Darwin, Huxley, and the Nineteenth-Century Rhetoric of Science

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Carol Berkenkotter

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Dedication

To the memory of my parents, Joyce and Tom Wright, who gave me the foundation I needed to build my career as a scholar; and to my children, Kelsey and Robert Wright, who gave me the best reasons for doing so.

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Chapter 1: Introduction

Thomas Henry Huxley, in his preface to *Darwiniana*, a collection of essays about Charles Darwin's works, says that "the assertion which I sometimes meet with nowadays, that I have 'recanted' or changed my opinions about Mr. Darwin's views, is quite unintelligible to me."[1] Perhaps the assertion would have been more intelligible if he had re-read his own book and compared it with some of his other writing. In a letter sent to Darwin on November 23, 1859, the day before the *Origin of Species* was published, Huxley told Darwin that he had just finished his advance copy the previous day. Then he went on:

> As to the first four chapters I agree thoroughly & fully with all the principles laid down in them— I think you have demonstrated a true cause for the production of species & have thrown the *onus probandi* that species did not arise in the way you suppose on your adversaries— [2]

The first four chapters of the *Origin of Species* lead up to and set forth the theory of natural selection as the primary driving force behind the process of evolution. The "true cause" Huxley mentions cannot be anything but natural selection. This paragraph, then, would seem to imply a complete acceptance of this theory.

And yet, on the same page of the preface I just quoted, Huxley expresses doubt about this theory: "I remain of the opinion expressed in the second [essay], that until selective breeding is definitely proved to give rise to varieties infertile with one another, the logical foundation of the theory of natural selection is incomplete."[3] The second

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[1] Huxley, *Darwiniana*, vi.
[2] Huxley, letter to Darwin. http://www.darwinproject.ac.uk/entry-2544
[3] Huxley, *Darwiniana*, vi.

essay is called "The Origin of Species," and is a commentary on that book. Huxley argues that "it is not absolutely proven that a group of animals, having all the characters exhibited by species in Nature, has ever been originated by selection, whether natural or artificial."[4] The distinction between natural and artificial selection is important. The *Origin* makes an analogy between artificial and natural selection. If species can be changed by artificial selection, then why not by natural selection? Modern scholars, such as Richard Richards and Peter Guildenhuys, have expressed doubt that Darwin was technically arguing by analogy. Huxley's own view of this issue, as we will see, was ambiguous. Regardless of the technical nature of Darwin's argument, however, if he could have shown that a new species could in fact arise from artificial selection, Huxley was ready to accept that they could do so by natural selection, as well.

At least, he appeared to be ready in 1860, when he wrote his essay called "The Origin of Species," and he also appeared to be ready in 1893, when he wrote the preface to *Darwiniana*. At other times, however, he appeared to accept natural selection uncritically, as he did in his letter to Darwin immediately after reading the work.

The interaction between Darwin and Huxley has been long noted and long misunderstood. Edward Poulton, in *Charles Darwin and the Theory of Natural Selection* (1896), devoted a chapter to the "Influence of Darwin upon Huxley."[5] He argued that Huxley was right in never having changed his view of natural selection, and attempted to support this claim with numerous extended quotations, including almost all of the relevant parts of *Darwiniana*. But Poulton's own evidence suggested otherwise. His

[4] Huxley, *Darwiniana*, 74.
[5] Poulton, *Darwin*, 119-143.

claim has been made even weaker by Darwin's letters that have come to light since Poulton was writing. I cannot fault him for that, but I can look further into a subject that remains of interest. And even in Poulton's day, there was evidence to show that the influence went both ways: not only did Darwin influence Huxley, but Huxley influenced Darwin.

The confusion over Huxley's view, and the idea that Darwin's bulldog must have accepted his essential idea of natural selection, continued into the twentieth century. In 1955, William Irvine wrote a biography of Darwin and Huxley, discussing their influences and comparing their ideas. He specifically discussed the views of Darwin, Lyell, Malthus, and Wallace on natural selection, but nowhere mentioned that Huxley's view was not always in line with Darwin's. In fact, he quoted a letter from Huxley to Lyell suggesting exactly the opposite: "Darwin is right about Natural Selection."[6] I am certain that he intended no misrepresentation of Huxley's view—to Irvine, that was in fact Huxley's view. But looking at the text of the entire letter, we see that Irvine left out the critical first word of the sentence: "If." Huxley was considering the possible implications of Darwin's being right: "If Darwin is right about natural selection–the discovery of this *vera causa* sets him to my mind in a different region altogether from all his predecessors–"[7] To me, there is a huge difference between saying Darwin was right and considering the possibility that he could be right.

Even when writers recognized that Huxley disagreed with Darwin on some points, they sometimes overlooked more important points. Stephen Jay Gould, in "The Episodic

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[6] Irvine, *Apes, Angels, and Victorians*, 176.
[7] Huxley, letter to Lyell. http://aleph0.clarku.edu/huxley/letters/62.html

Nature of Evolutionary Change," discussed the disagreement between Huxley and Darwin over saltation. Darwin believed that evolutionary change must happen slowly; Huxley felt that nothing about it kept it from happening quickly. Gould said Huxley believed "natural selection required no postulate about rates."[8] This comment implied that Huxley's only concern was about the rates, not about natural selection. And indeed, that was the case in the letter Gould discussed—the one Huxley sent the day before the *Origin* was published. Other documents, as Gould admits elsewhere, are "ambiguous" on this issue (a claim I will examine in more detail later), but Gould ultimately comes to the conclusion that "Huxley could oppose gradualism and still consider himself a supporter of natural selection . . . ."[9]

Darwin himself wasn't entirely consistent in presenting the value of natural selection. In the last sentence of the introduction to his *Origin*, he said, "I am convinced that Natural Selection has been the main but not exclusive means of modification."[10] The rest of the book emphasized natural selection, making only occasional mention of other forces, such as sexual selection and the use and disuse theory. Between the end of that book and the beginning of *The Descent of Man*, twelve years later, he changed his emphasis significantly. This shift may have stemmed from the influence of naturalists such as Huxley; Darwin made clear in his first paragraph that he was very much aware of them: "The greater number [of naturalists] accept the agency of natural selection; though some urge, whether with justice the future must decide, that I have greatly overrated its

[8] Gould, "Episodic," 179.
[9] Gould, *Structure*, 146.
[10] Darwin, *Origin*, 15.

importance."[11] The use and disuse theory was emphasized more in this book than in the *Origin*, and the theory of sexual selection was developed thoroughly. Nor did Darwin limit this shift of emphasis only to *The Decent of Man*. From what I can tell, each successive edition of the *Origin*, starting with the third, spent more and more time discussing mechanisms other than natural selection. In all of the editions, Darwin was firm in his support of evolution, but he became less firm in his support of natural selection as the mechanism for it.

The distinction between these two ideas—evolution and natural selection—is critical. Darwin was nowhere near the first to argue that species evolve. Indeed, the possibility had probably been considered by every major naturalist of Darwin's time. Starting with the third edition of the *Origin*, Darwin included a preface that listed more than twenty people before him who had set forth some sort of evolutionary theory. In arguing so well that species evolve, Darwin deserves credit for rhetorical skill, not scientific novelty. Before Darwin, relatively few people accepted evolution; after Darwin, almost all naturalists and much of the general public did. He was persuasive, but not original.

In setting forth his theory of natural selection, explaining how evolution happens, Darwin was more original but less persuasive. I don't mean to imply that he was the first to discuss natural selection, however, as he himself admits in the preface to his later editions:

> In 1831 Mr Patrick Matthew published his work on 'Naval Timber and Arboriculture,' in which he gives precisely the same view on the origin of species as that (presently to be alluded to) propounded by Mr Wallace and

[11] Darwin, *Descent*, 2.

> myself in the 'Linnean journal,' and as that enlarged in the present volume. Unfortunately the view was given by Mr Matthew very briefly in scattered passages in an Appendix to a work on a different subject, so that it remained unnoticed until Mr Matthew himself drew attention to it in the 'Gardener's Chronicle,' on April 7th, 1860. . . . He clearly saw, however, the full force of the principle of natural selection.[12]

Matthew, Alfred Russel Wallace, and Darwin all came up with this idea independently. Only Darwin compiled the evidence for it to be taken seriously as a scientific theory. Simply presenting the theory wasn't enough. Darwin and Wallace did so in papers for the Linnean Society in 1858,[13] and it was largely ignored; J.W.T. Moody correctly refers to it as "an historical 'non-event.'"[14] Once Darwin expanded the idea to the point where he could write the *Origin*, the reception was dramatically different, and the book sold out on its day of publication.[15]

Huxley was hugely influential in helping Darwin set forth his ideas on evolution, natural selection, and other aspects of natural knowledge. But given the complexity of the interactions between the two, we cannot view this help entirely within Huxley's role as Darwin's bulldog. To be sure, he supported Darwin's views, but he was also critical of them, and it is through this criticism that he was most helpful. He forced Darwin to think carefully about natural knowledge and to modify his ideas. Some of these modifications were to Darwin's advantage. Some of them, arguably, were not—Darwin's erroneous

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[12] Darwin, *Origin*, xiv. This quotation is from the third edition. Unless otherwise noted, all quotations from the *Origin* are from the first edition.

[13] Darwin, "Tendency of Species," and Wallace, "Tendency of Varieties." http://wallacefund.info/content/1858-darwin-wallace-paper

[14] Moody, "Reading of the Darwin and Wallace Papers," 474.

[15] This common statement might be misleading. All of the publisher's copies were sold to booksellers on the first day, but it took a bit longer for the books to find their ways into readers' hands. Also, the printing consisted of only 1250 copies, which wasn't a huge number even at the time.

idea of "pangenesis" comes to mind. Regardless, Darwin's theories were significantly different because of Huxley's influence. This dissertation will show how.

Goals

In this dissertation, I hope to offer compelling evidence for four major claims:

1. Although the rhetoric of science did not exist as a unified discipline until the late twentieth century, scholars in the early nineteenth century asked many of the same kinds of questions, and came up with many of the same answers. In other words, a rhetoric of science was available to those who wished to study it.
2. Although Huxley has variously been presented as advocating for Darwin's ideas and as misrepresenting Darwin's ideas altogether, no analysis of their interactions can be accurate without seeing how both men's views changed over time. The question is not merely how Darwin influenced Huxley; it is how Huxley influenced Darwin, and sometimes how Darwin's revised views influenced Huxley yet again.
3. Understanding the nineteenth-century rhetoric of science helps us understand the interactions between Darwin and Huxley, both of whom knew much more about theories of argumentation than most of their modern counterparts.
4. Because Darwin and Huxley were familiar with specific theories of scientific argumentation, rhetoricians and philosophers of argumentation actively influenced them. Even without a named discipline of the rhetoric of science, the concepts were more powerful then than we might expect them to be now.

In arguing for these three points, I will attempt, whenever possible, to understand the participants in their own terms. Understanding my approach to these goals may take some background knowledge, which I present here before moving on to my detailed methods in Chapter 2.

In his introduction to *Landmark Essays on Rhetoric of Science*, Randy Allen Harris claimed, "Until very recently . . . almost nobody felt that the subject matter of rhetorical criticism, those particular cases, could come from the sciences."[16] He credited Thomas Kuhn's *Structure of Scientific Revolutions* (1962) with being the catalyst for change. To be sure, he conceded that the rhetoric of science "goes back as long as the two fields have existed," but he viewed most of this history as "fragmented" and "mangled."[17] The implication is strong that no one did much with the rhetoric of science until after Kuhn.

We see a similar implication in *Starring the Text: The Place of Rhetoric in Science Studies*, by Alan Gross. In the "first generation" of rhetoric of science Gross identified, John Angus Campbell had been publishing in the field for the longest.[18] Campbell's work, like the rhetoric of science Harris discussed, came after Kuhn. Judy Segal, who placed her discipline of the rhetoric of medicine within the rhetoric of science, joined Harris and Gross in emphasizing the development of the both fields after Kuhn.[19]

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[16] Harris, *Landmark Essays*, xiii.
[17] Ibid., xii.
[18] Gross, *Starring the Text*, 5.
[19] Segal, *Rhetoric of Medicine*, 12-13.

Harris was correct that the rhetoric of science has been around for as long as rhetoric and science have. If anything, he understated the case. Edward Schiappa has argued convincingly that Plato was the first to use the term "*rhêtorikê*."[20] It was in *Gorgias* that Plato first used this term, and the title character used doctor-patient relations as an example of the use of rhetoric. More specifically, Gorgias talked about how patients are persuaded to undergo treatments when they are reluctant to do so. This remains a research focus in today's rhetoric of medicine. The rhetoric of medicine has been discussed for as long as the word "rhetoric" has been used.

I am not arguing that the discussion of science or medicine within the rhetorical tradition has been continuous. In fact, I don't think we can argue that the tradition itself has been continuous. Gross, in "The Rhetorical Tradition," points out that what continuity we can find is in the pedagogical strand of the tradition, not the intellectual strand,[21] and it is the intellectual strand that is relevant here. But to the extent that this strand exists at all, it seems that the rhetoric of science has been a fairly consistent part of it. This does not mean that a well-defined discipline of it has been, in the sense that we now have books with titles like *The Rhetoric of Science* and organizations like the Association for the Rhetoric of Science and Technology. But rhetoricians have frequently turned to scientific texts as sources for criticism. The lack of a specific discipline may come from a failure to see scientific texts as worthy of special attention. Far from avoiding these texts because their special nature made them above rhetorical scrutiny, rhetoricians included them on the same footing as literary, political, and religious texts.

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[20] Schiappa, "*Rhêtorikê*," 457.
[21] Gross, "Rhetorical Tradition," 32.

Ironically, this mixing of different types of texts probably led to the fragmentation Harris mentioned. It is hard to identify anomalies in a tradition as spotty as rhetoric's, but I see no reason to view the early twentieth century's avoidance of scientific texts as anything other than an anomaly.

I offer this claim about the role of science in the rhetorical tradition as a mere suggestion. If it is true, it is far too broad to support in a single dissertation. But this suggestion does, I hope, serve to provide context for my much narrower claim: that science did play a significant role in the rhetorical canon of the Victorian era. Self-identified rhetoricians, such as Hugh Blair, Benjamin Smart, and Richard Whately, discussed it. Philosophers studying argumentation, such as John Stuart Mill, John Herschel, and William Whewell, discussed it. And scientists themselves, such as Charles Darwin and Thomas Henry Huxley, were significantly influenced by the resulting theories. I will address all of these points in my dissertation.

That last point is strong enough that it deserves emphasis. Few people would suggest that many of today's scientists explicitly study the rhetoric of science—even now that it's an established discipline. Most works in the discipline comment on what scientists have done; they make no pretension of influencing the scientists themselves. I see hope for change in this area, but that's the way it stands now. And yet I am making the claim that even without an established discipline of the rhetoric of science, the theories of that type had an identifiable influence on the rhetoric of Darwin and Huxley. It is not merely the case that the dispute between these two rhetors took place against a

background of these ideas—although much of my dissertation will deal with this lesser claim. It is that both men discussed these ideas and applied them.

In supporting these claims, I take the approach of historical reconstruction, not contemporary appropriation. I will explore the distinction in more detail in Chapter 2. For now, it is enough to say my goal is to meet the rhetors on their own grounds—in their own time and at their own place. I am not attempting to interpret their rhetoric or their science in light of current theories. I am certainly not attempting to judge them for getting things right or wrong, as we would see them. As Martin J.S. Rudwick explained in the preface to *The Meaning of Fossils,*

> It is of course more fruitful, and more interesting, to refrain altogether from allotting them credit or black marks for their opinions, and to try instead to understand them as men of their own time, grappling with problems which they rarely had enough evidence to solve, and solving them, if at all, in terms of their own view of the world.[22]

One of the most important considerations in understanding them as men of their own time is to set aside our modern understanding of science. Today, evolution is established to the point that no working biologist seriously denies it. Natural selection is almost as well established. Biologists debate the extent of its effect, but they do not debate the existence of the effect. It is a solid theory—that is, an idea that explains a class of facts and allows for accurate predictions to be made. In the Victorian era, however, that was not the case. John Stuart Mill and Huxley both viewed it as a hypothesis. Perhaps surprisingly, so did Darwin. In a letter to Joseph Hooker on February 14, 1860, he said, "I have always looked at this doctrine of Nat. Selection as an hypothesis, which if it explained several

[22] Rudwick, *Meaning*. Page not numbered.

large classes of facts would deserve to be ranked as a theory."[23] This is in line with the introduction to his *Variation*, eight years later.[24] We cannot criticize those who failed to see the strength of Darwin's theory; the evidence for it remained disputable until the conclusion of the modern synthesis in the 1940s.

In a similar vein, my project requires setting aside modern views of rhetoric. This is less of a restriction than it may seem. The rhetorical theories available to Darwin and Huxley were surprisingly modern. Certainly it is true that the discipline has made progress in the last 150 years, but as Mill pointed out in his review of Benjamin Smart's work on rhetoric, some very bright minds had already been considering those issues for over two thousand years.

The relevant works

The rhetorician most associated with Darwin research is John Angus Campbell, beginning in the 1970s. Writing at a time when many rhetoricians seriously questioned the value of applying rhetoric to science, part of his goal was to show that even great scientists, like Darwin, used rhetoric. This idea had long been doubted. In a work published in 1896, E. Ray Lankester wrote, "The style of Darwin's writings is remarkable for the absence of all affectation, of all attempt at epigram, literary allusion, or rhetoric. In this it is admirably suited to its subject."[25] Rhetoric, then, wasn't suited to Darwin's subject, and even by the standards of scientific discourse, Darwin was remarkable for his lack of it. Campbell showed otherwise. "Charles Darwin: Rhetorician of Science" makes

[23] Darwin, letter to Hooker. http://www.darwinproject.ac.uk/entry-2696
[24] Darwin, *Variation*, Vol. I, 9.
[25] Lankester, "Charles Robert Darwin," 4392.

this case especially strongly. Besides his major point that Darwin was an effective rhetorician, he also attempted to offer a minor point involving Huxley's disagreements with Darwin: "Even Huxley, wholehearted as he was in advancing science through championship of Darwin, did not believe natural selection to be the sole cause of evolution."[26] But this is no real disagreement. Darwin didn't believe it was the sole cause of evolution, either. He made this view explicitly clear dozens of times in his work, starting with the introduction to the first edition of the *Origin*: "I am convinced that Natural Selection has been the main but not the exclusive means of modification."[27] As we have seen, Huxley's statement was actually considerably stronger than Campbell implies. Not only did Huxley not believe natural selection to be the sole cause, he wasn't sure it was a cause at all. As important as Campbell's research was in the early years of the rhetoric of science, it offers little help in understanding this dispute.

Darwin has received attention from other rhetoricians, as well. Gross discussed him extensively in *Starring the Text*. Leah Ceccarelli brought up his arguments in *Shaping Science with Rhetoric*. Celeste Michelle Condit did so in *The Meanings of the Gene.* Segal discussed his hypochondria in *Health and the Rhetoric of Medicine.* Most of these authors also mentioned Campbell's discussion of Darwin. But none of them said much about Huxley's disputes with Darwin.

[26] Huxley, "Rhetorician of Science," 14.
[27] Darwin, *Origin*, 6.

The subject has been examined in more detail by historians of science than by rhetoricians. Malcolm Jay Kottler, of the University of Minnesota's Bell Museum, is noteworthy in referring to the Darwin-Huxley interaction as a "debate."[28] He explained:

> Thomas Henry Huxley is well remembered as "Darwin's bulldog" because of his vigorous defense of the theory of descent after the publication of the *Origin*. But it is not very well known that Huxley, from 1859 to his death in 1895, remained doubtful about the theory of natural selection.[29]

Kottler, however, presented the Darwin-Huxley debate as a side note to the Darwin-Wallace debate. It did not in itself inspire a great deal of analysis, and Kottler was in any case not a rhetorician. His goal was not to offer a rhetorical analysis, but to set forth the historical fact of the disagreement.

Although Kottler said Huxley's doubts weren't very well known, this claim was true more of scholars of science than of scientists themselves. Writing in 1979, the paleoanthropologist Richard Leakey pointed out that "even Huxley, who called himself 'Darwin's bulldog' and was the most vigorous defender or Darwin's work in the later nineteenth century, did not believe that natural selection had been demonstrated as the primary mechanism of evolutionary change."[30] This doubt, according to Leakey, led Darwin to consider other mechanisms for evolution, including sexual selection.

