingrained analogy? As we have seen, strategies based on Longino's conditions for transformative criticism may not be adequate on their own to ensure that a research community will overcome the influence of an ingrained analogy. Thus, I amend Longino's account to include the creation of zones for the presentation and uptake of relevant critique based on epistemic and practical diversity.

Longino's account warrants a special type of critique, one where diverse members of a population bring to bear different experiences and background knowledge that can mobilize the kinds of robust criticism needed to expose and counteract the influence of an ingrained analogy. Social epistemologists' appeals to diversity have been used to support arguments to include marginalized groups, those who were historically left out of the discourse about knowledge production. For example, diversity might be defined as "the inclusion of the voices, experiences, perspectives, questions, interests, and social location of those groups who have been traditionally marginalised in the institutions of knowledge production and dissemination" (Robertson 2013, 300). I appeal to a broader conception of diversity; it is epistemic diversity, which may (or may not) arise from a diversity of background experience

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<sup>333</sup> According to Longino, the determination of whether a community is knowledge productive "is a matter of degree," communities may be dubbed "more or less dependable and effective as producers of knowledge" (Longino 2002, 134). What I claim is that communities should adopt Longino's criteria amended to include relevant external critique in order to be in the position of limiting the possibility of an ingrained analogy.

and knowledge. This type of diversity is often conflated with race, gender, ethnicity, etc., but it need not be. The type of diversity needed is that which provokes the constant questioning of the assumed similarities and differences creating the tension that keeps the metaphor alive and prevents it from becoming commonplace. A community with a diversity of practical experience amongst its members contributes to the background knowledge needed to render critique, supporting the duties attributed to the advocate and critic. This critique needs to occur whether the community is aware that those analogies are ingrained or not.

Admittedly, the adoption of strategies aimed at reducing the negative impact of analogical reasoning on scientific practice comes at a cost. Scientists may resist my demand for transparency regarding their use of analogical reasoning as well as the inclusion of criticism from those external to their discipline. Costs, in the form of time and expense, are legitimate concerns for researchers. Although I do not intend to pose a hardship on those conducting research, I believe that when scientists rely on analogical reasoning, they assume a burden to ensure that their use of such reasoning is done so as to be not only epistemically, but socially responsible. The cases of scientific practice in this dissertation demonstrate that lives can be negatively affected by social policies informed by misleading science. When scientists reason from metaphors and rely on argument for analogy, they must be made aware of the potential for their personal (or even society's) assumptions to implicitly affect their research, and indirectly the lives of others.<sup>334</sup>

To offset the financial costs and the costs of incorporating external critique into scientific practice, I suggest funding agencies make available funds and training for research that includes the use of analogical reasoning. In the same way that the National Science Foundation requires some disciplines, like Nanotechnology, to undergo training on values in

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<sup>334</sup> Although I do not have specific numbers, I appeal to the number of Native Americans and African Slaves who died as a result of Indian Removal and African Slavery as support. I also then ask the reader to consider the impact of those policies upon the groups still living in the United States and the lasting effects of those policies.

order to receive funding, agencies could support the creation of trading zones and venues for critical discourse. I recognize that the costs of making science more socially responsible are significant. I have listed only a few of the ways of making the adoption of my strategies more palatable for researchers.

#### **§5.4 A Return to the Darwin and the *Origin***

The scientific community that witnessed the publication of the *Origin* provided a robust scrutiny necessary to challenge Darwin's use of analogy, and as Darwin admits, this criticism prompted him to be more explicit and have more confidence in his theory. Differences believed to be relevant included the time required for both nature and man to create species, the lack of species produced by artificial selection, and even the question of agency (of selecting for a trait) not found in nature. These differences, as well as others, provided the necessary tension to challenge the metaphor and yet, the theory persevered. Not only did Darwin gain more confidence in his theory, but he was also very aware that his methods were under careful scrutiny since the scientific community included philosophers.<sup>335</sup> It does not matter if Darwin's analogy is now believed to be generally correct. At the time, like all periods of scientific practice, there was no indication of which metaphors are detrimental epistemically or socially. The redeeming feature of an account of analogy will be if it can test every metaphor and limit its implicit influence on a community. Communities that utilize strategies consistent with Longino's amended account of the conditions necessary to realize transformative criticism will have the resources to either exploit the epistemic diversity of its members or, when the community lacks this diversity, it will be receptive to relevant critiques introduced through the existence of trading zones.

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<sup>335</sup> Part of the Ox-Bridge training was in the classics. Unique to this time period was the close relation between philosophy and science.