Scientists today continue the discussion of Huxley's doubts. In *The Bonobo and the Atheist* (2013), the primatologist Frans de Waal spoke derisively about Huxley's lack

[28] Kottler, "Two Decades of Debate," 391.
[29] Ibid.
[30] Leakey, Introduction to *Origin*, 11.

of formal education, admitting, however, that he "was a self-taught comparative anatomist of great standing."[31] But then he added:

> He was notoriously reluctant, however, to accept natural selection as the chief engine of evolution and also had trouble with gradualism. These are no minor details, which is why we shouldn't be surprised that one of the last century's leading biologists, Ernst Mayr, harshly concluded that Huxley "did not represent genuine Darwinian thought in any way."[32]

I don't disagree that Huxley failed to represent genuine Darwinian thought. Evolution without natural selection or gradualism is not Darwinian evolution. But de Waal's view reverses the problem of seeing Huxley merely as Darwin's bulldog. Instead of viewing Huxley as parroting Darwin's ideas, he viewed Huxley's ideas as entirely separate from Darwin's. Neither approach accurately represents the interactions between the men. Mayr had a similar approach in *The Growth of Biological Thought*. The book quite brilliantly succeeded in Mayr's objective: not to reconstruct the history of biology, but to explain "the background and development of the ideas dominating modern biology."[33] He viewed history through an explicitly modern lens—and not only that, but the lens of explicitly modern science. This was exactly the opposite of my own approach, and it led to different conclusions. Mayr and de Waal were correct in their implications that Huxley didn't understand evolution very well—from a perspective of modern science. That is a useful perspective, but it is not mine. My own perspective, as I hope to make clear throughout this dissertation, is based as far as possible on nineteenth-century thought and knowledge.

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[31] De Waal, *Bonobo*, 34.
[32] Ibid.
[33] Mayr, *Growth*, vii.

Darwin respected Huxley's views much more than either de Waal or Mayr, both of whom had the advantage of more than a century of additional scientific knowledge to filter out Huxley's errors. Nor was Darwin alone in respecting Huxley. As Campbell said, "Both Darwin and Huxley enjoyed solid reputations as scientists."[34] Huxley's reputation has faltered over the years since he died, and without keeping this point in mind, we cannot effectively work toward an understanding of how Darwin's and Huxley's interactions were viewed at the time.

Although modern scientists and scholars who share my goal often have useful ideas and observations to include in my dissertation, any careful attempt at historical reconstruction requires a focus on the relevant works of the time. We know for certain some of the works that provided the context for the rhetoric used by both men. In Chapters 3 and 4, I'll set forth the evidence showing what Darwin and Huxley studied and what works were part of the ambient discourse. For now, my objective is only to introduce the most important of these works.

Benjamin Smart's *Beginnings of a New School of Metaphysics* (1839; includes sections first published in 1831). Smart, a rhetorician and philosopher, has been largely ignored by rhetoricians and philosophers for over a hundred years. Edward Manier, a philosopher of science, is rare among twentieth-century scholars in even mentioning him. In his time, however, he was far more respected. No less of an authority than John Stuart Mill, in his *System of Logic*, called him "always acute and often profound."[35] Mill glowingly reviewed his work in *The Examiner* (in March and April of 1832—a lengthy

[34] Campbell, "Rhetorician of Science," 6.
[35] Mill, *System of Logic*, 115 fn.

review spread out over two issues). Mill seemed unconcerned with Smart's comment that he had never read any of Mill's works.[36] Smart had, however, read—and discussed—Plato, Aristotle, Cicero, Quintilian, Blair, Campbell, and Whately (who was cited 29 times). Reading this book alone would provide a fair introduction to the canon of rhetoricians and philosophers. The section of Smart's *Metaphysics* devoted to rhetoric, which he defined as "the right use of words with a view to inform, convince, or persuade,"[37] was only seventy pages long. But he mentioned the subject often when discussing logic, grammar, and philosophy. And even those seventy pages were quite good—in Mill's opinion, the section on rhetoric was "the best of all: it is full of valuable truth and high moral feeling."[38] As we will see, Darwin also thought highly of Smart's work, which was, in my view, more theoretically rich than almost any other work on rhetoric in the nineteenth century. Smart followed up his reflections on rhetoric with *A Manual of Rhetoric* in 1848, but I am not sure Darwin ever read it, so I will focus on his *Metaphysics*.

Hugh Blair's *Lectures on Rhetoric and Belles Lettres*. A modern reader of Blair's *Lectures* may not view them as having much to do with rhetoric. "Traditionally," Kenneth Burke told us, "the key term for rhetoric is not 'identification,' but 'persuasion.'"[39] Neither one was the key term for Blair. I am not sure what was—class, perhaps, given that Blair's objective seemed to be assisting his readers in gaining the understanding of language that would make them at home among the right class of

[36] Smart, *Metaphysics*, 262 fn.
[37] Ibid., 87.
[38] Mill, Review of *Outline of Sematology II*, 212.
[39] Burke, *Rhetoric of Motives*, xiv.

people. Blair's work, published in 1783, was decades old when Darwin read it, but still very popular.

Richard Whately's *Elements of Rhetoric.* Whately, the Archbishop of Dublin, was interested in rhetoric largely as a way of persuading others of the word of God. Many of the examples in his *Elements* were theological. The techniques, however, have far wider applications. Indeed, his approach was heavily Aristotelian, and no one argues that Aristotle's goals were Christian. Whately's work is especially relevant not only because of Darwin's familiarity with it, but because Whately explicitly recognized the value of rhetoric as applied to science.

John Stuart Mill's *System of Logic, Ratiocinative and Inductive.* As its title implies, this was a work of logic, not rhetoric. But it dealt extensively with argumentation, and Mill was influenced by rhetoricians as he wrote it. He mentioned Smart, as we have seen, and his revision of this work was assisted by the rhetorician Alexander Bain.[40]

William Whewell's *Philosophy of the Inductive Sciences, Founded upon Their History.* This major work on the philosophy of science contributes to this project in two important ways. First, it set forth a theory of argumentation that was familiar to nearly everyone of significance taking part in this debate—not only Darwin and Huxley, but the people commenting on their works most effectively. Second, its depiction of the core questions and methods of science nicely summarized the state of nineteenth-century thought. Reading it helps us understand the background against which this debate took place.

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[40] Bizzell and Herzberg, *The Rhetorical Tradition,* 2nd ed., 1142.

John Herschel's *Preliminary Discourse on the Study of Natural Philosophy.* Herschel, an English polymath, set forth an inductive approach to science and to scientific argumentation. His discussion of analogy, especially, helped Darwin develop one of his most important argumentative strategies.

Jean-Baptiste Lamarck's *Philosophie zoologique*. Lamarck was probably the most influential writer about evolutionary theory before Darwin. Although today's views of his contributions are not always very flattering, he had more in common with Darwin than many writers today realize—so much, in fact, that it was a challenge for Darwin to make clear the essential differences.

Sir Charles Lyell's *Principles of Geology* and *Antiquity of Man*. Lyell's views on geology and the change of the earth were critical to Darwin's development throughout his career. As a young man, Darwin studied Lyell carefully and based many of his own methods on Lyell's. These methods, as we will see, include both science and rhetoric.

The remaining chapters

I am aware that my claims will require strong support, and I hope my remaining chapters will offer that. Chapter 2 will explain my methodology as I move forward. This methodology is based on a reconstruction of nineteenth-century science and its connection with the rhetoric of its day. Instead of working from modern rhetorical theory, I establish that Darwin and Huxley were familiar with some of the rhetorical and argumentative theories available to them, and then use those theories to shed light on their debate.

Chapter 3 will address my claim that rhetoric as it existed in the Victorian era—especially as it was known to Darwin, and to a lesser extent, to Huxley—was remarkably similar to the twentieth-century rhetoric of science in the issues it addressed and the answers given. One notable example involved the question of whether rhetoric is constitutive of knowledge. This chapter will also show that the rhetorical ideas explored at that time can be seen in the writing of both men.

Although I view rhetoric as a critical part of science (a view that is, as we will see, at least 180 years old), the two disciplines are usually studied separately, and I have found it convenient to continue this separation in the organization of my dissertation. Chapter 4, then, will move from a discussion of rhetoric to one of science. It will consider the question of what people knew about science before the Darwin-Huxley dispute, and also how this knowledge influenced the works of both men.

By this point in my dissertation, the reader should have an adequate understanding of the backgrounds of both Darwin and Huxley as they began their interaction. Chapter 5 will focus on the interaction itself, showing how the ideas of each man influenced the other over a period of about forty years (keeping in mind that Darwin's ideas continued to influence Huxley after Darwin's death). It will start with some areas of agreement to illustrate how the men communicated most often, and then move on to two essential areas of disagreement: natural selection and saltation.

Chapter 2: Methods

This dissertation is about history. More specifically, it is about rhetorical history. It is also about the history of science and scientific inquiry. My methods of researching the history of rhetoric have been strongly influenced by my study of scientific methods of inquiry, especially Darwin's. I will begin this chapter by briefly explaining those methods. Then I will apply them to the study of history in general, and finally to rhetorical history in particular.

Other historians have also been influenced by scientific approaches. In discussing rhetorical historiography, David Gold has explained that "empirical researchers sometimes make distinctions between discovery and verification modes. In the first, one knows little and works inductively to find patterns; in the latter, one works deductively to test hypotheses."[41] Scientists have made this distinction, as well, but the more reflective ones have questioned whether the process really works that way. They argue that it isn't even possible to make it through the first part of that process, much less the second. Sir Peter Medawar, who was awarded the 1960 Nobel Prize in Physiology or Medicine, put it succinctly: "The theory underlying the inductive method cannot be sustained."[42] He went on to add, "And our great modern authority on the nature of scientific method, Professor Karl Popper, has no use for induction at all: he regards the inductive process of thought as a myth."[43] Medawar blamed the misunderstanding of induction on "the work of a great

[41] Gold, "Historiography," 24.
[42] Medawar, "Fraud," 9.
[43] Ibid., 11.

and wise but in this context, I think, very mistaken man—John Stuart Mill."[44] Mill's view of induction, as we will see, strongly influenced both Darwin and Huxley. Within that context, for my purposes, we must take it seriously. That doesn't mean, however, that his mistake should carry over into my historical study of these men.

It would be possible, of course, for induction to fail in science, but succeed in history. But Medawar's objections to the use of induction in sociology, applying the method to "the actual raw facts about what people do and what people say" in today's world, would seem even more relevant to the study of what people did and said in the world of the past. We have even less information to work from in studying historical people than in studying those living today.

Darwin shared Medawar's objection to "unprejudiced observation," which Medawar called "the starting point of induction." Darwin's comment on the subject relates as well to history as to science: "How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service!"[45] In Chapter 3, I will discuss how this quotation helps shed light on Darwin's scientific methods. For now, my point is that as I went through materials from Darwin and Huxley, all of my observations were intended to be for or against some view. These views turned out to be correct perhaps half of the time. Even when they were wrong, however, they helped me to understand some issue more clearly, and to pass on that understanding to my readers.

One example should suffice. At the start of this project, I knew that nearly all biologists accept some modified version of Darwin's theory of evolution by natural

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[44] Medawar, "Fraud," 8.
[45] Darwin, letter to Henry Fawcett. https://www.darwinproject.ac.uk/letter/DCP-LETT-3257.xml

selection. Its status as an established theory, not a mere hypothesis, is firm. People who present it as a hypothesis rarely understand it. I felt it reasonable, then, to assume that any of Darwin's contemporaries who viewed it as a hypothesis must similarly misunderstand it. This assumption led to several potential research questions. Why, for example, did Darwin not correct Huxley's misunderstanding when Huxley said Darwin's theory was "still a hypothesis, and not yet the theory of species"?[46] The answer, as we will see, is that Darwin's idea was properly viewed as a hypothesis at the time. But none of my observations about theories and hypotheses would have been of any service if I had not been seeking them out to support my view.

This process works from an assumption that needs to be made explicit, because it guides this entire dissertation: the relevant consideration is not what we now perceive to be true, but what Darwin and Huxley perceived to be true. Granted, in explaining their perceptions, I am necessarily working from my own, but the ultimate goal is to get as close to theirs as possible. The next section explains this assumption in more detail.

History

For any set of events that occurred in the past, many stories could be told. A particular historian's choice of story depends largely upon his or her choice of methods. These methods help determine which events he or she becomes aware of in the first place, and having become aware of them, they determine even more strongly the significance he or she places upon them. In working toward an understanding of my methods, two distinctions immediately become evident. The first is the distinction

[46] Huxley, *Darwiniana*, 74.

between contemporary appropriation and historical reconstruction. The second is that between history as it really was (that is, independent of any narrative) and history as perceived by those who lived it. As I explain these major distinctions, a few minor distinctions will come into play, as well.

Commenting on Richard Rorty's distinction between historical reconstruction and rational reconstruction, Stephen Makin explains the former more concisely than Rorty does: "An historical reconstruction of some philosopher's thought gives an account of what some past thinker said, or would have said, to his contemporaries. The thinker is not treated as reeducated into our techniques and positions."[47] It is my goal to give such an account of the debate between Darwin and Huxley. Because I am not assuming they would have knowledge of modern science, I am not privileging ideas that are today viewed as important (unless they were viewed as important in the nineteenth century).

I am also not assuming they would have knowledge of modern rhetoric, so my analysis is not based on Kenneth Burke or I.A. Richards or Chaïm Perelman and Lucie Olbrechts-Tyteca. But this raises the question of what knowledge Darwin and Huxley did have of rhetoric. This question is critical to my dissertation, and one of my goals is to offer an answer. Part of that answer will be definitive. We know, for example, that Darwin was familiar with William Whewell's ideas on arguing from induction ,[48] and that Huxley was familiar with John Stuart Mill's ideas on the same subject.[49] With this knowledge in mind, we can see whether the rhetorical techniques they use actually follow the methods they claim. Another part of that answer must be speculative. We don't

[47] Makin, "What Ancient Philosophers Said," 122.
[48] Darwin, *Notebook D*, 117.
[49] Huxley, *Darwiniana*, 73.

know every book and lecture these men saw, and even if we do know, we can't always say for sure that a rhetorical technique was influenced by that work or something else. But even the speculative parts of this answer, if presented carefully, should help us understand the kinds of thinking used at the time.

I believe this understanding is important, even—perhaps especially—when the thinking is different from our own. Rorty explains why: "The main reason we want historical knowledge of what . . . dead philosophers and scientists, would have said to each other is that it helps us to recognize that there have been different forms of intellectual life than ours."[50] In the case of this debate, we often don't have to guess what they would have said to each other. We have many surviving letters and journal entries, as well as the published results of these behind-the-scene exchanges. Some letters haven't survived, and of course, we can't be sure of their spoken words. But even in these cases, we can sometimes make reasonable assumptions about what must have been said, based on surviving replies or observations made by others at the time.

If we grant, then, that my goal is to reconstruct history, the question then becomes what history is being constructed. The historian Stuart Clark presents two contrasting possibilities: first, that there is some "real" history that we can now pull together, regardless of any individual's perceptions of that reality; and second, that the most we can do is work toward an understanding of the perceptions of those who lived at the time. He examines both of these possibilities in his study of the historians of the journal *Annales: Economies, sociétés, civilisations.* He argues that these historians had a huge effect on historical thought: "Indeed, no comparable group of scholars has exerted a more

[50] Rorty, "Historiography," 51.

decisive influence."[51] Out of this group, "the single most important influence has been that of Fernand Braudel," the editor from 1957 to 1968, and associated with the journal both before and after that time .[52] As influential as he may have been, I reject the core of his approach. Braudel, like me, was interested in reconstructing history. Unlike me, his goal was "to show how the world *was* in times past, irrespective of how it was seen by those who lived in it."[53] To me, the view of those who lived in it was a very real part of how it really was. Certainly, there were cases in which perception did not match reality. People in ancient Egypt were evolving, even if they didn't know it. But the lack of knowledge created an epistemological reality that I consider worth studying in its own right.

> Braudel himself was aware of this view of reality:
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> > Braudel describes the world of events as narrow, superficial, ephemeral, provisional and capricious. Above all, it is the world of illusion. He does not deny that like the other layers of history it has its own reality, but he argues that this is reality as it appears to agents, not reality as it is. ... The perspectives in which they [the agents] view their lives are too short and constricted to allow them to discriminate properly between what is important and what is trivial.[54]

The perspective Braudel wished for—the ability to put individual experiences in context—is available only through hindsight. Through hindsight, we can see that Darwin's ideas on pangenesis were trivial. Neither he nor Huxley had any way of knowing that at the time, however. Braudel's attempt at realism actually requires a modern perspective that is anachronistic—and therefore actually unrealistic—when placed in historical context.

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[51] Clark, "The Annales Historians," 186.
[52] Clark, "The Annales Historians," 179.
[53] Ibid., 189-190.
[54] Ibid., 184.

I am not alone in my claim that the original agents' view of history is important. Gesa Kirsch and Jacqueline Royster were explicit on this point as they described the questions that guide their research as rhetorical historians. They wrote from a feminist perspective, and their goal of studying forgotten women might seem at odds with my goal of studying famous men. But the guiding questions for their methodology are identical:

> How do we transport ourselves back to the time and context in which they lived, knowing full well that it is not possible to see things from their vantage point? How do *they* frame (rather than *we* frame) the questions by which they navigated their own lives? What more lingers in what we know about them that would suggest that we need to think again, to think more deeply, to think more broadly?[55]

Kirsch and Royster did concede some value in looking "from the present into the past."[56] For them, however, that was only one small part of what they called "tacking out." By this, they mean taking a wider view. The concept is taken from Clifford Geertz. He once asked, "Are we, in describing symbol uses, describing perceptions, sentiments, outlooks, experiences? And in what sense? . . . That we know words or that we know minds?"[57] He didn't answer his own question, but he did offer some critical guidance for those wishing to do so:

> In answering this question, it is necessary, I think, first to notice the characteristic intellectual movement, the inward conceptual rhythm . . . namely, a continuous dialectical tacking between the most local of local detail and the most global of global structure in such a way as to bring them into simultaneous view. ... Hopping back and forth between the whole conceived through the parts that actualize it and the parts conceived through the whole that motivates them, we seek to turn them, by a sort of intellectual perpetual motion, into explications of one another.[58]

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[55] Kirsch and Royster, "Feminist Rhetorical Practices," 648-49.
[56] Ibid., 651.
[57] Geertz, *Local Knowledge*, 69.
[58] Ibid.

The tacking Geertz describes here—tacking in to a more specific view, tacking out to a global one—is present throughout my dissertation. I wish to explore the minds of Darwin and Huxley by exploring their words. But these words mean nothing without the context of the lives of both men, as well as the culture in which they lived. This approach is precisely the opposite of Braudel's; as Clark explained, "what has interested Braudel is nature rather than culture, 'things' rather than 'words.'"[59] Darwin and Huxley were certainly interested in nature, but they interpreted it through their culture and expressed it through their words.

In addition to rejecting Braudel's approach, I'm also rejecting another trend: "Historians today are less content to 'find' manifest evidence and more intent on 'discovering' evidence that has until recently been latent in such data pools as census questionnaires or slave-market records" (Clark and McKerrow 39). Manifest evidence includes works explicitly written by people in history: letters, notebooks, newspaper editorials, and so forth. That is exactly the kind of evidence that interests me. As Culpepper Clark and Raymie McKerrow point out, turning to latent evidence means that "historians assume even more the role of *rhetorical agents* as they invent ways to reconstruct history and thereby to argue its meaning."[60] This is indeed a way to reconstruct history, but I think it is a less reliable way to determine what historical figures thought than reading their actual words is.

[59] Clark, "The Annales Historians," 190.
[60] Clark and McKerrow, "Rhetorical Construction of History," 39.

Rhetorical history

David Zarefsky, in "Four Senses of Rhetorical History," explained the "(at least) four different kinds of inquiry embraced by the term 'rhetorical history.'"[61] These are "the history of rhetoric, the rhetoric of history, historical studies of rhetorical practice, and rhetorical studies of historical events."[62] This dissertation includes three of the four. It avoids the second one (except in this chapter).

For my purposes, exploring some of the history of rhetoric is a means to an end, not an end in itself. I want to know why Darwin and Huxley used the rhetorical techniques they did, and part of the answer may come from the rhetorical techniques they studied. Explaining this requires me to discuss the rhetorical works that were available at the time, and to examine evidence that either man read these works. But the works themselves are relevant only insofar as they shed light on this debate.

Much of what Zarefsky called "the rhetoric of history" might be better viewed as the rhetoric of historiography. Historiography is the study of how history is written. One could analyze the rhetorical situations behind different historians' approaches to Darwin and Huxley. Richard Dawkins and John Angus Campbell both examine the work of these men in light of current knowledge. In this respect, their approach is different from mine. But they have very little in common with each other. Dawkins is a scientist, and like almost all scientists, he accepts Darwin's theory in broad outline. Campbell has said of Darwin's theory, in an essay critical of Dawkins, that "like every scientific theory in

[61] Zarefsky, "Four Senses," 26.
[62] Ibid.

human history, it too is probably wrong."[63] This difference in conclusions stems from differences in methods, purpose, audience, and exigence. Rhetorical tools would allow for an interesting comparison, but such a study would be quite different from what I do here. My goal is not to study historians, even historians as agents of history; it is to study the original agents in their original context.

The third type of inquiry is historical studies of rhetorical events. The publication of *The Origin of Species* was a major rhetorical event, and many scholars have already placed it in historical context. Its effect on science and society has been remarkably well documented. When I refer to that, I will do so for the purpose of tacking out—of putting my own observations and analysis in perspective. On a more local scale, when tacking in, I examine individual letters and journal entries as a series of small rhetorical events with a large cumulative effect.

Of these four types, the most important one for my purposes is a rhetorical study of historical events. These are, for the most part, ordinary events in the lives of extraordinary people. The tools for examining them are pulled from the body of rhetoric, as it was available to people of the time. These tools are the subject of the next chapter.

[63] Campbell, "Science Education," A9.

Chapter 3: The Rhetoric

In 2006, Alan Gross published *Starring the Text: The Place of Rhetoric in Science Studies*. Much of his first chapter traced the development of answers to the question of whether rhetoric is constitutive of knowledge. This question is critical for the rhetoric of science, because it helps establish the relationship between science and rhetoric. Is rhetoric just something that is added to scientific knowledge, to make it more persuasive? Or is the knowledge itself inherently rhetorical?

We can place the debate between Darwin and Huxley against the background of this question. It is not new. Writing in the late eighteenth century, Hugh Blair, in his *Lectures on Rhetoric and Belles Lettres*, said, "Knowledge and science[64] must furnish the materials that form the body and substance of any valued composition. Rhetoric serves to add the polish; and we know that none but firm and solid bodies can be polished well."[65] Rhetoric, then, is not the knowledge being shared; it is merely the polish over it. Rhetoric could not even be used effectively without solid science. Contrasting with Blair's view was that of Benjamin Smart.

In the section on rhetoric in his *Metaphysics*, Smart emphasized that rhetorical methods are not "ornaments superinduced on the plain matter of language, and capable of being detached from it: they are the original texture of language."[66] Of course, saying

[64] Although both Blair and Gross discuss science, a direct comparison of their ideas may be misleading. Blair's definition of science was much broader, to the point that "knowledge and science" was almost redundant. My point stands, however, that they had opposing views on the relation between science and rhetoric.
[65] Blair, *Lectures* I, 3.
[66] Smart, *Metaphysics*, 214.

that rhetoric is the original texture of *language* is not the same as saying it is the original texture of *knowledge*. For that, we need another step: the connection between language and knowledge. Smart discussed this connection several times, and stated it most clearly in the conclusion to his section on rhetoric:

> To conclude ; — the theory which, in this treatise, we have endeavoured to establish is this, — that we come at all our knowledge by the use of *media*, which media are, chiefly, words ; and that, as the words procure the notions, the notions exist not antecedently to language . . . .[67]

In short, we can have neither knowledge nor notions without language, and rhetoric is the original texture of language, incapable of being detached from it.

The contrast between Smart's view and Blair's can be made even more distinct when we read Smart's firm rebuttal of the view that a rhetor's techniques are a "means to dissemble and put a gloss upon, rather than to discover his real sentiments."[68] Smart rejected the idea that rhetoric was a gloss; Blair explicitly stated that that rhetoric was the polish. For our purposes, gloss and polish are probably synonymous. With either term, it's clear that Blair viewed rhetoric as something added to knowledge; Smart viewed it as something required for knowledge.

Darwin was familiar with the works of both Blair and Smart, and specifically with the works quoted above. In a list called "Books that I have read thro since my return to Edinburgh," Darwin mentions "Blairs lectures on Belles Lettres. 3 Vol."[69] I take this to mean that Darwin had read Blair's three-volume *Lectures on Rhetoric and Belles Lettres*, which must have given him a rather thorough background on one of the most popular

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[67] Smart, *Metaphysics*, 247. Emphasis in original.
[68] Ibid., 208.
[69] This is from Darwin's student booklist, which can be viewed here: https://www.darwinproject.ac.uk/what-darwin-read/darwin-s-student-booklist

approaches to rhetoric of the day. We know, as well, that he read Smart's book. He said he did, and added that it was in the library of the Athenæum Club,[70] where both he and Smart were members. There is no evidence that Darwin and Smart ever corresponded, but they may well have spoken in person. They had many common interests.

In spite of their disagreements about the nature of rhetoric, both Blair and Smart agreed that science was an appropriate subject for rhetoric. Blair, for example, offered a stylistic analysis of a paper saying that light and colors exist only in the mind, not in matter.[71] Smart, too, as we will see in a moment, discussed matter and mind.[72] Whately agreed with both of them about analyzing science with rhetoric; at one point, which we will examine in more detail later, he criticized writers on chemistry and natural history for misusing arguments by analogy.[73] Scientists themselves, for the most part, were much less open to the idea of combining rhetoric and science. The purpose of words was to convey knowledge in its purest form. Darwin was a rare exception. To see how far his views diverged from the norm, it may help to see how other famous scientists saw the subject before him.

Antoine-Laurent Lavoisier, the father of modern chemistry, was an especially good example. In the preface to his *Traité élémentaire de Chimie* [*Elements of Chemistry*] (1789), he insisted that "*Le mot doit faire naître l'idée; l'idée doit peindre le*

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[70] Darwin's reading notebooks. https://www.darwinproject.ac.uk/what-darwin-read/darwin-s-reading-notebooks

[71] Blair, *Lectures*, 155. http://archive.org/stream/lecturesonrhetor00blai/lecturesonrhetor00blai_djvu.txt

[72] Smart, *Metaphysics*, 451, 454.

[73] Whately, *Elements of Rhetoric*, 73.

fait; ce sont trois empreintes d'un meme cachet."[74] Smart took a different view—not in direct response to Lavoisier, but it might as well have been:

> Wherever there is a word, there is said to be an idea . . . and wherever there is an idea we are apt to believe there is a thing correspondent to it. It will be much better to consider every word as fitted to be the sign of some notion[75] or state of intellect; then to ask whether, beyond the notion, there are real things understood by it; and whether, of these things, further knowledge can, by searching, be attained. [76]

For Smart, this was not only a general rhetorical principle, but one that applied specifically to the language of science. He followed this statement with a fairly lengthy exposition of the scientific terms time, space, matter, mind, and infinity. Each case demonstrated the point that we cannot assume a direct connection between words and ideas, or between ideas and facts. As he phrased it earlier in his book, "there is no such thing as an express and direct image of thought."[77] It is only through rhetoric that we can bridge this gap, not through some nonexistent, purely representational language. Having discussed these terms in light of his rhetorical theory, he then built upon them to make other claims about the nature of science—for example, that a science of mind should be viewed at the same level as the science of matter.[78] (Sigmund Freud had not yet been born.) Again, we see a deep interest in applying rhetoric to science, and no reservations at all about doing so.

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[74] "The word should produce the idea, and the idea should be a picture of the fact; these are three impressions of the same seal." Original version is at http://gallica.bnf.fr/ark:/12148/bpt6k5524956g/f7.image

[75] I am here using "notion" and "idea" interchangeably. Smart argued for a possible distinction (13), but it's not relevant to my current argument.

[76] Smart, *Metaphysics*, 448-49.

[77] Ibid., 210.

[78] Ibid., 457.

The rhetoric of preconceived ideas

Part of the nineteenth-century debate on rhetoric and science focused on preconceived ideas. Most scientists defined science in a way that rejected preconceptions. In doing so, they defined it in a way that dictated the arguments that could be presented. Science—and scientific arguments—had to come from outside the mind, in nature, not from anything already existing in the mind. The question of what arguments could be presented was a rhetorical one. Thus, scientists were, in effect, defining science in rhetorical terms. This definition led, in turn, to certain rhetorical moves, such as claiming to reject preconceptions. In 1864, Louis Pasteur could have been speaking for the scientific community as a whole[79] when he gave a well-attended presentation on the validity of spontaneous generation (a theory that Pasteur discredited; it claims that life can generate itself spontaneously—that is, it can come from non-life):

> *Je pourrais même ajouter: Comme savant, peu m'importe. C'est un question de fait; je l'ai abordée sans idée préconçue, aussi prêt à déclarer, si l'expérience m'en avait imposé l'aveu, qu'il existe des générations spontanées, que je suis persuadé aujourd'hui que ceux qui les affirment ont un bandeau sur les yeux.*[80] [81]

Apparently, anyone who takes any other approach cannot speak as a scientist.

Huxley seems to have agreed that preconceived ideas create a problem for science. Some of his comments on the validity of Miltonic creationism are remarkably similar to Pasteur's on spontaneous generation: "For my part, I have no prejudice one way or the other. If there is evidence in favour of this view, I am burdened by no

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[79] Excluding Félix Pouchet, whose arguments on spontaneous generation he was refuting.
[80] "I might even add: as a scientist, I do not care. It is a question of fact; I approached it without a preconceived idea, equally ready to declare, if experiment had imposed upon me the view, that spontaneous generations exist, as I am now persuaded that those who affirm them have a blindfold over their eyes."
[81] *Oeuvres de Pasteur*, II, 334.

theoretical difficulties in the way of accepting it; but there must be evidence. Scientific men get an awkward habit—no, I won't call it that, for it is a valuable habit—of believing nothing unless there is evidence for it . . . ."[82] Neither rhetor was copying from the other. Both were using an accepted rhetorical technique of the time: identification with the ideal of an objective scientist who would consider his opponent's claim fairly. This is a special case of identification with any competent group, as we see as far back as Plato's *Gorgias*. Socrates explained that it makes sense to listen to the counsel of specialists in their area of specialty, such as a master workman when walls have to be built or the military when battles have to be arranged.[83] This rather obvious point implies that rhetors who identify with the specialist community can take on the ethos of that community. The scientific community during the eighteenth and nineteenth centuries was an especially powerful community to identify with, because of its association with facts and evidence—the inartistic proofs that transcended anything a rhetor could control. Both Pasteur and Huxley were specifically identifying with this aspect of the scientific community.

Smart again questioned the accepted scientific viewpoint. To him, preconceived notions were necessary for persuasion, in science as everywhere else: "Every argument used to influence others, is considered to derive its efficacy from some pre-existing notion, opinion, or ruling motive, whether permanent or transitory, in the hearer . . . ."[84] There is, of course, a critical distinction between avoiding pre-existing notions in rhetors and avoiding them in audiences; Smart was very clear on the difference between thinking as a rhetor and thinking as an audience. The point is simply that getting rid of pre-

[82] Huxley, *Lectures*, 10.
[83] Plato, *Gorgias*, 93 (455b).
[84] Smart, *Metaphysics*, 198.

existing notions is not necessary to think as a *scientist*. Although Smart was not here specifically referring to scientists, we have no reason to believe he thought they had a privileged status in this respect. He was, after all, speaking of *every* argument, scientific or otherwise.

Smart's view here is consistent with his view that rhetoric is constitutive of knowledge. It is through rhetoric that we are able to connect the knowledge we already have (our preconceptions) with the knowledge from our observations, creating a coherent understanding of our world. Without that, we would never gain the deeper understanding that science strives to provide.

Huxley would seem to disagree with this point in his private letters, as in his public lectures. In a letter to Charles Kingsley, he said, "Science . . . warns me to be careful how I adopt a view which jumps with my preconceptions, and to require stronger evidence for such belief than for one to which I was previously hostile." To take this approach, Huxley says, "Sit down before fact as a little child, be prepared to give up every preconceived notion, follow humbly wherever and to whatever abysses nature leads, or you shall learn nothing."[85] It is important to understand, however, that the conflict we see here between rhetoricians and scientists existed only in their descriptions of language and persuasion. In their actual speaking and writing, as they used the techniques rhetoricians described.[86] Huxley may have said that science taught to "give up

[85] Huxley, letter to Kingsley. http://aleph0.clarku.edu/huxley/letters/60.html

[86] This is still true today. In a book published this year, for example, de Waal said, "It would be great if everyone were open-minded and purely interested in the evidence, but science is not immune to preconceived notions ..." (*Animals* 113).

every preconceived notion," but when lecturing on what it means to be a scientist, he plainly started with the audience's preconceived notions, just as Smart explained.

When arguing that "We Are All Scientists," Huxley said near the beginning of his presentation that his audience might understand his point better if he gave them a familiar example.[87] He then worked from their preconceived understanding of thought—the kind of thought used to conclude that a house with a broken window and missing valuables has been burglarized—to explain the kinds of thinking scientists do. Again, Huxley was encouraging an *audience* to work from preconceived understandings, not necessarily doing so himself, and we might normally see a difference between helping someone else reach a conclusion and coming to a conclusion on one's own. In this case, however, Huxley was attempting to demonstrate how scientists think. The rhetorical tool he used—the one Smart suggested he had to use—was at odds with the kind of thinking he had elsewhere claimed scientists do.

Having read Smart's work, Darwin's understanding of preconceived ideas was more sophisticated than that of most other scientists. Darwin believed that they could be essential for a scientist. In a letter to Henry Fawcett, he said, "About 30 years ago there was much talk that geologists ought only to observe and not theorise . . . . How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service!"[88] The letter was written in 1861. Thirty years before, in 1831, Lyell's *Principles of Geology* was being discussed extensively. The Geological Society of London had been founded in 1807 on principles of pure observation, partly in revolt

[87] Huxley, "We Are All Scientists." http://www.is.wayne.edu/MNISSANI/A&S/allsci.htm
[88] Darwin, letter to Fawcett. https://www.darwinproject.ac.uk/letter/DCP-LETT-3257.xml

against excessively speculative notions about the origin of the earth. Lyell's *Principles*, which went well beyond observations and into the realm of theory, shook up perceptions about how geology should be done. It also, as we will see, dramatically influenced Darwin's understanding of rhetoric and science.

The first sentence I quoted above (about observing and not theorizing) cannot be taken to support the value of pre-existing ideas, because it is quite possible—indeed, it was considered ideal—to observe first and then theorize.[89] But to me, the second sentence quoted there implies that the view must be held in advance. How else could one decide what observations are worth making?

At the same time, Darwin saw that preconceived notions could be a barrier to communication. In the conclusion to the *Origin*, Darwin admitted the difficulty of persuading "experienced naturalists" whose long-held views were too strongly opposed to his.[90] But he did "look with confidence to the future, to young and rising naturalists, who will be able to view both sides of the question with impartiality."[91] In these specific examples, then, Darwin's views on preconceived notions were exactly the opposite of Huxley's. Huxley argued, like Pasteur, that scientists needed to be free from preconceptions. But he explicitly declared his reference to them when explaining what scientists do. They should be rejected when doing science, but used when communicating science. Darwin, on the other hand, argued for them when doing science, but saw them as a potential obstacle when communicating science. We cannot, however, read too much

[89] This was the approach Newton claimed for himself when he said, "hypotheses non fingo" ("I frame no hypotheses"). Draft B of the "Scholium Generale."
[90] Darwin, *Origin*, 481.
[91] Ibid., 482.

into these specific examples. Darwin was well aware of the advantages of comparing new ideas with old; that's why he used analogies so frequently.

When we examine the Victorian-era dispute between scientists and rhetoricians on the relationship between science and rhetoric, we see that this dispute existed only at the level of explicit claims. When it came to implicit knowledge, as it appeared in their actual scientific arguments, scientists communicated the way rhetoricians said they did. Smart himself recognized this point. He explained that if his theory is correct,

> we claim for it the merit of a *discovery*, because the common theory, that is, the theory which men are presumed to act upon, and to which all preceptive works are adapted, — not the theory which, unawares, they really act upon, — exhibits that connexion in a very different light.[92]

Real language—what rhetors really act upon—is best described by his theory, not their own. Mill's greatest criticism of Smart was in claiming to offer a new theory: "There is no room now for a new theory," because "These subjects have now occupied the attention of thinking men for upwards of two thousand years."[93] Much as I respect Mill's point, I believe Smart deserves some credit here. I know of no other rhetorician in the nineteenth century whose theories are so much in line with today's thinking.

The originality of Smart's theory is relevant to my purposes because it helps establish how widespread the ideas were. If they were already in the air, as it were, and Smart's contribution was simply to gather them and explain them in a single volume, that increases their potential influence on participants in the debate—not only Darwin and Huxley, but everyone else who jumped in at the time. As it happens, I am not claiming that this is the case. For one thing, Mill was unable to give an example of anyone else

[92] Smart, *Metaphysics*, 248. Emphasis in original.
[93] Mill, Review of *Sematology* II, 211.

who had presented these ideas before Smart. Instead, my own claim is merely that Smart's ideas existed and were known to eminent thinkers of the day, including Darwin himself. The entire debate between Darwin and Huxley took place against a background of disputes between scientists and rhetoricians about the role of rhetoric in science. We will continue to see how this background affected the debate.

Rhetoric by philosophers

Then, as now, much of the rhetoric of science was done by people who didn't identify themselves as rhetoricians. They may even have spoken derisively of rhetoric, as John Locke did when writing "Of the Abuse of Words": "It is evident how much men love to deceive and be deceived, since rhetoric, that powerful instrument of error and deceit, has its established professors, is publicly taught, and has always been had in great reputation . . . ."[94] Huxley seems to have shared this view, as we see in his "Autobiography." Early in his speaking career, he explains, "I believe I had every fault a speaker could have (except talking at random or indulging in rhetoric)."[95] By rhetoric, I assume he meant flowery, verbose language, with a plethora of figures of speech. Certainly, no one, least of all the best rhetoricians of the time, would have recommended that approach. But this was not what the best rhetoricians of the time meant by rhetoric. On the contrary, they specifically rejected it. We have already discussed the views Blair and Smart had of rhetoric. If nothing else, they agreed on this point. So did Whately. On the first page of his *Elements of Rhetoric*, he expressed annoyance that the term "is apt to suggest to many minds an associated idea of empty declamation, or of dishonest

[94]Locke, *Human Understanding*. Smart also quotes this passage in a lengthy footnote beginning on page 208.
[95] Huxley, *Readings*, 13.

artifice."[96] He went on to say that he had neither any ability nor any interest in "any display of florid eloquence and oratorical ornament."[97] If Huxley had been trained in the rhetoric of Whately (or Campbell, Blair, or Smart), he would not have spoken so derisively of "indulging in rhetoric." This was no isolated usage; in at least one account of his famous debate with Bishop Wilberforce, he accused his opponent of plunging into "scientific questions with which he has no real acquaintance, only to obscure them by an aimless rhetoric …."[98] And in his *Lectures on Evolution*, he said, "I shall rejoice … if I have thus convinced you that the great question which we have been discussing is not one to be dealt with by rhetorical flourishes, or by loose and superficial talk; but that it requires the keen attention of the trained intellect and the patience of the accurate observer."[99] Granting that the phrase "rhetorical flourishes" isn't the same as "rhetoric," this still isn't a usage we'd expect from someone who'd studied any influential rhetoricians. This usage separates rhetoric from science altogether.

None of this should imply that Huxley never studied argumentation. In "The Origin of Species," his review of Darwin's book, he showed significant familiarity with Mill's approach to argumentative proof:

> But what Mr. Darwin has attempted to do is in exact accordance with the rule laid down by Mr. Mill; he has endeavoured to determine several great facts inductively, by observation and experiment; he has then reasoned from the data thus furnished; and lastly, he has tested the validity of his ratiocination by comparing his deductions with the observed facts of Nature. Inductively, Mr. Darwin endeavours to prove that species arise in a given way. Deductively, he desires to show that, if they arise in that way, the facts of distribution, development, classification, &c., may be accounted for, i.e. may be deduced from

[96] Whately, *Elements of Rhetoric*, iii.
[97] Ibid., iv.
[98] F. Darwin. *Life and Letters*, 322.
[99] Huxley, *Lectures on Evolution*.