## Conclusion

In his 1934 Herbert Spencer Speech, Albert Einstein advised the members of the audience who wanted to learn about the methods of physicists to “stick closely to one principle: don’t listen to [the scientists’] words, fix your attention on their deeds” (Einstein 1934, 163).<sup>336</sup> The reason, according to Einstein, was not that scientists would lie about their work; rather, Einstein noted that scientists’ accounts of research are colored by their aims and aspirations, and include references to theoretical assumptions they take to be real.

Although Einstein’s advice was intended for those interested in the methods of physics, his principle seems apt for studies of all kinds of science, and it seems especially fitting for the disciplines that rely on metaphors and analogies as justification for their theories. Recall that Kevin Dunbar (2008) and Karen Knorr-Cetina (1983) find that scientists either do not remember their reliance on analogical reasoning or they do not include it in their descriptions of practice. Indeed, as Nancy Nersessian points out, the accounts given by scientists demonstrate an “overemphasis on the *product* and [a] relative neglect of the *process* of scientific investigation” (Nersessian 1988, 41). Even when the focus is strictly on practice, Doren Recker claims you will find a “pervasive use of metaphor within legitimate science [that] is so badly recognized and understood” (Recker 2004, 217). Given that the use of analogical reasoning is ubiquitous in as well as indispensable to science, scientific communities should not abandon their reliance on analogical reasoning. Yet, an uncritical

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<sup>336</sup> According to Einstein,

If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don’t listen to their words, fix your attention on their deeds. To him who is a discoverer in this field, the products of his imagination appear so necessary and natural that he regards them, and would like to have them regarded by others, not as creations of thought but as given realities.

(Einstein 1934, 163)

use of analogical reasoning that risks misleading an entire discipline is epistemically irresponsible.<sup>337</sup>

Shortly after the publication of the *Origin*, Darwin admitted his frustration with the critiques of his theory in a letter to Thomas Henry Huxley,

I have got fairly sick of hostile reviews. Nevertheless, they have been of use in showing me when to expatiate a little and to introduce a few new discussions... I entirely agree with you, that the difficulties on my notions are terrific, yet having seen what the Reviews have said against me, I have far more confidence in the *general* truth of the doctrine than I formerly had.

(Darwin 1887, 94-95)<sup>338</sup>

As we have seen, the reviews of Darwin's theory contained critiques of the assumptions that underpinned his metaphor, and Darwin acknowledged to Huxley that the negative reviews—although difficult for him to entertain—instilled a confidence in his work. In other words, the community of philosophers, scientists, and others, mounted extensive opposition to Darwin's theory, subjecting it to a sustained, robust scrutiny and the theory was refined in response to it. The critical environment in which Darwin's theory was forged exemplifies the kind of scrutiny necessary to decrease the likelihood of an ingrained analogy.

The strategies that I propose in Chapter Five are motivated by considerations of how analogical reasoning can misdirect practice, as revealed in the analyses of two cases from science: the 19<sup>th</sup> century research on human intelligence conducted by Dr. Samuel George Morton and the contemporary research investigation of brain organization theory in

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<sup>337</sup> Recker states that not understanding how analogical reasoning works is “mischievous” (Recker 2004, 217). Yet, as I have shown, in the case of 19<sup>th</sup> century race science—which informed government policies of slavery and Indian removal—and the case of BOR—which has the potential to marginalize women in the workplace, if it has not already done so—this failure to appreciate the workings of analogical reasoning in science is not merely mischievous, but it poses very serious threat to certain segments of society.

<sup>338</sup> Darwin's letter to Thomas Henry Huxley dated December 2, 1860. See Darwin, F. (1887) pp. 94-5.

psychology. In both cases, the assumptions that underpin the dominant metaphor were ingrained (no longer understood as non-literal statements) within the minds of the scientific community. As we saw in Chapter Four, the ingrained assumptions became an integral part of the analogical inferences, subsequently affecting the design of experiments, the interpretation of data, and reducing the likelihood of the community accepting critique from those outside of the discipline. In this way, a metaphor is self-reinforcing and its ingrained assumptions are difficult to detect. Although the problem of ingrained analogy has the potential to misdirect scientific practice, it can also lead to negative consequences when social and political policies are informed by science.

Given these potential harms, scientific communities bear a responsibility to address the problem of ingrained analogy. Towards that goal, they should maximize the effectiveness of Paul Bartha's advocate-critic device, which implies exploiting the tension between the need for being explicit and economical when listing the relevant assumptions that underpin a scientific metaphor. Conditions of objectivity, like those found in Helen Longino's account of transformative criticism, advance the goal of maximizing the critical interactions between the advocate and critic. However, when communities lack sufficient epistemic diversity, Longino's conditions may not be adequate on their own to ensure that a scientific community will overcome the influence of an ingrained analogy. Longino's account assumes diversity from the outset; she expects members of a community to have different experiences and background knowledge, which could provide the robust criticism needed to impede the influence of an ingrained analogy.<sup>339</sup> If the community lacks epistemic diversity, such as when members share background knowledge and experiences, Longino's conditions for transformative criticism, as stated in *The Fate of Knowledge*, do not provide any guidance for importing the necessary novel expertise from outside of the community.