> their mode of origin, combined with admitted changes in physical geography and climate, during an indefinite period.[100]

He then went on to argue that although Darwin's method was flawless, he did not fulfill the method's conditions, and thus, he did not prove that evolution is driven by natural selection. Huxley would not have been able to make this claim as he did without an understanding of the theoretical framework Mill's work on argumentation provided.

As it happens, even if Darwin's argument had fulfilled all of the conditions of Mill's rules, it wouldn't have resulted in absolute proof. After explaining the relation between induction and deduction, Mill added, "This type of ratiocination does not claim, like the syllogism, to be conclusive from the mere form of the expression; nor can it possibly be so."[101] But perhaps the question of whether Darwin followed Mill's rules is not even relevant. Mill argued, "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."[102] What Darwin offered was not proof, but merely an "unimpeachable example of a legitimate hypothesis."[103] And because it was nothing more than a hypothesis—however legitimate it might have been—Darwin "was not bound by the rules of Induction, but by those of Hypothesis. And these last have seldom been more completely fulfilled."[104]

Huxley, like Mill, viewed Darwin's idea as a hypothesis. In "The Origin of Species," he concedes that it is "an extremely valuable, and in the highest degree

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[100] Huxley, *Darwiniana*, 73.
[101] Mill, *System of Logic*, 202.
[102] Ibid., 499 fn.
[103] Ibid.
[104] Ibid.

probable, doctrine, indeed the only extant hypothesis which is worth anything in a scientific point of view; but still a hypothesis, and not yet the theory of species."[105] The strength of the hypothesis, allowing it to be applied to cases yet unobserved, made it suitable for Mill's system of argumentation:

> We shall consider every process by which anything is inferred respecting an unobserved case, as consisting of an Induction followed by a Deduction; because, although the process needs not necessarily be carried on in this form, it is always susceptible of the form, and must be thrown into it when assurance of scientific accuracy is needed and desired.[106]

But although Huxley thought Darwin used the form of an induction followed by a deduction, and although he thought scientific accuracy was needed and desired, he was not assured of the accuracy of natural selection. In spite of his rejection of this particular argument, his knowledge of this rhetorical form—and his trust in it—was undeniable.

The rhetorical force of an argument by induction (even without deduction) was immensely powerful in nineteenth-century science—and rhetoric, for that matter. Even Smart seemed impressed by it: "The presumed fact, on which so much stress was formerly laid, that the higher and more important mental phenomena are entirely unconnected with animal organization, is daily crumbling under the grasp of modern inductive science."[107] My point at the moment has nothing to do with animal organization (although it is notable that he and Darwin shared an interest in this subject), only that Smart felt modern inductive science had the power to crumble a formerly presumed fact. In a similar vein, claiming a lack of induction was a serious criticism. Fawcett wrote to Darwin that he had personally spoken with Mill, who was impressed with Darwin's

[105] Huxley, *Darwiniana*, 74.
[106] Mill, *System of Logic*, 203.
[107] Smart, *Metaphysics*, 43.

approach. Then he added, "It is easy for an antagonistic reviewer when he finds it difficult to answer your arguments to attempt to dispose of the whole matter by uttering some such commonplace, as 'This is not a Baconian induction.'"[108] Darwin's notebooks suggest that he felt he was in fact arguing by induction. As he explained in Notebook D, "The line of argument often pursued throughout my theory is to establish a point as a probability by induction, & to apply it as hypotheses to other points, & see whether it will solve them."[109] Note that he wrote of a "line of argument," or a rhetorical approach. Induction isn't merely a way of approaching science; it's a way of approaching scientific argumentation. Even people who claimed rhetoric should be separate from science fully accepted the scientific nature of this form of argumentation.

Turning to Darwin's *Autobiography*, we see that he presented his approach not only as inductive, but Baconian: "I worked on true Baconian principles, and without any theory collected facts on a wholesale scale."[110] This line leads to an interesting contrast with the exchange of letters Darwin had with Fawcett. We have already seen that nineteenth-century scientists, such as Pasteur and Huxley, gained rhetorical power from the claim of examining facts without preconceived ideas. We have also seen the rhetorical power of Baconian induction. Darwin is here combining them. But this is exactly the opposite of what we saw in the Darwin-Fawcett exchange. There, failure to follow Baconian induction was not the problem an antagonistic reviewer might imagine it to be, and facts had to be for or against some theory if they were to be of any use.

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[108] Fawcett, letter to Darwin. http://www.darwinproject.ac.uk/entry-2868

[109] Darwin, *Notebook D*, 117. http://darwin-online.org.uk/content/frameset?pageseq=103&itemID=CUL-DAR123.-&viewtype=side

[110] Darwin, *Autobiography*, 119.

How can we reconcile Darwin's claim that he worked without a theory with his point that facts could be of no use without a theory? Did Darwin work inductively, or didn't he? In considering this question, I believe it helps to draw a distinction between inductive science and inductive rhetoric. Darwin did indeed collect facts on a wholesale scale, without any theory. But he began doing so well before the 1837 date he gave in his autobiography. Perhaps he began doing so in 1832, after he was underway on his *Beagle* voyage.[111] Perhaps it was in 1828, as he collected beetles at Cambridge. Perhaps it was in 1809, shortly after he was born. Darwin's theory was based on observations that people make whenever they see nature. Thus, long before he had a theory to work from, he was making observations that would later contribute to that theory. In that sense, his method was purely inductive. He was, in his mind, gathering fact after fact that would later support evolution by natural selection.

Many Darwin scholars have attempted to isolate the point when Darwin thought of his theory. I am not convinced that there is one. Reading *The Voyage of the Beagle* from the viewpoint of someone who was already familiar with evolution, I saw several passages that hinted at the idea of the transmutation of species. I don't believe he had the full theory in mind on his voyage. But neither do I believe it waited until the Sketch of 1842. I think it's reasonable to assume that as his theory took shape over the years, Darwin began focusing his observations more and more on facts that would support the version of the theory he had in mind at the time. So what started at birth as random

[111] Darwin did work from theories as he collected specimens during his *Beagle* voyage, but they were primarily theories of field geology, such as he had learned from Adam Sedgwick. Sedgwick actively opposed evolution, and read the *Origin* "with more pain than pleasure," as he said in a letter to Darwin. http://www.darwinproject.ac.uk/entry-2548

observations continued to 1859 as a concentrated effort to find facts that supported his theory. Indeed, this effort continued for the rest of his life. Even as he studied earthworms in his last years, he made observations that were of use specifically for supporting his theory of evolution.

But this process of real thought—the way science is really done—is too messy for persuasive rhetoric. For rhetorical purposes, it made sense to present a coherent narrative of induction. This was not just a matter of claiming he used induction, as he did in his *Autobiography*, but presenting his theory in the argumentative format of induction, as he did in the *Origin*.

I made the distinction between inductive science and inductive rhetoric for ease of explaining. But I must emphasize that the science and the rhetoric were not actually separate in any real sense. Darwin's *Autobiography* describes his scientific processes and the development of his thought, but it is unquestionably also a work of rhetoric, serving to persuade people of the value of what he did. We can even back up a step from the description of these processes, to the processes themselves. They were guided by Darwin's arguments with himself about what was important and what was likely to be persuasive in future interactions with scientists. In this sense, the science was inherently rhetorical. My claim here is nothing new—indeed, it is entirely in line with Smart's theories about rhetoric and science.

Mill's ideas on induction were not the only ones widely discussed at this time. The debate between Mill and Whewell on induction could lead to a dissertation by itself.

Huxley, as we have seen, tended to work from Mill when discussing induction. Darwin, on the other hand, may have worked from Whewell. It is clear that he had read about Whewell's theories well before he wrote the *Origin*. In Notebook E, for example, he wrote, "December 16th [1838] The end of each volume of Whewells Induction History contains many most valuable references."[112] Besides this note, we have even stronger evidence that he read Whewell's *History of the Inductive Sciences*: Edward Manier has observed that Darwin's personal copy "was marked and annotated, particularly the sections dealing with biology and geology in the third volume."[113] Manier added that there is little evidence that Darwin ever read Whewell's *Philosophy of the Inductive Sciences*, in spite of its inclusion on his list of "Books to be Read."[114] I believe he was correct. Still, the *History* includes quite a bit of information on scientific induction in its own right. One final piece of evidence of its influence on Darwin comes from the *Variation*, where he refers to Whewell as "the historian of the inductive sciences."[115]

Rhetoric by scientists

The scientists whom Darwin and Huxley were familiar with used rhetorical techniques that influenced them as much as their explicit knowledge of rhetoric did. These rhetorical techniques also influenced the standard views later generations had of these scientists' work. One way to examine these views is to turn to modern textbooks. Although all books published in recent years would seem to be outside the bounds of my analysis—they obviously had no influence on either Darwin or Huxley—I find them

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[112] Darwin, *Notebook E*, 69. http://darwin-online.org.uk/content/frameset?pageseq=1&itemID=CUL-DAR124.-&viewtype=text
[113] Manier, *Young Darwin*, 51-52.
[114] Ibid., 52.
[115] Darwin, *Variation*, Vol. II, 357.

useful for highlighting contrasts. By backtracking from what students learn today, we can better understand what scientists learned then.

The textbook view of uniformitarianism is presented clearly in the opening pages of *The Earth: An Introduction to Physical Geology*.[116] This book claims that the idea was originally set forth by James Hutton, who wrote so badly that no one understood it until it was picked up by Hutton's friend, John Playfair, who clarified it in *Illustrations of the Huttonian Theory*.[117] Not until Charles Lyell wrote his *Principles of Geology*, however, did the idea really catch on. Lyell's approach was suggested by his subtitle: *Being an Attempt to Explain the Former Changes in the Earth's Surface, by Reference to Causes now in Operation*. The authors of *The Earth* explain that Lyell "was able to show more convincingly than his predecessors that the geological processes which are observed today can be assumed to have operated in the past."[118] This is the principle of uniformitarianism, and today it is "just as viable as in Lyell's day."[119] Almost any other textbook in use over the last couple of generations would present a similar story. A closer look at Lyell's work, however, suggests that something quite different was going on.

The historian Martin J. S. Rudwick has taken such a look, and one of his most important conclusions is that Lyell didn't present uniformitarianism as a single concept. Actually, Lyell himself did not even use the word; it was coined by William Whewell in an 1832 review of the book.[120] He did, however, refer to "uniformity," and Rudnick

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[116] Tarbuck and Lutgens, *The Earth*, 3-4.
[117] John Playfair was the brother of William Playfair, the inventor of statistical graphics. Both were pioneers in visual rhetoric for scientific and technical purposes.
[118] Tarbuck and Lutgens, *The Earth*, 4.
[119] Ibid.
[120] This is not the same as the 1831 review that I discuss in some detail in Chapter 5.

analyzed "Lyell's confusion—whether conscious or not—of several different meanings of 'uniformity.'"[121] Stephen Jay Gould extended Rudwick's analysis in *Time's Arrow, Time's Cycle*. These are Lyell's four meanings, as Gould explained them:[122]

1. Uniformity of law
2. Uniformity of process
3. Uniformity of rate, or gradualism
4. Uniformity of state, or nonprogressionism

For most of my purposes, we can group the first two and the last two. The uniformities of law and process are assumptions that guide historical sciences. The laws and processes of nature that we see today are the same as the ones that have always existed, so we can extrapolate from observations we make today and reach conclusions about the past (or the future, for that matter). This was not an especially controversial idea, even in Lyell's time.

Uniformity of rate and state are quite different from uniformity of law and process. Uniformity of rate means that the world has never changed much faster or slower than it is doing now, and uniformity of state means that the world has always been in its current state—it has always looked and worked the same way. As Gould explained, these "are testable theories about the earth—proposals that may be judged true or false on empirical grounds."[123] That is quite different from an assumption held *a priori*.

[121] Rudwick, *The Meaning of Fossils*, 188.
[122] Gould, *Time's Arrow*, 119-123.
[123] Ibid., 120.

The section in which Gould explained all of this was called "Multiple Meanings of Uniformity and Lyell's Creative Confusion." He argued that Lyell's use of these multiple meanings was a rhetorical technique. It success was astounding; it was

> perhaps the neatest trick of rhetoric, measured by subsequent success, in the entire history of science. He labeled all these different meanings as 'uniformity,' and argued that since all working scientists must embrace the methodological principles, the substantive principles must be true as well.[124]

Gould went on to say that "Lyell's rhetorical success must rank among the most important events in nineteenth-century geology."[125] I am less convinced of its success than Gould was. As Rudwick explained, Lyell's confusion led to a "discrepancy between these two levels of debate."[126] He made a strong case that many scientists accepted some of Lyell's claims but rejected others. If Lyell's ability to conflate these two aspects of uniformity were as successful as Gould claimed, the debate would not have existed on both of those levels. But even Rudwick argued—and I have no reason to doubt—that "Lyell's creative confusion about the 'uniformity of nature' ensured that successive editions of the *Principles* were read more widely and with greater enthusiasm than his elaborate technical argument might suggest."[127] This confusion has continued to this day, as we see in modern textbooks, which invariably fail to distinguish between Lyell's different meanings. The difference is significant, because not all of Lyell's definitions are still viable. Nonprogressionism, for example, is indisputably invalid: the earth has not always looked the same as it does now.

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[124] Gould, *Time's Arrow*, 119.
[125] Ibid.
[126] Rudwick, *Meaning of Fossils*, 188.
[127] Ibid.

Rudwick said nothing about whether Lyell's confusion was deliberate. Gould suspected that it was not.[128] I suspect that it was. As both Rudwick and Gould repeatedly mentioned, Lyell had been a practicing barrister, formally trained in principles of argumentation. He was also one of the world's foremost experts on geological theories and methods. It seems odd that someone with his knowledge of rhetoric and science would have used this rhetorical technique by accident. And it is in fact a recognized rhetorical technique, although it had not yet been named at the time. It is now called polysemy. More specifically, Lyell's technique was a kind of polysemy that Leah Ceccarelli[129] has referred to as "strategic ambiguity." She explained that "this form of polysemy is likely to be planned by the author and result in two or more otherwise conflicting groups of readers converging in praise of a text."[130] Lyell's purpose differed from the examples Ceccarelli gave. For her, the multiple meanings were intended to appeal to multiple audiences. For Lyell, the multiple meanings were intended to gain multiple assents. That is, the same audience would agree to more than one point, because the same word referred to more than one thing.

Lyell's techniques of argument are quite important to consider when analyzing the works of Darwin and Huxley, because both men were intimately familiar with them. Darwin's interest in Lyell's work seems to have started in 1831, when John Henslow suggested that he take the first volume of Lyell's *Principles of Geology* on board the *Beagle*. The other two volumes were mailed to him throughout the voyage. By the time

[128] Gould, *Time's Arrow*, 119.
[129] Ceccarelli, "Polysemy," 404.
[130] Ibid.

he returned home, he already had the respect of Lyell, who had read his papers and seen his specimens. Lyell had his, as well. In his *Autobiography*, Darwin said

> I had brought with me the first volume of Lyell's *Principles of Geology*, which I studied attentively; and this book was of the highest service to me in many ways. The very first place which I examined, namely St. Jago in the Cape Verde islands, showed me clearly the wonderful superiority of Lyell's manner of treating geology, compared with that of any other author, whose works I had with me or ever afterwards read.[131]

Lyell's manner of treating rhetoric was also useful.

Regardless of whether Lyell realized he was promoting two different ideas with his use of "uniformity"—one idea about scientific method, and one idea about the nature of the world—I am almost certain that Darwin did. Darwin's development of his theory of evolution required a firm acceptance of the first idea, and a firm rejection of the second. If Darwin had fallen for what Gould called Lyell's "trick of rhetoric," he could never have written the *Origin*. Accepting Lyell's uniformity of law and of method allowed Darwin to make observations on the *Beagle* and apply those observations to a theory that held true as far back in time as life had existed. Rejecting Lyell's uniformity of state allowed Darwin to set forth a theory of major organic change. If the world had always been as it is now, evolution could never have happened. And rejecting Lyell's uniformity of rate allowed Darwin's theory to account for the speeding up and slowing down of evolutionary change, depending on environmental factors. Having such a powerful example of strategic ambiguity—and even if its success fell short of Gould's claims, it was still quite successful—gave Darwin an understanding of how he could use the technique himself.

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[131] Darwin, *Autobiography*, 77.

In a broader sense, Darwin also learned of the importance of rhetoric in science. Even if we were to grant the existence of science without rhetoric, Lyell's *Principles* would not be it. This was unquestionably not a case of the facts speaking for themselves. Even in Lyell's day, fossil evidence was quickly making it difficult to argue for uniformity of rate, and still more difficult to argue for uniformity of state. It was evident that the earth had not always been changing at the same rate, and still more evident that it had not always been in its current state. As Rudwick pointed out, many of Lyell's peers were unpersuaded on these matters, so his rhetoric was not an unqualified success. But without powerful rhetoric, he would not even have been taken seriously. His opponents had both literal and figurative mountains of evidence against him.

Lyell's *Principles of Geology*, then, was a highly rhetorical work. This was true not only in the superficial sense of meeting the definition of "rhetoric"—for example, Smart's definition as "the right use of words with a view to inform, convince, or persuade."[132] It also demonstrated rhetoric as constitutive of knowledge. The knowledge of geology that was assembled in textbooks, and that has been passed down to today's geology students, is grounded in the rhetoric Lyell set forth.

Darwin's situation with evolution by natural selection was similar to Lyell's situation with uniformity. His peers accepted part of his argument (evolution) but not always all of it (natural selection). Unlike Lyell, Darwin did not conflate or confuse the two parts of his argument. On the contrary, he repeatedly made them as distinct as possible. But like Lyell, Darwin needed powerful rhetoric if he was going to be taken seriously. One of his rhetorical techniques was strategic ambiguity.

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[132] Smart, *Metaphysics*, 87.

Huxley, like Darwin, was quite familiar with Lyell's work. As we will see in Chapter 4, he seems to have given Lyell some feedback on a draft of *The Geological Evidences of the Antiquity of Man* a few months before it was published. In this work, Lyell reversed his earlier view on uniformity of state. He was writing after the *Origin* had been published, and it was no longer possible for him to entertain the notion that species had never changed over time. He accepted evolution—including, under Huxley's influence, the evolution of man—but not natural selection as the mechanism for it. Huxley had also read Lyell's *Principles* long before, and he knew of Hutton's work (although it is not clear to me whether he read it in the original, which was famously badly written, or in Playfair's interpretation of it). Was he aware that Lyell's use of "uniformity" was ambiguous—and so, by extension, was the principle of uniformitarianism? Unlike Rudwick and Gould, Huxley never referred to Lyell's "multiple meanings." But in unpacking some implications of the concept, he showed a highly nuanced understanding of its different aspects.

In his *Lectures on Evolution*, Huxley examined the hypothesis that someone going back in time, no matter how far, "would see a world essentially, though perhaps not in all its details, similar to that which now exists."[133] This was what Gould called uniformity of state. Then Huxley explained, "This view was held more or less distinctly, sometimes combined with the notion of recurrent cycles of change, in ancient times; and its influence has been felt down to the present day." The combination of linear time and recurrent cycles of change was the theme of Gould's book, reflected in its title: *Time's Arrow, Time's Cycle*. Huxley next added that this view "is not inconsistent with the

[133] Huxley, *Lectures on Evolution*. http://aleph0.clarku.edu/huxley/CE4/LecEvol.html

doctrine of Uniformitarianism." Note the phrasing here. Huxley did not say that the doctrine *was* uniformitarianism. Uniformitarianism is not the same as uniformity of state, but it is consistent with it. I take this to mean that uniformity of state can be incorporated into a doctrine of uniformitarianism. This doctrine, Huxley said, "was held by Hutton, and in his earlier days by Lyell." By "earlier days," I assume Huxley meant before Lyell's *Antiquity*, in which Lyell finally rejected the uniformity of state (but not, I should add, the other three definitions Rudwick and Gould identify). Huxley's view of uniformitarianism here was radically different from that in almost all modern textbooks. He saw it as a doctrine already on its way out, even in Lyell's lifetime, not something that is "just as viable as in Lyell's day." Uniformitarianism, Huxley argued, could imply the existence of an eternal world. Then he added,

> Not that I mean to say that either Hutton or Lyell held this conception–assuredly not; they would have been the first to repudiate it. Nevertheless, the logical development of some of their arguments tends directly towards this hypothesis.

Uniformitarianism, then, was based on multiple arguments, some of which might lead to conclusions that even its most noted proponents would reject. It was not a single concept, nor could it be reduced to something as simple as uniformity of state. So although Huxley did not understand the concept exactly as Rudwick and Gould did, I think it's clear that he did not conflate multiple definitions that had to be accepted or rejected *in toto*. Neither Huxley nor Darwin fell for Lyell's "trick of rhetoric"; on the contrary, they seem to have learned from it. We will see in Chapter 5 how this knowledge played out in their interactions with each other. In the meantime, before we can understand the interactions

between Darwin and Huxley, we need to understand the science that guided these interactions. We will take a look at that in Chapter 4.

Chapter 4: The Science

Just as the works of Darwin and Huxley did not exist in a rhetorical vacuum, neither did they exist in a scientific vacuum. Seeing the context of these scientists' writings is not simply a matter of being aware of the facts of science as they were known in the Victorian era. It is, rather, a matter of seeing how contesting claims were presented and which ones dominated. Before Darwin, the evolutionary claims of Jean-Baptiste Lamarck dominated the discussion. Darwin referred to him as "the first man whose conclusions on this subject excited much attention."[134] Unfortunately, not all of this attention was based on Lamarck's actual ideas. His views were misunderstood even in his own time, and these misunderstandings have continued to the present day. My goal in this chapter is to describe to a modern audience the scientific ideas surrounding the dispute between Darwin and Huxley. To do that, I need to discuss Lamarck's actual writings, which were readily available to Darwin, Huxley, and everyone taking part in the discussion. I also need to discuss how Lamarck's ideas were interpreted at the time—both correctly and incorrectly. To put these ideas in context for a modern audience, it might be best to start with modern misconceptions, which have been presented most clearly in popular textbooks. It is not my wish to criticize the textbooks themselves; they have simply set forth the standard view of today's community of biologists. Indeed, that is why I chose them. Consider, for example, *Biology: The Network of Life*:

> Jean-Baptiste-Pierre-Antoine de Monet de Lamarck, a French scientist of the late eighteenth century, believed that there had been a general *evolution* from simple

[134] Darwin, *Origin*, 3rd ed., xiii.

> one-celled organisms to highly complex organisms such as humans. Because he could not identify any mechanisms responsible for such changes, his scientific contemporaries dismissed his ideas as idle speculation.[135]

The second sentence is simply wrong. Lamarck identified several mechanisms. His lack of persuasion came from a lack of evidence, not from a lack of mechanisms. Moreover, this narrative implies an inaccurate (and unfortunately common) view of Darwin's role. It is not true, for the most part, that Darwin's contemporaries accepted evolution because he offered a mechanism. They were far more accepting of evolution itself than they were of his mechanism, natural selection.

Life: The Science of Biology offered a different view of Lamarck: "Lamarck was the first person to support the idea of evolution with logical arguments and was also the first person to put forth a hypothesis concerning the mechanisms of evolutionary change."[136] Recognizing that Lamarck did propose mechanisms for change is a step in the right direction. What was his hypothesis? The book explains:

> He suggested that living organisms have the ability to change gradually over many generations by the inheritance of structures that have become larger and more highly developed as a result of continued use or, conversely, have diminished in size as a result of disuse. For example, he suggested that water birds extend their toes while swimming, stretching the skin between them. This stretched condition, he thought, can be inherited by the offspring . . . .[137]

This "use and disuse" theory is what is now popularly referred to as Lamarckism—or, in its adjectival form, as Lamarckian. The theory is also referred to as the inheritance of acquired characteristics. The definition of Lamarckism on dictionary.com pulls these aspects together quite nicely: "the Lamarckian theory that characteristics acquired by

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[135] Mix, Farber, and King, *Biology*, 441. Emphasis in original.
[136] Purves, Orians, and Heller, *Life*, 14.
[137] Ibid.

habit, use, or disuse may be passed on to future generations through inheritance." The example of a blacksmith is more widely used than the water bird one, as we see in this recent case: "So, for example, a blacksmith could bulk up over years of hammering iron, then sire a bunch of thick-armed kids to join the family business."[138]

But although the inheritance of acquired characters was in fact part of Lamarck's theory, Lamarck "neither originated this idea nor claimed special credit for it. Indeed, the reality of the inheritance of acquired characters was not an issue for him or his contemporaries" (Burkhardt xxix). Nor was it an issue for Darwin or his contemporaries, and Darwin made no attempt to refute it. Chapter 5 of the *Origin* had a section called "Use and disuse, combined with natural selection." To Darwin, the use and disuse theory was something to supplement the mechanism of natural selection, not to be replaced by it. This section specifically mentioned water birds (although it focused on wings, not feet), just as Lamarck did. He included similar sections in some of his other works, most notably the *Descent*. And he seemed to accept Lamarck's blacksmith example in Notebook N: "An habitual action must some way affect the brain in a manner which can be transmitted. — this is analogous to a blacksmith having children with strong arms. — The other principle of those children, which chance? produced with strong arms, outliving the weaker ones, may be applicable to the formation of instincts, independently of habits."[139] So sometimes multiple generations can evolve strong arms by use and disuse, and sometimes they can evolve strong arms by the "other principle"—that is, natural selection. In this notebook, Darwin didn't seem to be questioning either of these

[138] Radiolab, "Leaving Your Lamarck." http://www.radiolab.org/2012/nov/19/leaving-lamarck/
[139] Darwin, *Notebook N*, 42. http://darwin-online.org.uk/content/frameset?pageseq=1&itemID=CUL-DAR126.-&viewtype=text

mechanisms, only the mechanisms for developing habits and instincts. Far from viewing use and disuse as a false theory to be refuted, Darwin viewed it as an obviously true theory that could bolster any weaknesses in his own theory of natural selection.

Evolution: Process and Product gave much more information about Lamarck than the general biology textbooks I mentioned earlier. It discussed the use and disuse theory in some detail, then added another alleged aspect of Lamarck's theory:

> Lamarck believed that the environment acted by means of the nervous system; in other words, the desire of the animal leads to the formation of new structures. In its crudest form, this would mean that a person who muses "Birds can fly, so why can't I?" should sprout wings and take to the air![140]

Stephen Jay Gould blames Lamarck's former colleague, Georges Cuvier, for this misrepresentation of Lamarck's views: "Cuvier's rhetoric was brilliant, his characterization grossly distorted."[141] Lamarck did not, of course, believe in this crude form of the theory, and neither did any other sane person, at his time or any other. Nor, as we will see, did he believe the less obviously silly idea that the desire of the animal had much effect on new structures. Certainly, Darwin didn't, either.

If Lamarck set forth a theory of evolution by use and disuse, and Darwin's own theory involved evolution by use and disuse, was Darwin's *Origin* just a modification of Lamarck—perhaps with natural selection added? Lyell thought so. In the introduction to his *Geological Evidence of the Antiquity of Man* (1863), he referred to "the recent modifications of the Lamarckian theory of progressive development and transmutation, which are suggested by Mr. Darwin's work on the 'Origin of Species by Variation and

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[140] Dodson and Dodson, *Evolution*, 109.
[141] Gould, *Structure*, 172.

Natural Selection.'"[142] Huxley disagreed strongly. In a letter to Lyell in 1862, in which he was apparently responding to a draft of Lyell's book, he said, "I should no more call his doctrine a modification of Lamarck's than I should call the Newtonian theory of the celestial motions a modification of the Ptolemaic system."[143] Lyell seems to have remained unconvinced, and the published version stood as I quoted above.

Darwin himself believed that he offered something quite different from Lamarck. As far back as 1844, when Darwin first proposed his idea of evolution to Joseph Hooker, he said, "Heaven forfend me from Lamarck nonsense of a 'tendency to progression' 'adaptations from the slow willing of animals' &c,—but the conclusions I am led to are not widely different from his—though the means of change are wholly so—."[144] It was, of course, the "means of change" that made Darwin's ideas revolutionary. People who focused only on his conclusions, like Lyell, assumed that Darwin had little new to offer. People who focused on the means of change, like Huxley, were much more impressed. This was true even though, in the very same letter to Lyell just mentioned, Huxley admitted that Darwin's idea for a means of change might be incorrect.

Darwin said little about Lamarck in the first edition of the *Origin*, and he certainly didn't present his ideas as a modification of Lamarck's. Beginning with the third edition, he had more to say. In the "Historical Sketch" that he added at this point, he spoke of Lamarck as a "justly-celebrated naturalist."[145] After listing some areas of agreement, such as the validity of the analogy between natural and artificial selection, the difficulty of

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[142] Lyell, *Antiquity*. http://www.gutenberg.org/files/6335/6335-h/6335-h.htm
[143] Huxley, letter to Lyell. http://aleph0.clarku.edu/huxley/letters/62.html
[144] Darwin, letter to Hooker. http://www.darwinproject.ac.uk/entry-729
[145] Darwin, *Origin*, 3rd ed., xiii.

distinguishing species and varieties, and the use and disuse theory, Darwin explained where he and Lamarck parted company: "he likewise believed in a law of progressive development; and as all the forms of life thus tended to progress, in order to account for the existence at the present day of very simple productions, he maintained that such forms were now spontaneously generated."[146] This disagreement created problems that led to other revisions in the *Origin*.

The law of progressive development, innocent as it may have sounded, had a major obstacle on the road to acceptance for any theory of evolution. It seemed fairly minor in 1801, when Lamarck first published his theory. If organic change is always progressive—that is, if evolution always moves from the simple to the complex—how is it that simple organisms still exist? Lamarck's answer was new forms were routinely generated out of nothing. Humans come from a long line of descent; earthworms come from a short one. Even in 1859, when the first edition of the *Origin* was published, this answer created no major problem. Louis Pasteur had just started his correspondence with Félix Pouchet, discussing spontaneous generation. But in February of 1860, Pasteur published the first of a series of papers showing, with more and more evidence, that spontaneous generation didn't happen. So beginning with the third edition of the *Origin*, in 1861, Darwin commented wryly, "I need hardly say that Science in her present state does not countenance the belief that living creatures are now ever produced from inorganic matter."[147] He emphasized that his theory of natural selection did not depend on spontaneous generation, because it did not assume that evolution is inherently

[146] Darwin, *Origin*, 3rd ed., xiii.
[147] Ibid., 135.

progressive. There is no inherent survival advantage in complexity, so there is no reason for simple creatures to become more complex unless they find an ecological niche that requires complexity.

In his letter to Hooker, besides distancing himself from the tendency to progression, Darwin also distanced himself from the idea of adaptations from the slow willingness of animals. The idea here is that organisms change because they will it to happen. He continued to push back against this view in the introduction to the *Origin*, where he said it was "preposterous" to suggest that the structure of the misseltoe [*sic*] is due to "the volition of the plant itself."[148] It is somewhat puzzling that Darwin would have found it necessary to say that. Certainly it is true that Darwin was not alone in thinking this was part of Lamarck's theory. Wallace, for example, mentioned it in his Linnean Society paper of 1858 (presented jointly with Darwin's).[149] But it's such a strange notion that I have trouble believing Lamarck would have suggested it seriously. His most detailed explication of his theories on animal development was in a rather hefty and wide-ranging tome called *Philosophie zoologique* (*Zoological Philosophy*), published in 1809. This work contained a chapter called *De la volonté* ("From the Will"). Nowhere in his discussion of the will did Lamarck say anything about using it to promote development. In fact, the chapter strongly implied that the vast majority of evolutionary development could not be driven by the will, because most animals don't have it, and even those that do (including humans) don't always use it. Lamarck has been so widely

[148] Darwin, *Origin*, 3.
[149] Wallace, "Tendency of Varieties." http://wallacefund.info/content/1858-darwin-wallace-paper

misunderstood that I think it would be best to let him speak for himself. This was the introductory paragraph to his chapter on the will:

> *Je me propose de prouver, dans ce chapitre, que la* volonté*, qu'on a regardée comme la source de toute action, dans les animaux, ne peut avoir d'existence que dans ceux qui jouissent d'un organe spécial pour l'intelligence, et qu'en outre, à l'égard de ces derniers, ainsi qu'à celui de l'homme même, elle n'est pas toujours le principe des actions qu'ils exécutent*.[150] [151]

If the will doesn't even exist without an organ for intelligence, then it's obviously not required for evolution, or animals would never be able to evolve this organ. For that matter, plants would never be able to evolve at all. Lamarck would have agreed it was preposterous for plants to evolve by volition.

Nothing in this chapter absolutely contradicted the idea that the will could have some small role as a mechanism for evolution, but then, nothing in the rest of the book said that it does. The most we might be able to read into it involves the will as a secondary cause. That is, animals may will a certain motion repeatedly, and that motion may lead to development that is then passed on to offspring. There is a huge difference, however, between this process and the direct action of the will.

Granted, *Philosophie zoologique* was not Lamarck's only book, and Huxley argued that Lamarck's views were significantly different in 1794, when he published *Recherches sur les causes des principaux faits physiques* (*Research on the Causes of Principal Physical Facts*), as well as in 1776, when he wrote that work. But Huxley's point deals mainly with spontaneous generation, which Lamarck did not then accept. In

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[150] Lamarck, *Philosophie zoologique II*, 330. Emphasis in original.

[151] "I propose to prove, in this chapter, that the *will*, which was previously regarded as the source of all action in animals, can have no existence except in those with a special organ for intelligence, and besides, with respect to this last point, it is the same for men, the will is not always the principle of the actions performed."

fact, Lamarck's view at the time was that "*je crois qu'il est aussi impossible à l'homme de connôitre la cause physique du premier individu de chaque espèce*,"[152] [153] which was remarkably similar to Darwin's view in the *Origin* and the *Variation*. As far as Lamarck's views on evolution by volition, his *Recherches* said nothing on that. As we have seen, his *Philosophie zoologique* presented evidence that opposed this view. But even though Huxley spoke highly of the *Philosophie*, calling it "very remarkable" and "still worthy of attentive study,"[154] he elsewhere presented exactly the same view of Lamarck as everyone else did. In one of his lectures to working men, he paraphrased Lamarck as saying that "it is a matter of experience that an animal may be modified more or less in consequence of its *desires* and consequent actions."[155] (Huxley then gave the blacksmith example.)

It turns out that I am not the first to see the discrepancy between Lamarck's actual words and the mid-nineteenth-century misrepresentation of them. Richard W. Burkhardt, Jr., listed "numerous demonstrations of its inappropriateness (e.g. Russell 1916; Cannon 1957; Jordanova 1976; Burkhardt 1977 and 1981; and Richards 1982)."[156] Note that the first of these sources was published in 1916. In the decades when Darwin and Huxley were writing, people assumed Lamarck's theory was based partly on evolution by will, and for my purposes, it does not matter that they were wrong. Darwin still needed to distance his theory from this one if he was to have any hope of being persuasive. His success has continued to this day, as we see in these lines from Richard Leakey:

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[152] Quoted by Huxley, *Darwiniana*, 211.
[153] "I think it is also impossible for man to know the physical cause of the first individual of each species."
[154] Huxley, *Darwiniana*, 212.
[155] Ibid., 467. Emphasis added.
[156] Burkhardt, *Philosophy of Lamarck*, xxx.

> Lamarck's theory was rather different [from Darwin's use-and-disuse theory] because he additionally postulated a desire for change, or *besoin*, which caused that change to happen in the organism itself and then to be passed on to its offspring. In Darwin's time this aspect of Lamarck's theory was not generally accepted, but virtually every scientist believed that characters acquired by use or disuse could be inherited.[157]

Leakey understood Darwin's theory better than the writers of the textbooks I've quoted, and he realized that Darwin did present a use and disuse theory. But even he failed to understand Lamarck's argument.[158]

The popularity of Stephen Jay Gould's books, as well as of his most famous theory, requires me to address another common misunderstanding of nineteenth-century evolutionary theories. In 1972, Gould and Niles Eldredge set forth their theory of punctuated equilibrium. Instead of progressing at a steady rate, they said, evolution speeds up and slows down, depending on changes in the environment. In "Evolution as Fact and Theory," a widely reprinted essay, Gould gave a common metaphor for the difference between the theory he and Eldredge proposed and previous ones: evolution happens "more like climbing a flight of stairs (punctuations and stasis) than rolling up an inclined plane."[159] The implication is that previous theories of evolution reject stasis and present a model of continuous progression.

Huxley, at times, implied this, as well. In his *Lectures on Evolution*, he discussed fossil evidence for evolution. Then he said,

> Facts of this kind are undoubtedly fatal to any form of the doctrine of evolution which postulates the supposition that there is an intrinsic necessity, on the part of

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[157] Leakey, Introduction to *Origin*, 17.

[158] The astute reader will have noticed that I have not explained how Lamarck came to be so widely misunderstood in the first place. Tracing this error would take me outside the bounds of this dissertation; I refer interested readers to Burkhardt's work. Gould offers a briefer discussion in his *Structure*.

[159] Gould, "Fact and Theory," 260.

> animal forms which have once come into existence, to undergo continual modification; and they are as distinctly opposed to any view which involves the belief, that such modification may occur, must take place, at the same rate, in all the different types of animal or vegetable life.[160]

But at no point did he say who held these views. The only scientists at the time who seriously argued for a continuous rate of change were geologists. Lyell, as we have seen, was one of them. No major evolutionist did, however.

The theories of both Darwin and Lamarck have been interpreted this way. In both cases, it's easy to see how this misinterpretation could have happened. But in both cases, it is in fact a misinterpretation. We've already seen that Darwin's model didn't require progression at all, at least not in the sense of increasing complexity. Neither did it require constant change. Gradual change, yes, compared with geological time, but not constant change. He explained quite clearly in the *Origin* 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."[161] In the third edition, he added that "geology tells us that some of the lowest forms, as the infusoria and rhizopods, have remained for an enormous period in nearly their present state."[162] Nor was there any reason to predict otherwise. If an organism is adapted to its environment, natural selection gives it no reason to change.[163]

Lamarck made the same point. In his *Philosophie zoologique*, he mentioned Geoffroy Saint-Hilaire's collection of mummified animals from Egypt. These animals

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[160] Huxley, *Lectures on Evolution*. http://www.gutenberg.org/files/2629/2629-h/2629-h.htm
[161] Darwin, *Origin*, 108-09. Emphasis added.
[162] Darwin, *Origin*, 3rd ed., 135.
[163] Species may still change over time—through genetic drift, for instance, although no one before the twentieth century understood that—but this change is much slower than natural selection allows.

were almost identical to today's animals, leading to the claim that organisms must not change over time.[164] Lamarck readily agreed that they were in fact unchanged. Then he added, "*Il seroit assurément bien singulier que cela fût autrement; car la position de l'Egypte et son climat sont encore, à très-peu près, ce qu'ils étoient à cette époque.*" [165] [166] With no change in the environment, there was no incentive for the birds to evolve. His law of progression said only that organisms must progress, not that they must do so continuously. Lamarck, Wallace, Darwin, Huxley, Gould, and Dawkins are all in agreement on this point: when organisms are adapted to a stable environment, their evolution remains relatively stable. When their environment changes, they evolve to adapt. For every standard theory of evolutionary change, the appropriate model is a staircase, not an inclined plane. Evolutionists argue about the steepness of the staircase, and sometimes about whether it goes up or down, but they almost never claim it's a smooth slide. That was as true in the nineteenth century as in the twentieth and today.[167]

If Lamarck was on people's minds when it came to biological change before Darwin, Lyell was on people's minds when it came to geological change. We have already discussed some of Lyell's rhetoric. It may not be obvious why his science was also relevant. Wasn't Darwin's theory of evolution a biological theory, not a geological

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[164] Although Geoffroy at first believed that his collection disproved Lamarck's theory (Rudwick *Fossils* 150-151), he later reconsidered. For the most part, he supported Lamarck's ideas. Lamarck was here not refuting Geoffroy, but the unnamed authors of a report in the *Annales du Muséum d'Histoire Naturelle.*

[165] Lamarck, *Philosophie zoologique I*, 70.

[166] "It certainly would be strange if it were otherwise, for the position of Egypt and its climate are still very nearly what they were at that time."