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<sup>339</sup> Although this supports incorporating people of different races and gender into scientific communities, it does not solve the problem of ingrained analogy.

To deal with the problem of ingrained analogy, scientific communities may need to appeal to a refined version of Longino's conditions, one that goes beyond her requirements for public venues that promote critique, a sincere uptake of relevant criticism, the exercise of a tempered equality of intellectual authority and public standards of evaluation (Longino 2002, 131). Communities seeking to incorporate external critique can rely on refined versions of Longino's requirements for the creation of venues and the tempered equality of intellectual authority to construct trading zones—idealized and/or tangible areas for the uptake of criticism presented by those individuals external to the discipline. For example, requiring the creation of venues, or dedicating space in existing venues, solely devoted to the critique of analogical reasoning in scientific practice. In order to restrict the type of criticism posed, the community will need to require all critique to be relevant to the issue at hand. This can be accomplished by using constraints on the criticism, such as limiting critical inputs to only those individuals who have working knowledge of the phenomena and whatever similarities/differences are posited in the analogies comprising the metaphor. By doing so, the community not only reduces the outsider's expectation of having to respond to every critique posed to the community, it also increases the likelihood of robust criticism. Individuals, who enter into the trading zone, will be granted a tempered equality of intellectual authority given the subject in which they specialize; thus, aiding the community's ability to deal with the problem of ingrained analogy. The creation of trading zones will not only encourage novel critiques from outside the discipline, it would also make the critical assessment of shared values and standards possible. For example, philosophers, science technology studies scholars, and historians of science would be granted an area of discourse in the trading zone for meta-level critiques.

Of course, the adoption of strategies, or any change to the status quo, will meet with resistance from scientific communities. Admittedly, my strategies entail not only tangible

costs, but they can also be viewed as a threat to the autonomy of science. I do not mean to downplay these concerns, but I believe that scientists, once they are aware that analogical reasoning can result in negative social and political consequences, are obliged to ensure that the community limits the ability of the metaphor to mislead. In addition, I suggest that funding agencies provide funds to cover the costs of time and research for projects that are transparent as regards the particular use of analogical reasoning in research.

In *Philosophy of Science after Feminism*, Janet Kourany called upon philosophers to “criticize and even transform science rather than conform to it” (Kourany 2012, 120). Kourany seeks a “broadening [of] our conception of scientific rationality to encompass the ethical aspects of science, by acknowledging the inextricable interconnections of the ethical and epistemic” (Kourany 2012, 120). As such, any account of analogical reasoning in science must not only assess the plausibility of individual arguments of analogy, but it must also address the problem of ingrained analogy. Future research should include investigating the reliance on analogical reasoning in other scientific disciplines in order to understand the extent to which it might mislead scientists and/or pose social or political harms.

At the level of collective community practice, one possible research question might include investigating the roles that philosophers, historians, and science technology studies (STS) scholars can play as active semi-external (insider-outsider) critics within scientific communities. Possible roles for these potential critics include:

- Creating Practical Reasoning Courses Tailored for Scientists
- Including the Tabular Representation of Analogy in Scientific Reasoning Courses
- Training Scientists in Values/Ethics (NSF Funding Requirement)
- Embedding Philosophers, Historians, and STS Scholars in Research Groups
- Developing Channels of Communication with the Public

These roles not only seek to improve the use of analogical reasoning in science, but they could change the relationship between science and the humanities. As a result of their particular expertise—which includes being well-versed in argumentation, the negotiation of practice, and training in critical analyses (to include the attention to values and social justice)—philosophers, historians, and STS scholars are particularly suited for ensuring a robust critique of the assumptions of a community’s metaphor. In addition, they may also be able to contribute to the evaluation and utilization of a community’s shared set of standards. Recall, that Darwin’s community not only provided a robust critique of his analogy between artificial and natural selection, but it also entertained critical discourse on the use of analogy.

Both science and the humanities have something to offer each other. Unlike Kourany, I do not expect philosophers to transform science. Instead, I seek to transform the relationship between science and the humanities. I envision a more collaborative production of knowledge. By drawing on the critical strengths of the humanities and the intellectual authority of science, trading zones, the locations where critical discourse takes place, could be the first step for ensuring that the overall process of producing knowledge is socially responsible.

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