[167] Marlene Zuk, of the University of Minnesota's Department of Ecology, Evolution and Behavior, is at the forefront of research showing that under certain environmental conditions, evolution can take place much faster than Darwin believed. But this difference is one of degree, not kind. In fact, the degree is more in line with the views of Lamarck, who believed significant changes could be made in a couple of thousand years. See, for example, Zuk, M. and Tinghitella, R.M. "Rapid evolution and sexual selection." In: *Sociobiology of Communication*, P. D'Ettorre and D.P. Hughes, eds. Oxford: Oxford UP, 2008.

one? And yet I think I can make a case that Darwin was a geologist in two quite different senses, both of which relate to his theory. Explanations for both of these were inspired by information in Sandra Herbert's "Darwin the Young Geologist." In the first place, Darwin was formally trained as a geologist. He studied under John Henslow at Cambridge. Henslow gave him the chance to go with Adam Sedgwick on a geological field trip to Wales. It was through Henslow that Darwin was able to take part on the *Beagle* voyage—Henslow himself passed up the chance. Darwin routinely sent geological samples back to England, and it was this work that first made him known among serious scientists. Lyell, who was at the time the president of the Geological Society of London, mentioned Darwin's work in his presidential address on February 19, 1836. Darwin returned to England in October of that year. Thus, partly because of Lyell's efforts, Darwin was known as a geologist before he even made it home from his voyage.

In the other sense that Darwin could be considered a geologist, Lyell was influential, as well. As Herbert explained, he greatly expanded the definition of geology. Fossils were part of the study of geology before Lyell, but primarily just as a way of dating rock formations. Under Lyell, the question of species development came under the umbrella of geology. So in studying the origin of species, Darwin was studying geology as it was newly redefined.[168]

Probably the most important scientific ideas Darwin got from Lyell were the uniformities of law and process. These were not new to Lyell. Most members of the Geological Society of London accepted them at that time. But it was Lyell who

[168] In my view, this new definition seems to have taken geology back to the days of "natural history," when scientists—to use an anachronistic term—approached the pasts of animals and minerals with a similar methodology.

demonstrated to Darwin, step by step, how to work from these assumptions (scientifically), and how to build an argument from them (rhetorically). (This parenthetical division is purely heuristic; the actual process blended indistinguishably.) To modern readers, it may seem obvious that the current laws of nature are the ones we have always had. In Lyell's time, it was by no means obvious. In his review of the first volume, Whewell said that "all the ages of eternity would not enable us to coax what we *know* of modern changes into a semblance with the conditions of the past." [169] Little things like ocean waves and earthquakes and volcanoes could not cause the changes we saw in geology. This turned out to be exactly the kind of argument Darwin had to fight in his claim for a gradualistic view of evolution. Everyone agreed on the small changes, but it wasn't clear that these changes could add up to big ones. Even today, many people accept what they call microevolution (changes within species) without accepting macroevolution (changes between species). Like Whewell reviewing Lyell's book, they reject the notion that these small processes could accumulate into changes that seem different in kind.

Lyell's book had a much greater flaw, however, according to Whewell. The author put himself under the obligation to demonstrate three points. All of them had to be demonstrated under the conditions of causes still in operation. The first two aren't highly relevant to my current purposes, and Lyell covered them with varying success. This is the third: "That the changes from one set of animal and vegetable species to another, are also explicable or conceivable on the assumption of these same conditions." [170] Unlike the

[169] Whewell, "Review of *Principles of Geology*," 200.
[170] Ibid., 194.

first two, which Lyell at least attempted to deal with, "The third point he has at present left untouched." [171] In leaving it untouched, Lyell left this question open for a young Darwin to begin considering on his voyage.

Besides giving Darwin the question—which he would have run into anyway, after all—Lyell also gave Darwin a critical working assumption for answering it. Contrary to what Whewell implied, there was actually a significant difference between Lyell's view and "the well-known Huttonian doctrine" that James Hutton had set forth in 1785.[172] Hutton mentioned plants and animals, but said nothing about how they changed. He applied his argument only to, in Whewell's words, "the strata at the surface of the earth." Lyell extended Hutton's idea to suggest that not only the earth's strata, but the earth's life, had always operated under principles still at work. So if Darwin could observe certain principles suggesting how plants and animals changed from one species to another, he could reasonably assume that these principles had always been in action. Without that assumption, nothing he saw could be applied to previous geological eras, and he would have no logical foundation for any theory of evolution, by natural selection or anything else.

If Volume I of Lyell's *Principles* gave Darwin a question and a method for approaching it, Volume II gave him the scientific and rhetorical knowledge to begin answering it. It was in this volume that Lyell touched upon the species question. The title page included a quotation from Playfair's *Illustrations of Huttonian Theory*: "A change in the animal kingdom seems to be part of the order of nature, and is visible in instances to

[171] Whewell, "Review of *Principles of Geology*," 194.
[172] Hutton, *System of the Earth*.

which human power cannot have extended." This volume was devoted to a discussion of these changes, with frequent references to Lamarck. As we saw in Chapter 3, Lyell advocated the uniformity of state. Nowhere was this more clear than when he defined species, which "have certain distinguishing characters in common which will never vary, and which have remained the same since the creation of each species."[173]

But although Darwin obviously disagreed with Lyell on variations in species, he apparently agreed with Lyell's view on how to divide and explain the species question. Volume II of the *Principles* included chapters with section headings called "Geographical distribution of Animals" and "Geographical distribution and migrations of fish."[174] Compare that with "Geographical Distribution," a chapter title in Darwin's *Origin.* Similarly, Lyell's *Principles* had section headings called "Phenomena of hybrids" and "Hybrid plants"; Darwin's *Origin* had a chapter called "Hybridism." In one sense, Darwin was taking from Lyell knowledge about science. Hybridism and geographical distributions are scientific concepts. In another sense, this knowledge was more rhetorical than scientific. It was from Lyell that Darwin learned how to divide the problem so he could argue about it effectively. Deciding on categories of evidence was a rhetorical choice; nature did not present these categories automatically.

We have seen, then, that Darwin was working within the frame that Lyell defined for his geological research problem. But I believe it can also be useful to view Darwin as having broken away from geology when he took up the study of species development. For him at that time, geology became supplemental information for backing up answers

[173] Lyell, *Principles of Geology*, Vol. II, 3.
[174] Ibid., viii.

to his primary questions; it was not in itself the primary focus of his research. Distancing himself from Lyell's discipline was a critical move in his career. As Martin J. S. Rudwick pointed out, he needed to get out of Lyell's shadow.[175] In geology as traditionally defined, he was never likely to gain a bigger name than Lyell had. In biology, he had a chance—and the chance paid off. Huxley seemed to find that mildly surprising:

> Like the rest of us, he had no proper training in biological science, and it has always struck me as a remarkable instance of his scientific insight, that he saw the necessity of giving himself such training, and of his courage, that he did not shirk the labour of obtaining it.[176]

But it was his geological training more than his biological training that seems to have made the difference. He developed the most powerful idea in biology before he began formally studying the subject. It is certainly true, however, that his background in biology, once he had it, made it far easier for him to present evidence for his idea. In this respect, I disagree with Ruse, who argued that "there is no great difference between the *Essay* and the *Origin.*"[177] I grant that the core arguments of 1844's *Essay* and 1859's *Origin* are similar, in the same sense that the core argument of Patrick Matthew, from *Naval Timber and Arboriculture*, is the same as that given by Darwin and Wallace.[178] But Darwin in 1844 simply did not have the biological knowledge necessary to make his case as successfully as he did in 1859. If he had presented his idea then, it might well have been ignored for the same lack of evidence that Matthew's piece had—or even that Darwin's and Wallace's 1858 Linnean Society papers had.

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[175] Rudwick, "Geology," 517.
[176] Letter to Francis Darwin. Quoted in *The Life and Letters of Charles Darwin*. 347.
[177] Ruse, *Revolution*, 184.
[178] Darwin, *Origin,* xiv.

Breaking away from Lyell's discipline did not mean he broke away from Lyell himself. The two of them corresponded until Lyell's death, and both of them were members of the Athenæum Club (as was Benjamin Smart). Throughout his *Autobiography*, Darwin spoke highly of Lyell. Lyell's scientific and rhetorical influence on Darwin may have exceeded Huxley's, and like Huxley, Lyell was himself hugely influenced by Darwin. Darwin literally changed his view of the state of the world. He has done the same for countless others.

Science from rhetoricians

As we saw in the last chapter, at least one view of rhetoric suggests that it cannot be separated from the argument being conveyed. Rhetoricians influenced not merely some superficial techniques Darwin and Huxley used for persuasion, but the arguments themselves. This was true even when they were not discussing rhetoric as commonly defined. Consider, for example, Hugh Blair and his *Lectures on Rhetoric and Belles Lettres*. Although Blair was a Christian—indeed, he was a minister—he was not a Biblical literalist. His description of how language came to be, given in Lecture VI, implied a development of both language and humanity. He granted that language must originally have come from God. How else could we get around the paradox that humans could not come together in societies without language to guide them—but they would have no way of developing language without first interacting in societies?[179] But God did

[179] Blair, *Lectures*, 71.

not give humans a perfect system. Indeed, Blair suggested that the earliest language consisted of nothing more than cries of passion.[180]

Compare that with the Book of Genesis, in which some form of language apparently existed before anything else: "And God said, Let there be light: and there was light" (Gen. 1:3). Presumably, language existed as soon as God spoke. Even if we limit our discussion to language men knew, "Adam gave names to all cattle, and to the fowl of the air, and to every beast of the field"—before Eve even existed (Gen. 2:20). Naming all of the cattle and fowl and beasts required far more linguistic competence than "uttering those cries . . . which are the signs of fear," as Blair suggested the first users of language must have done.[181]

Smart, who was familiar with Blair's claim that God must have given man his first words,[182] chose to avoid the question altogether: "As to the question, whether speech was or was not, in the first instance, revealed to man [by God], we shall not meddle with it: we do not propose to inquire how the first man came to speak . . . ."[183] For our purposes, the descriptions of language development given by both men were otherwise similar enough.

The development Blair described—from cries of passion, to nouns, to inflections, to figures of speech (which he insists must have come about rather early in the development of language), to modern prose—was not merely a development of language. It was a development of humanity. "Think of the circumstances of mankind when

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[180] Blair, *Lectures*, 72.
[181] Ibid.
[182] Smart, *Metaphysics*, 6 fn.
[183] Ibid., 3.

Languages began to be formed," Blair asked of us. "They were a wandering scattered race; no society among them except families; and the family society too very imperfect . . . ."[184] Humanity developed as language did—and language developed in a branching form, not merely becoming more complex in a linear process, but branching into all of the different languages we have today. Blair was not explicit about this branching when it came to spoken language, and his reservations are understandable, given the lack of evidence we have of pre-written language. When it came to written language, however, he was quite clear that all alphabets derived from the same source, adding, "An invention so useful and simple was greedily received by mankind, and propagated with speed and facility through many different nations."[185] The use of "propagated" is noteworthy, because it suggests a metaphorical connection between language and breeding that we will see again.

Although Blair described a development of language, from primitive to advanced, that paralleled the development of humanity, he did not extend his discussion of language beyond humanity. Whately, however, did. In the first edition of his *Elements of Rhetoric*, he argued that animals can use language to communicate (although they cannot use it as an instrument of thought).[186] We don't know for sure that Darwin read this, although it's fairly likely. It was published the year Darwin started at Cambridge, and it covered the kind of material that Darwin, who then planned to become an Anglican parson, would have found relevant. What we do know is that Whately made the same point elsewhere, and Darwin quoted it in his *Descent of Man*:

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[184] Blair, *Lectures*, 71.
[185] Ibid., 94.
[186] From Bizzell and Herzog, *Rhetorical Tradition*, 1st ed., 840.

> But man, as a highly competent judge, Archbishop Whately remarks, "is not the only animal that can make use of language to express what is passing in his mind, and can understand, more or less, what is so expressed by another."[187]

His footnote said Whately was being quoted in the "Anthropological Review, 1864, page 158." Turning to this source, we see that the original quotation was from "On Instinct, Dublin, 1847." Darwin was evidently inspired to follow up on the original source. In his private list of "Books to be read," 1852-1860, he included "Whately, Richard. 1847. On instinct. A lecture. Dublin." The connection between language and animals not only interested him, but led him to believe it would be worth further study.

Combining the ideas of Smart, Whately, and Blair, we get a picture of the most primitive forms of language—language used only for basic communication—existing in animals, then developing through human society as humans themselves developed. Developing is not the same as evolving. The developmental connection, for Blair, was literal. Language development and human development coincided exactly. The evolutionary connection, for Darwin, was metaphorical. Language evolved in a similar way as organisms evolved. But the parallels between development and evolution are significant. For Darwin, they constituted part of his argument for evolution. In his *Variation*, after discussing the fossil record, Darwin said, "Ancient and extinct forms of life often show combined or intermediate characters, like the words of a dead language with respect to its several offshoots or living tongues. All these and other such facts seemed to me to point to descent with modification as the method of production of new groups of species."[188] Note that Darwin was here arguing only for "descent with

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[187] Darwin, *Descent*, 53.
[188] Darwin, *Variation*, Vol. I, 11.

modification"—that is, evolution. He was not arguing for natural selection. Both the fossil record and the analogy with languages were arguments only for evolution, and they were arguments that Huxley explicitly accepted.

As he continued his argument into a discussion of natural selection, Darwin was careful not to claim more than the evidence indicated:

> As the first origin of life on this earth . . . is at present quite beyond the scope of science, I do not wish to lay much stress on the greater simplicity of the view of a few forms or of only one form having been originally created, instead of innumerable miraculous creations having been necessary at innumerable periods; though this more simple view accords well with Maupertuis's philosophical axiom of "least action."[189]

Note the similarities to the claims we've seen about the origin of language. For both life and language, we don't know for sure how they started, so we have to speculate. In both cases, it's possible that God created a few forms to start with, and these forms then propagated and developed as they spread throughout the earth. In the *Origin*, when describing the ultimate origins of life, Darwin shifted throughout his revision process from Smart's approach to Blair's. In the final paragraph of the first edition, he said, "There is grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one . . . ."[190] Like Smart, he avoided the question of whether God did it. In the second edition, however, which otherwise had remarkably few changes (Darwin was very rushed to get it out), he added the phrase "by the Creator"

[189] Darwin, *Variation*, Vol. I, 12.
[190] Darwin, *Origin*, 490.

after "breathed."[191] Like Blair, he here suggested that God was necessary to get everything started.

The connections between life and language don't end there. In both cases, Blair and Darwin pointed to morphological evidence for connections between modern and extinct forms. For example, Blair pointed out that "the Greek characters, especially according to the manner in which they are formed in the oldest inscriptions, have a remarkable conformity with the Hebrew or Samaritan characters, which, it is agreed, are the same with the Phœnician, or the alphabet of Cadmus."[192] Darwin, in the *Variation*, said he "found in South America great pieces of tessellated armour exactly like, but on a magnificent scale, that covering the pigmy armadillo; I had found great teeth like those of the living sloth, and bones like those of the cavy."[193] Rhetoricians seem to have presented ideas on language development that are too similar to Darwin's ideas on species development to be dismissed as coincidence.

We can be especially sure it's not coincidence in the case of Smart. Darwin mentioned his work in an early notebook.[194] How early is debatable. Darwin said, in a comment obviously added later, that the notes were "written about the year 1837 & earlier." Certainly they were written long before the *Origin*. One of them began, "'Smart Beginning of a New School Metaphysics,'— give my doctrines about origin of

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[191] Darwin, *Origin*, 2nd ed., 490. Note that the page numbers are the same in both editions, suggesting the lack of changes in the book as a whole. Aside from this change, the most noteworthy one was the addition of Wallace's name to give him credit for the idea of evolution by natural selection—"in a conspicuous place," as he wrote to Lyell: http://www.darwinproject.ac.uk/entry-2647

[192] Blair, *Lectures*, 94.

[193] Darwin, *Variation*, Vol. I, 11.

[194] Darwin, "Old and useless notes." http://darwin-online.org.uk/content/frameset?pageseq=1&itemID=CUL-DAR91.4-55&viewtype=text

language." I take this to mean that Smart's work gave the same doctrines about the origin of language as Darwin himself did. Chances are that Darwin's views at that time had been influenced by his earlier reading of Blair. Darwin went on to say, "At least it appears all speculations of the origin of language.— must presume it originates slowly— if their speculations are utterly valueless— then argument fails— if they have, then language was progressive.—" If the word "life" were substituted for "language" in that note, it would reflect Darwin's views just as well. He closed the note with the point that "declension &c often show traces of origin—"

Decades later, when Darwin wrote the *Descent*, he could even more easily have switched words while remaining faithful to his views: "The survival or preservation of certain favoured words in the struggle for existence is natural selection."[195] He has almost repeated the subtitle of the *Origin*, except with "words" in place of "races." Note that he was now making a stronger claim than we saw before. He was now arguing not only that language and organisms both evolve; he was arguing that they both evolve by natural selection. He started out applying linguistic evolution to biological evolution, helping him understand that species evolved just as languages do; then he applied his ideas on the mechanism of species to the question of how languages evolve, and came up with the same answer: natural selection.[196]

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[195] Darwin, *Descent*, 60-61.

[196] The connection between the evolution of humanity and the evolution of language is still discussed, and not always as a metaphor. Consider, for example, Dan Dediu and Stephen Levinson, who have argued that a "reassessment of the antiquity of modern language, from the usually quoted 50,000–100,000 years to half a million years, has profound consequences for our understanding of our own evolution in general and especially for the sciences of speech and language."
http://www.frontiersin.org/Language_Sciences/10.3389/fpsyg.2013.00397/abstract

At the risk of seeming excessively speculative, I'd like to suggest another area in which Smart's style of thought might have influenced Darwin's approach to thinking about species. For Smart, traditional approaches to language viewed it as being made up of words—which it is, of course. But Smart found it often useful to consider words as the smallest units of unified thought. In some cases those are in fact words in the traditional sense. More often, they are sentences. In rare cases, they can be entire books or sets of books. In this wider sense, Smart preferred to put "word" in capitals, as when saying the Bible is the WORD of God.[197] As a prime example of this usage, he mentioned

> Mr. Whewell's work on the Inductive Sciences . . . — a tolerably long-winded [WORD], it will be said; — three thick, closely printed, octavo volumes! Not a whit longer than necessary, I answer, nor yet so long as necessary, to produce what is sought — that scientific state of intellect in the reader . . . which the accomplished author has attained.[198]

It is possible that Smart inspired Darwin to read Whewell and perhaps develop "that scientific state of intellect." We at least know that Darwin took notes on Whewell later in the same notebook in which he took notes on Smart. And there is no question at all that Darwin was heavily influenced by Whewell's ideas; he opened the *Origin* with a quotation from Whewell, and he referred to Whewell's work routinely after he first took notes on it. But Whewell's ideas were in the air anyway; even without Smart, Darwin could hardly have avoided them. My point goes beyond that.

Throughout his *Metaphysics*, Smart moved back and forth between analysis (which he considered the realm of logic) and synthesis (the realm of rhetoric). This is the kind of thinking he needed to see how words relate to WORDS. It is also the kind of

[197] Smart, *Metaphysics*, 55 fn.
[198] Ibid., 483 fn.

thinking Darwin needed to see how individuals and species related to a theory that explained the development of all life. In much the same way as Whewell's three-volume set consisted of a single WORD, Darwin's 490-page *Origin* was only the abstract of a single long argument. Indeed, he continued this kind of thinking not only throughout the *Origin*, but throughout every book he wrote for the rest of his life. This was true even in his last book, *Vegetable Mould and Worms*. Writing about earthworms may have seemed like a comedown after the *Origin* and the *Descent*, but it showed a remarkable consistency with his earlier thought processes—and with Smart's. Stephen Jay Gould made a related point in "Worm for a Century, and All Seasons." Tiny worms have a huge effect on the world's ecosystem. As Darwin put it in his conclusion, "It may be doubted whether there are many other animals which have played so important a part in the history of the world, as have these lowly organised creatures."[199] Small words make a WORD. Small worms make a world.

[199] Darwin, *Mould and Worms*, 313.

Chapter 5: The Interactions

Darwin and Huxley interacted extensively, both in person and in letters, for about thirty years. They exchanged ideas, debated, and built on each other's work. Any analysis that focused exclusively on their agreements or their disagreements, or that suggested the sole direction of influence was from Darwin to Huxley (or vice versa), would be rather limited. In this chapter, I will explain how Huxley built on some of Darwin's ideas, before Darwin picked up where Huxley left off and extended the ideas even further. Then I will step away from their areas of consistent agreement and turn to an area of inconsistent agreement: natural selection. Finally, I will analyze an area in which their disagreements began before they started corresponding on this issue, and were never resolved: saltation.

We have already seen that Darwin's view of evolution was influenced by reading the works of rhetoricians who didn't interest Huxley. Although Huxley didn't get these ideas from Blair, Smart, or Whately, he did get them from Darwin, and he included them in his own argumentation. In *Evolution and Ethics*, Huxley argued that "just as philologists infer former connection of races, and a parent language, to account for generic similarities among existing languages," we can do the same for similarities among existing species.[200] The phrase "former connection of races" is especially notable, because the term referred then not only to human races, but to species. Indeed, that's how Darwin used the term in the full title of his famous book: *On the Origin of Species by*

[200] Huxley, *Evolution and Ethics*, 62.

Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life. Granted, it's clear that Huxley was here referring to human races, but the word choice leads the reader to extrapolate from human races to species-level races. The use of "generic" does the same thing in extrapolating from generic languages to biological genera.

Huxley was far more enthusiastic than Darwin about extending Darwin's ideas on human evolution. In the first edition of the *Origin*, Darwin sidestepped this issue, saying merely, "Light will be thrown on the origin of man and his history."[201] By the sixth edition, he was willing to intensify that statement: "Much light will be thrown on the origin of man and his history."[202] By then, he had written *The Descent of Man*, and the intensifier seemed justified. But in the twelve years between the first editions of the *Origin* and the *Descent*, it was Huxley, not Darwin, who had the most to say about human evolution.

Huxley expressed his views of human evolution in a series of lectures and essays, starting soon after Darwin published the *Origin*. In 1863, he gathered several of these short works into *Evidence as to Man's Place in Nature*. Perhaps the most controversial chapter was the second one, "On the Relations of Man to the Lower Animals,"[203] first published in 1861. He makes his topic clear in his opening sentence:

> The question of questions for mankind—the problem which underlies all others, and is more deeply interesting than any other—is the ascertainment of the place which Man occupies in nature and of his relations to the universe of things.

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[201] Darwin, *Origin*, 488.
[202] Darwin, *Origin*, 6th ed. 428.
[203] Huxley, "Relations of Man." http://www.gutenberg.org/files/2932/2932-h/2932-h.htm

How can we answer this? Huxley developed his argument by illustrating the development of an organism. It mattered little which organism—snake, dog, cat, bird—all of them develop in an egg that is pretty much the same regardless of whether it's inside the mother's body or outside. Then he argued that "it is a general law, that, the more closely any animals resemble one another in adult structure, the longer and more intimately do their embryos resemble one another . . . ."[204] With the stage set, he then moved on to a more specific question, asking "what results are yielded by the study of the development of Man. Is he something apart? Does he originate in a totally different way from Dog, Bird, Frog, and Fish . . . ?" Huxley's answer, of course, is no. Humans and animals develop in about the same way. "Indeed, it is very long before the body of the young human being can be readily discriminated from that of the young puppy . . . ." And not only are humans and lower animals similar in their development, humans and apes are especially so. Thus, Huxley concluded:

> Startling as the last assertion may appear to be, it is demonstrably true, and it alone appears to me sufficient to place beyond all doubt the structural unity of man with the rest of the animal world, and more particularly and closely with the apes.[205]

By implication, humans are related to other animals, and particularly to apes. Darwin had, at that time, made no such claim.

Although Huxley's claim was more attention-getting to the general public than Darwin's ideas in the *Origin* were, it rested on stronger evidence. To serious scientists, it was less controversial. Huxley's strongest claim in this essay was only that humans are structurally apes. Certainly there were those who objected to that claim. Richard Owen,

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[204] Huxley, "Relations of Man." http://www.gutenberg.org/files/2932/2932-h/2932-h.htm
[205] Ibid.

superintendent of natural history at the British Museum, comes to mind immediately, and in this book, Huxley specifically refuted many of Owen's arguments. But Huxley had visible evidence for the structural similarities. Darwin had no such evidence for the validity of natural selection.

Developmental and structural similarities are not, in themselves, guarantees of evolution. People had long known that humans and other mammals developed in similar ways—giving live birth, drinking mother's milk, gaining strength and size, and so forth. Likewise, it was no secret that, say, human eyes and dog eyes are similar. That didn't mean that humans and dogs shared a common ancestor. But given that the structure of other animals evolved over time, and given also that human structures are similar to other animals' structures, "there would be no rational ground for doubting that man might have originated . . . by the gradual modification of a man-like ape . . . ." Ultimately, then, the question came down to the validity of Darwin's hypothesis. "But here we enter upon difficult ground," Huxley said, "and it behooves us to define our exact position with the greatest care."[206]

If structural evidence were the only relevant consideration, according to Huxley, Darwinism would be quite solid. It is here that Huxley, in spite of his care, introduces an ambiguity. What is Darwinism? Is it the existence of evolution, or is it the validity of natural selection? Ernst Mayr has pointed out, correctly, "When using the word 'Darwinism,' Huxley applied it as often as not simply to the theory of evolution by common descent."[207] That's in contrast with the way modern biologists use the term,

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[206] Huxley, "Relations of Man." http://www.gutenberg.org/files/2932/2932-h/2932-h.htm
[207] Mayr, *Biological Thought*, 511.

which involves natural selection. But if that was what Huxley meant in this case, he was expressing a very rare (for him) doubt in the existence of evolution. Structure, as it happens, is not the only difference between species. Issues of fertility are relevant, as well. And there was no evidence at the time of new species that were infertile with each other.

This strikes me as an argument against natural selection, based on the analogy with artificial selection. But because Huxley never mentions either natural selection or the analogy itself in this essay, his argument remains ambiguous. As we will see, Darwin was also ambiguous in his discussion of this analogy, so it makes sense that his supporters would have been. In spite of this ambiguity, however, Huxley was far more explicit than Darwin had ever been about the connection between humans and other animals, and about the logical point that if we could in fact prove that other animals evolved, it would be reasonable to prove that humans did, too.

Huxley's claim of the connection between humans and apes is well known. Less often discussed is Huxley's contribution to arguments on the evolution of the horse. It may seem like a mere footnote in the argument, but it is useful to examine for Huxley's clarity and firmness in supporting evolution (albeit not natural selection). In the *Origin*, Darwin mentioned horses several times. When it came to their origins, "from reasons which I cannot give here, I am doubtfully inclined to believe, in opposition to several authors, that all the races have descended from one wild stock." [208] I do not know for sure what reasons Darwin had in mind at that point, in Chapter 1. But he came back to the issue in Chapter 5, where he used evidence from the stripes on various breeds to

[208] Darwin, *Origin*, 18.

conclude, “For myself, I venture confidently to look back thousands on thousands of generations, and I see an animal striped like a zebra, but perhaps otherwise very differently constructed, the common parent of our domestic horse, whether or not it be descended from one or more wild stocks, of the ass, the hemionus [onager], quagga, and zebra.”[209] But that says little about their distant origins, only that the ancestor of the horse must have been striped. Still, he wondered what else we might someday be able to say about the origins of horses. In Chapter 9, when discussing imperfections of the fossil record, he suggested "asking ourselves whether, for instance, geologists at some future period will be able to prove, that our different breeds of cattle, sheep, horses, and dogs have descended from a single stock or from several aboriginal stocks."[210]

Huxley immediately jumped on the topic of horses, and in a lecture about the *Origin* on February 10, 1860, he discussed them extensively—perhaps a little too extensively, in Darwin’s view. In a letter to Joseph Hooker four days later, Darwin said, “I succeeded in persuading myself for 24 hours that Huxley's lecture was a success.” Upon further reflection, however, he had to admit that Huxley “gave no just idea of natural selection,” and added, “It was really provoking how he wasted time over the idea of a species as exemplified in Horse.”[211] But as more evidence became available on the evolution of horses, Huxley was able to return to the subject with more positive results, answering Darwin’s challenge to prove the descent of the breeds.

In Part III of Huxley’s *Lectures on Evolution*, called “Demonstrative Evidence of Evolution,” nearly all of the evidence presented involved equine evolution. Huxley’s real

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[209] Darwin, *Origin*, 167.
[210] Ibid., 298-99.
[211] Darwin, letter to Hooker. http://www.darwinproject.ac.uk/entry-2696

point had little to do with horses, except as a convenient example. If he could demonstrate from the fossil record that horses evolved, he could demonstrate that evolution was real. He began by raising two points that Darwin himself had mentioned: first, that the imperfection of the fossil record makes any evidence we do find that much more significant; and second, that the discussion of horses includes "their allies, the ass, zebra, quagga, and the like."[212] He then went on, over a period of several pages, to echo Darwin's point about homologous structures. In the *Origin*, Darwin merely pointed out that the same bones are found in "in the arm of the monkey, in the fore leg of the horse, in the wing of the bat, and in the flipper of the seal."[213] Conspicuously absent was any mention of the arm of the human. Huxley not only mentioned humans explicitly, he matched the bones part by part: "Thus, the part of the horse's skeleton, which corresponds with that of the human hand, contains one overgrown middle digit, and at least two imperfect lateral digits; and these answer, respectively, to the third, the second, and the fourth fingers in man."[214] In doing so, he not only extended Darwin's ideas on equine evolution, he extended Darwin's ideas on human evolution.

The same logic that Darwin used to predict stripes on the ancestor of the modern horse, when applied to homologous skeletal structures, allowed Huxley to make predictions for what we could expect to find in the fossil record. These predictions were not trivial. In order for Darwin's ideas to function as a worthwhile hypothesis, they had to be useful for predictions that could be tested. What might these predictions be? Huxley explained that

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[212] Huxley, *Lectures on Evolution*, 32.
[213] Darwin, *Origin*, 200.
[214] Huxley, *Lectures on Evolution*, 34.

> the general principles of the hypothesis of evolution lead to the conclusion that the horse must have been derived from some quadruped which possessed five complete digits on each foot; which had the bones of the fore-arm and of the leg complete and separate; and which possessed forty-four teeth, among which the crowns of the incisors and grinders had a simple structure; while the latter gradually increased in size from before backwards, at any rate in the anterior part of the series, and had short crowns.[215]

He went on for another paragraph of detail, then said, "Let us turn to the facts, and see how far they fulfil these requirements of the doctrine of evolution."[216] The facts were taken from the fossil record, and they presented a chronology that remains basically sound to this day. Horses evolved in America, spread to Europe, died out in America "for some reason or other,"[217] and then were returned to America when European explorers arrived there. As they evolved, they went through the morphological changes mentioned above. Huxley concluded that "the history of the horse-type is exactly and precisely that which could have been predicted from a knowledge of the principles of evolution."[218] Much of this history was unknown when Darwin wrote the *Origin*. The *Orohippus*, which was the earliest equine fossil available at the time of Huxley's lecture, as well as the one that most effectively fulfilled his predictions, remained undiscovered until 1870.

It surprises me that the *Orohippus* is not more widely discussed today. Its discovery, ten years after Huxley began his lectures on horses, is one of the earliest effective cases that demonstrated the predictive power of evolutionary theory.

By the time Darwin wrote the *Descent*, Huxley had already paved the way for discussing homologous structures between humans and other animals. The line from the

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[215] Huxley, *Lectures on Evolution*, 36.
[216] Ibid.
[217] Ibid., 39. This is the only point on which Huxley seems rather vague.
[218] Ibid., 40.

Origin quoted earlier, in which Darwin specifically left out humans, became "the hand of a *man* or monkey, the foot of a horse, the flipper of a seal, the wing of a bat, &c."[219] He picked up another line of argument from the *Origin*—about stripes on ancestral horses—and used it to make an argument about human evolution that he wouldn't have touched before Huxley paved the way with his anatomical studies:

> It is quite incredible that a man should through mere accident abnormally resemble, in no less than seven of his muscles, certain apes, if there had been no genetic connection between them. On the other hand, if man is descended from some ape-like creature, no valid reason can be assigned why certain muscles should not suddenly reappear after an interval of many thousand generations, in the same manner as with horses, asses, and mules, dark-coloured stripes suddenly reappear on the legs and shoulders, after an interval of hundreds, or more probably thousands, of generations.[220]

The *Descent* was not the first time that Darwin returned to horses' stripes after writing the *Origin*. In the *Variation*, he had an entire chapter on horses and asses, including nine pages of information on their stripes—much of that in the small print he used for supplementary detail.[221] Nowhere in there, however, did he say anything about human evolution.

With the taboo of human evolution out of the way, other comparisons between humans and horses presented themselves. Darwin explained that some races of humans had gone extinct and had been replaced in those areas by other races, just as the fossil horse became extinct from South America, "to be replaced, within the same districts, by countless troops of the Spanish horse."[222] I don't mean to imply that Darwin received this information directly from Huxley, but it seems apparent that Darwin was continuing

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[219] Darwin, *Descent*, 31. Emphasis added.
[220] Darwin, *Descent*, 129.
[221] Darwin, *Variation*, Vol. I, 56-64.
[222] Darwin, *Descent*, 239.

the kind of argument that Huxley had made repeatedly. His next book, *The Expression of the Emotions in Man and Animals*, kept up the habit of comparisons between men and horses. In discussing the relation between voluntary and involuntary movements of animals at various levels of sophistication, he pointed out that "when a man or horse starts [is startled], his heart beats wildly against his ribs, and here it may be truly said we have an organ which has never been under the control of the will, partaking in the general reflex movements of the body."[223] The book as a whole seeks to minimize the distinction between humans and other animals; this example is only one of hundreds, and it's precisely the sort of thing Darwin avoided before Huxley made his comparisons between humans and other apes.

Disagreements on natural selection

Darwin was profoundly interested in Huxley's view of natural selection. In a letter on November 24, 1859, the same day the *Origin* was published, he wrote to Huxley: "Remember how deeply I wish to know your general impression of the truth of the theory of Natural Selection.—only a short note— at some future time if you have any lengthy criticisms, I sh[d] be infinitely grateful for them. You must know well how highly I value your opinion.—"[224] He was unaware that Huxley had already written to him the day before.

I quoted this letter earlier, but it deserves examination again, so we can see exactly what Huxley did and did not agree with in the book. "As to the first four

[223] Darwin, *Expression of Emotions*, 40-41.
[224] Darwin, letter to Huxley. http://www.darwinproject.ac.uk/entry-2550

chapters," Huxley said, "I agree thoroughly & fully with all the principles laid down in them."[225] The first four chapters lead up to and set forth Darwin's theory of evolution by natural selection. This would seem to be an acceptance of that theory.

Huxley went on, "I think you have demonstrated a true cause for the production of species & have thrown the *onus probandi* that species did not arise in the way you suppose on your adversaries—" "*Onus probandi*" simply means "burden of proof." But although a modern reader, untrained in Latin, might have preferred that Huxley use less Latin, this sentence may actually reflect a case in which more Latin could have clarified things. The phrase "true cause," in English, suggests a cause that happens to be true. By that reading, Huxley is agreeing with Darwin, saying he's right about the cause.

I think it is more likely, however, that Huxley was referring to the philosophical concept of *vera causa*. This assumption is borne out in a letter to Lyell, in which he explicitly used the phrase *vera causa* to describe natural selection.[226] Although the Latin phrase does in fact translate as "true cause," it has other implications, as we have seen. Mill, like Huxley, referred to natural selection as a *vera causa*,[227] and as we saw in Chapter 4, he considered it a mere hypothesis. How could he state that a cause was true if it was only hypothetical? According to Mill, natural selection "proved to be capable of producing effects of the same kind with those which the hypothesis ascribes to it." To say that it is *capable* of producing those *kinds* of effects is not the same as saying that it *does*

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[225] Huxley, letter to Darwin. http://www.darwinproject.ac.uk/entry-2544

[226] Huxley, letter to Lyell. http://aleph0.clarku.edu/huxley/letters/62.html : "If Darwin is right about natural selection–the discovery of this *vera causa* sets him to my mind in a different region altogether from all his predecessors–" Incidentally, Darwin used the phrase in the *Origin*, but never as an unambiguous description of natural selection.

[227] Mill, *System of Logic*, 499 fn.

produce those *particular* effects. Mill went on to point out that "the question of possibility is merely one of degree."[228] So for Mill, as long as the cause could possibly produce the right kind of effects, it's a *vera causa*. But what about Huxley? We've seen that he read Mill and ascribed to some of his ideas on logic, but that doesn't mean he used all of the same definitions. Fortunately, he gave use a definition in "We Are All Scientists": a cause which is competent to explain the observed phenomena.[229] But he emphasized that it "is a hypothetical conclusion, of the justice of which you have no absolute proof at all; it is only rendered highly probable by a series of inductive and deductive reasonings." So when he referred to natural selection as a "true cause," he may well have meant only that it could explain the observed phenomena, not that it did, and certainly not that it had been proven to do so. Indeed, his concern throughout his life seems to have been that Darwin showed only that natural selection could happen, not that it did happen. Thus, the apparent discrepancy between his initial letter and his later comments may not be as great as one might suppose.

I don't mean to imply, of course, that Huxley's views were perfectly consistent. He did, after all, say he agreed with the chapters that explained natural selection, when he elsewhere expressed reservations. These reservations began very shortly after the letter to Darwin. In "The Darwinian Hypothesis," first published on December 26, 1859 and later reprinted in *Darwiniana*, Huxley recommended a state of "active doubt."[230] So it took Huxley about a month to go from "I agree thoroughly & fully" to saying we couldn't

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[228] Ibid.
[229] Huxley, "We Are All Scientists."
[230] Huxley, *Darwiniana*, 20.

affirm the truth or falsehood of Darwin's hypothesis, and we would probably need at least another twenty years of research to tell.

In 1878, Huxley wrote "Evolution in Biology" (also reprinted in *Darwiniana*). This paragraph might suggest that a mere nineteen years, not twenty, were enough to show that natural selection has real validity: "How far 'natural selection' suffices for the production of species remains to be seen. Few can doubt that, if not the whole cause, it is a very important factor in that operation . . . ."[231] Compare that with this line from the last sentence in the introduction to the last edition of the *Origin* (1876): "I am convinced that Natural Selection has been the most important, but not the exclusive, means of modification."[232] Both rhetors agreed that it's important, but not everything.

None of that kept Huxley from saying, in 1893, that the theory's logical foundation was incomplete. This was not the same kind of hedging as we saw in the 1878 essay. His claim at that time was that natural selection is a cause of evolution; we just don't know how much of a cause. This claim is quite in line with modern science. As Dawkins put it, "All reputable biologists . . . agree that natural selection is one of [evolution's] most important driving forces, although—as some biologists insist more than others—not the only one."[233] This is quite different from saying we cannot logically prove it happens at all—something no biologist today, even intelligent design theorists such as Michael Behe, would claim.[234] I mention the views of modern science not to

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[231] Huxley, *Darwiniana*, 223.
[232] Darwin, *Origin*, 6th ed., 4.
[233] Dawkins, *Greatest Show on Earth*, 18.
[234] In *Darwin's Black Box: The Biochemical Challenge to Evolution*, Behe concedes that Darwin's idea could explain horse hooves, but not life's foundation (4). This claim strikes me as pretty much in line with Darwin's own view.

prove a point, but to clarify it. The views of Dawkins and Behe are irrelevant to my argument, but if Huxley put forth two sets of ideas, one of which modern science would accept and one of which it would reject, that helps, I hope, illustrate the discrepancy between the ideas.

Analogy

As we have seen, Huxley found Darwin's arguments on natural selection inconsistently persuasive. Before we can see why, we have to see exactly what those arguments were. What Richard Richards refers to as the "standard reading" of Darwin's argument is that Darwin argued by an analogy between artificial and natural selection. Just as humans can bring about change in organisms by allowing only certain ones to reproduce, so can nature bring about change in organisms by killing off the unfit ones before they can reproduce. But Richards himself rejected this reading. His rejection might not be relevant to my purposes, considering that he offered it in 1997, but he offered important evidence that the standard reading was not in fact standard in Darwin's day (although it was not unheard of, either).

Richards is not the only modern scholar to reject the standard reading. Peter Gildenhuys, in "Darwin, Herschel, and the Role of Analogy in Darwin's *Origin*," started with Richards's view and built upon it. Both Richards and Gildenhuys rejected the notion that Darwin's primary argument was an argument from analogy. Richard Dawkins, working from a different intellectual tradition, also rejected this notion. They each offered interesting points, but their interpretations of analogy would be helped by an understanding of another rhetorical device that figures prominently in the *Origin*:

polysemy. I will take a moment to examine their claims before applying their ideas to the specific views Darwin and Huxley seem to have held.

As Leah Ceccarelli has explained, rhetorical polysemy simply refers to "multiple meanings."[235] The same text will elicit different meanings from different readers, even if it's a scientific text. These multiple meanings are not necessarily the result of misreadings. On the contrary, as we will see, even intelligent readers can interpret exactly the same words in different ways. My focus here is on a specific type of polysemy that Ceccarelli called "strategic ambiguity."[236] She said that "this form of polysemy is likely to be planned by the author and result in two or more otherwise conflicting groups of readers converging in praise of a text."[237] Darwin was certainly aware that his writing could be ambiguous, and although he usually tried to avoid ambiguity, he did see its value in certain rhetorical situations.

I am not the first rhetorician to comment on Darwin's use of strategic ambiguity; John Angus Campbell did so forty years ago. He didn't use the term, but the concept was clear. In a 1975 essay, he argued,

> *The Origin* attempts to accommodate two opposite intellectual and theological currents. . . . Darwin's *Origin* can sustain either an agnostic or theistic reading. One may come away from it and in perfect candor say with Huxley that natural selection is the "death blow" to conventional teleology,[238] or one may in equal

[235] Ceccarelli, "Polysemy," 396.
[236] Ibid., 404.
[237] Ibid.
[238] The text here reads "teleology," but that seems out of place with the context, where Campbell is discussing theology, not teleology. Quoting this passage, Ceccarelli replaces that word with "theology" (407). In fact, however, Campbell's work choice is correct, however strange it may seem: "Teleology, as commonly understood, had received its deathblow at Mr. Darwin's hands" (Huxley *Darwiniana* 82). One could argue, of course, that a death blow for teleology also has an effect on theology, but if that was Campbell's point, it was not explicit.

> sincerity say with Kingsley that natural selection teaches us that God is so wise, He makes all things to make themselves.[239]

Darwin presented an argument that could be interpreted in different ways by different audiences, and either of these ways would be to his benefit.

Campbell discussed audiences who differ on theology, not philosophy. Specifically, he didn't consider audiences who might have philosophical disagreements about arguments by analogy. He saw strategic ambiguity in the *way* that Darwin argued from analogy, saying, "The image of nature as breeder seems to leave the door open for readers to emphasize different aspects of it and satisfy different theological bents."[240] But he saw no ambiguity in the *existence* of this argument from analogy. On the contrary, he took it as a given: "*The Origin* relies upon analogy in particular and imagery in general to develop an argument."[241] I believe that the debate over Darwin's use of argument by analogy stemmed from another example of strategic ambiguity.

Darwin wished to appeal to the popular, educated reader. In this he succeeded; the book sold out on the first day it was published. But he wanted to make his case to scientists and philosophers of science, as well. One option for appealing to multiple audiences is to use multiple arguments. Another is to use strategic ambiguity within the same argument. This approach allows different readers to look at the same words and obtain different meanings. Before taking a look at the different meanings scholars have taken from Darwin's analogy between artificial and natural selection, a brief look at the text itself might be instructive.

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[239] Campbell, "Polemical," 384-85.
[240] Campbell, "Polemical," 382.
[241] Ibid., 376-77.

In his chapter on natural selection, Darwin said, "As has always been my practice, let us seek light on this head from our domestic productions. We shall here find something analogous."[242] No sensible reader can deny that Darwin is here offering an analogy between natural selection and domestic productions.[243] The question is whether this analogy serves as an illustration or an argument. Upon a first reading, I assumed it was the latter. Darwin was clearly presenting an argument. He included an analogy while doing so. It seemed reasonable that the argument would be an argument by analogy. Moreover, later on the same page, he added, "But how, it may be asked, can any analogous principle apply in nature? I believe it can and does apply most efficiently . . . ."[244] This was not merely an argument that the principle is adequate (meaning it can apply) but that the principle is responsible (meaning it does apply).

But it's also possible to view this same passage as an illustration, intended only to clarify Darwin's point, not to prove it. He said we will "seek light," which helps us see his point better. Moreover, Darwin's introduction said that he would offer facts to *illustrate* his point.[245] One implication may be that Darwin didn't intend to prove his argument in this book, only to illustrate it. This seems to be the interpretation John Stuart Mill implied when he said, "Mr. Darwin has never pretended that his doctrine was proved. . . . And is it not a wonderful feat of scientific knowledge and ingenuity to have rendered so bold a suggestion, which the first impulse of every one was to reject at once,

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[242] Darwin, *Origin*, 111-12.
[243] Polysemy suggests multiple meanings, but not infinite ones. We cannot, for example, reasonably interpret Darwin as supporting the Genesis account of creation.
[244] Ibid., 112.
[245] Ibid., 2.

admissible and discussable, even as a conjecture?"[246] Mill's interpretation is different from mine, but both of us accept the point we believe Darwin makes. It was Darwin's strategic ambiguity that allowed him to be persuasive to these very different audiences.

With this in mind, let us now see how modern scholars have interpreted Darwin's analogy. We can identify at least four quite different interpretations:

1. Darwin argued by analogy between artificial and natural selection.
2. Darwin did not use an analogy between artificial and natural selection in his argument.
3. Darwin's argument was not an argument from analogy, but from causal analysis. It did depend on an analogy, however.
4. Darwin's argument was not an argument from analogy, but experimental induction. It used an analogy, but did not depend on it.

Note the essential point that all four approaches lead to an effective argument, even if they disagree on the kind of argument it is. Note also that each of these approaches existed in some form in the original discourse, making it relevant for my goal of historical reconstruction.

The first has been the most common view in recent years. It has been held by Campbell (a rhetorician), George Levine (a professor of English literature), Stephen Jay Gould (a scientist), and a number of philosophers, including Michael Ruse, Kenneth Waters, and Daniel Dennett. It's also the approach I took when I first read the *Origin*.

The second is the argument Richard Richards made. He claimed that Darwin argued by induction, not by analogy. Induction and analogy are not mutually exclusive,

[246] Mill, *System of Logic*, 499 fn.

as Smart implied in his definition of induction: "Induction is the bringing of particular facts within a notion by experiment, or within the scope of a notion by widely collected analogies."[247] So analogies can be used with inductive arguments, but these are not themselves arguments by analogy.

What reasons to we have to believe Darwin's analogy is not in fact an argument by analogy? The strongest reason Richards gave is that the analogy was used by people on both sides of the argument to suggest the immutability of species.[248] Lyell, who opposed the idea of evolution, used the analogy to support his claim, and Wallace, when arguing for evolution, presented the analogy as a contrary argument to be refuted.[249] And indeed, it is not difficult to see how the analogy could support the opposing view. As Huxley mentioned many times, artificial selection never produced a new species. If artificial selection and natural selection really were analogous, that would suggest that natural selection couldn't produce new species, either. Without mentioning the analogy, Huxley even stated explicitly that neither artificial nor natural selection had ever been proven to create new species.[250]

Richards conceded that some of Darwin's contemporaries thought he was in fact arguing by analogy; others didn't.[251] He didn't specifically mention Huxley. There is no question that Huxley saw Darwin as using an analogy. People who said otherwise had a view Huxley called "wholly untenable."[252] But the use of an analogy is not the same as

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[247] Smart, *Metaphysics*, 495-96 fn.
[248] Richards, "Inefficacy," 76.
[249] Ibid.
[250] Huxley, *Darwiniana*, 74.
[251] Richards, "Inefficacy," 95.
[252] Huxley, *Darwiniana*, 76.

an argument by analogy. As Richards pointed out in a section called "Argument vs Illustration," analogies work as well to illustrate points as to argue them.[253] In his view, Darwin was presenting the analogy merely to work from his audience's pre-existing understanding of selection, based on what they had observed on farms, to enhance their understanding of selection in the wild.[254] Even if the audience refused to accept Darwin's analogy altogether, his argument would still be sound. The question remains open of how Huxley viewed Darwin's analogy. I will return to it in a moment, after I have examined some other approaches.

Gildenhuys took the third approach I mention above. He began his essay with a discussion of Richards's work, but quickly moved on from there. He said that Richards's "positive assessment of the role of domestic organisms in the *Origin* is certainly too narrow."[255] A broader view—and one he considers more accurate—comes from seeing how Darwin's argument conforms to the philosophy of John F. W. Herschel in *A Preliminary Discourse on the Study of Natural Philosophy*.

Gildenhuys argued (correctly, I believe) that Herschel's approach was "a two-step process, beginning with the *explanation* of phenomena, through the exposure of their often hidden causes, and ending with the *generalization* of these causal processes to form laws of nature."[256] Instead of arguing by analogy, Darwin was arguing through this two-step process. Selection was the cause Darwin used to explain variation over time, and once that cause was set forth, Darwin could use it to generalize throughout a variety of

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[253] Richards, "Inefficacy," 94.
[254] Note this further example of the rhetoric of pre-existing ideas.
[255] Gildenhuys, "Role of Analogy," 594.
[256] Ibid. Emphasis in original.

situations, including intentional selection, unconscious selection, natural selection, and probably (though Gildenhuys didn't mention it) sexual selection. Thus, it becomes a law of nature.

Gildenhuys did not deny that Darwin used an analogy, only that he used an argument *from* analogy. (Even Richards admitted that "Darwin does indeed appeal to domestic breeding as analogy."[257]) As Gildenhuys argued,

> Though Darwin's generalization of selection depends on his establishing that artificial and natural selection are analogous insofar as they both operate through the same causes, Darwin's argument for the existence and power of natural selection is not an argument from analogy.[258]

Richard Dawkins also denied that Darwin used an argument from analogy. "Artificial selection," he argued, "is not just an *analogy* for natural selection. Artificial selection constitutes a true *experimental*—as opposed to observational—test of the hypothesis that selection causes evolutionary change."[259] This was not a point that Darwin discussed in the *Origin*, and no one seems to have thought of it when the work was first published. According to Richard Leakey, "Because natural selection had not been subjected to experimental proof, Huxley and others withheld wholehearted assent . . . ."[260] It had, of course—for thousands of years—but that wasn't the way readers initially saw the issue. Huxley, in a lecture on February 10, 1860, said that "a well conducted series of experiments very probably would" give him the evidence needed to persuade him of natural selection, implying that no such experiments had yet been done.[261] [262]

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[257] Richards, "Inefficacy," 95.
[258] Gildenhuys, "Role of Analogy," 604.
[259] Dawkins, *Greatest Show*, 66. Emphasis in original.
[260] Leakey, Introduction to *Origin*, 11.
[261] Huxley, "On Species and Races." http://aleph0.clarku.edu/huxley/SM2/Sp-R.html

Although most readers of the time missed the point Dawkins made, Darwin himself did not. This fact lends relevance to Dawkins' idea. If the idea of artificial selection as a form of experimentation were unique to Dawkins, it would be irrelevant to my analysis. Even if we grant its validity, it tells us nothing about Darwin's argument if Darwin didn't say it. But he did—after his readers, including Huxley, missed this point about the *Origin*. In the introduction to his *Variation*, published eight years after Huxley's lecture, he explained that humans "may be said to have been trying an experiment on a gigantic scale; and it is an experiment which nature during the long lapse of time has incessantly tried."[263] But even Darwin did not seem to view this point as critical to his argument. He did not see himself as introducing any new material in his introduction. As he explained in a footnote, "To any one who has attentively read my 'Origin of Species' this Introduction will be superfluous."[264] If all of the critical ideas of his introduction were in the *Origin*, and this idea was not in the *Origin*, it must not have been a critical idea. Moreover, he did not add it to the sixth edition of the *Origin*, published four years after the first edition of the *Variation*. And finally, although it's clear that Darwin saw artificial selection as an experiment, his view was not quite as literal as that of Dawkins. Compare "may be said to have been trying an experiment" with "constitutes a true *experimental* . . . test." Nevertheless, the fact remains that it was reasonable, even in Darwin and Huxley's time, to view artificial selection not merely as

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[262] In fact, experiments of the sort Huxley describes here have been done. Unfortunately for Huxley, who died in 1895, the earliest description I can find for them is in 1905. Hugo de Vries developed new species of Ornothera (evening primrose). His motivations could have been taken from Huxley: "If we want to know how species originate, it is obviously necessary to have recourse to direct observation. … All our hopes now rest on the result of experiments" (De Vries 516-517).

[263] Darwin, *Variation*, Vol. I, 3.

[264] Ibid., 2 fn.

an analogy for natural selection, but as an experiment, with all of the rhetorical strength that experimental results carry.

If it could be so many other things, why have so many readers interpreted it as an argument by analogy? One possibility is that the vast majority of Darwin's readers, over the last century and a half or so, have been simply confused. I have trouble accepting this possibility. Darwin wrote his book to appeal to a very wide audience, most of whom had no special scientific or philosophical background. He would not have constructed an argument that could be understood only by scholars at top universities.

Another possibility is that Gildenhuys was confused. I don't buy that, either. Herschel says much about analogy in scientific argument, and he makes clear that analogies can be used both to explain and to support these arguments. Gildenhuys knew what Herschel said about scientific arguments by analogy, and could see that Darwin is not taking this approach. Rather, Darwin was taking a different Herschelian approach—one that may make use of analogy, but that is not technically an argument from analogy. We can't tell for sure that Darwin intended to follow Herschel, but we do know that Darwin was influenced by him, and Gildenhuys seems accurate in his description of Darwin's argument.

We are left, then, with my claim that all of these views are acceptable interpretations of Darwin's argument. Most readers see Darwin as presenting an argument by analogy, and in general, they seem to find it persuasive. Richards, Gildenhuys, and other modern scholars believed Darwin was presenting a different kind

of argument, but they also found it persuasive. More to the point, it was persuasive to the nineteenth-century audiences who were familiar with that kind of argument.

It isn't really surprising that Darwin would have written his book in a way that would appeal to these very different audiences. Just as he knew that some of his readers would be theists and others would be atheists, he knew that some of his readers would be unfamiliar with Herschel's ideas and others (including Herschel himself) would be intimately familiar with them. The latter audience may have been a small percentage of his overall audience, but it was disproportionately important to the success of his idea, and well worth the effort of appealing to. The ambiguity of his text, allowing for these multiple interpretations, was an effective strategy indeed.

We still haven't addressed the question of whether Darwin's ambiguity was strategic. He was unquestionably aware that his discussion of natural selection was ambiguous, because Wallace pointed it out to him.[265] In Wallace's view, Darwin used the term "natural selection" in two ways. The first involved constantly comparing natural selection with man's selection, thus emphasizing the analogy. The second involved the personification of nature as doing the selecting. The first use was an analogy; the second use was personification. The first focused on man; the second focused on nature. Darwin conceded the point: "Your criticism on the double sense in which I have used Natural Selection is new to me and unanswerable; but my blunder has done no harm, for I do not believe that anyone excepting you has ever observed it."[266] And yet he did not take Wallace's suggestion to avoid the term in later editions of the *Origin*. He apparently felt

[265] Wallace, letter to Darwin. http://www.darwinproject.ac.uk/entry-5140
[266] Darwin, letter to Wallace. http://www.darwinproject.ac.uk/entry-5145

that not only had the ambiguity done no harm, it was not likely to in the future. Admittedly, he did not go so far as to suggest that it was beneficial.

He knew, however, that ambiguity could be beneficial in some situations. For him, natural selection depended on a struggle for existence, and he chose the word "struggle" only after careful thought. As he explained in the book he was working on before dropping it to write the *Origin* as an abstract:

> I shall employ the word struggle . . . including in this term several ideas primarily distinct, but graduating into each other, as the dependency of one organic being on another,—the agency whether organic or inorganic of what may be called chance, as in the dispersal of seeds & eggs, & lastly what may be more strictly called a struggle, whether voluntary as in animals or involuntary as in plants.[267]

The word "struggle" appears ninety-five times in the first edition of the *Origin*, including the title. Each time, it could have any one of three possible meanings, distinct but graduating into each other. In fact, the ambiguity of the gradations themselves suggests that the word could in effect be balanced between two meanings at once. He deliberately continued using "natural selection" ambiguously even after Wallace pointed it out, and he deliberately used "struggle" (and thus, "struggle for existence") ambiguously from the first time he chose the word. None of this is any guarantee that the particular ambiguities I discussed earlier in this chapter were deliberate, but it does suggest that Darwin was strategically ambiguous in his use of related terms.

To shed more light on how analogies were viewed at the time, it may help to see how the day's rhetoricians defined the term. They would probably have viewed Darwin's argument as one by analogy. It very nearly fit the ideal model set forth by Richard Whately in *Elements of Rhetoric*, the rhetoric textbook used at Cambridge when Darwin

[267] Darwin, *Natural Selection*, 187.

studied there. Not every apparent argument from analogy is a real one, Whately explained, and the term is often misused, even by "eminent writers (especially on Chemistry and Natural History)."[268] A true analogy is not a direct resemblance; rather, the word should strictly be used when "the two things (*viz.* the one *from* which, and the one *to* which, we argue) are not, necessarily, themselves alike, but stand in similar *relations* to some other things."[269] The example he gave was an egg and a seed. They aren't really alike, "but bear a like relation, to the parent bird and to her future nestling, on the one hand, and to the old and young plant on the other, respectively."[270] An analogy succeeds or fails on the strength of this relation. Whately emphasized that the question is not whether the difference between the two things is great or small; a small difference may destroy the analogy, and a large one may be simply irrelevant to the analogy (although it may be highly relevant in other contexts).

Huxley granted the connection between natural and artificial selection, but argued that the analogy failed on another ground. Artificial selection does not cause sufficient change over time to explain the origin of species. Therefore, given the connection that Darwin himself made, natural selection also does not cause sufficient change over time to explain the origin of species. Artificial selection can create change in varieties—from one breed of pigeon to another, say. But it cannot create a change in species. Species, by definition, are infertile with one another. This is a difference in kind, not in degree. Less than three months after the *Origin* was published, Huxley argued in a lecture that no one had yet proved that "modifications having the physiological character of species . . . have

[268] Whately, *Elements of Rhetoric*, 73.
[269] Ibid. Emphasis in original.
[270] Ibid.

ever been produced from a common stock."[271] By "physiological character," he meant issues of fertility, as opposed to morphological character, which involved issues of body form. Two organisms could appear to be very different morphologically, such as a Chihuahua and a Great Dane, but still be physiologically the same species.

Huxley seems to have discussed his objections with Darwin before giving this lecture. We don't know exactly what he said. We do have Darwin's reply, written on January 11, 1860. He agreed with the difficulty Huxley raised, but asked Huxley, "Will you oblige me by reading again slowly from p. 267–272."[272] These pages in the first edition of the *Origin* constituted a section called "Fertility of Varieties when crossed, and of their Mongrel offspring."[273] It began, "It may be urged, as a most forcible argument, that there must be some essential distinction between species and varieties . . . inasmuch as varieties, however much they may differ from each other in external appearance, cross with perfect facility, and yield perfectly fertile offspring."[274] This "forcible argument" was precisely Huxley's point. Varieties of organisms are fertile with one another; species are not. If artificial selection cannot create species that are infertile with one another, then neither can natural selection.

But the rest of this section complicated the issue. The distinction between species and varieties is not a simple matter of fertility. Sometimes species are not fertile when intercrossed.[275] Sometimes varieties are. In any case, Darwin concluded, "I do not think that the very general fertility of varieties can be proved to be of universal occurrence, or

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[271] Huxley, "Species and Races," http://aleph0.clarku.edu/huxley/SM2/Sp-R.html
[272] Darwin, letter to Huxley. https://www.darwinproject.ac.uk/letter/DCP-LETT-2649.xml
[273] Darwin, *Origin*, 267.
[274] Ibid., 267-68.
[275] Ibid., 269-70.

to form a fundamental distinction between varieties and species."[276] Darwin was here supporting his original analogy. There is no "essential" or "fundamental" distinction between varieties and species; producing new varieties (which Huxley agreed does happen) is differently only in degree from producing new species; therefore, the analogy as originally given still holds true.

Huxley was not persuaded, and went on to give his lecture on February 10, about a month after Darwin wrote that letter. Darwin needed a new approach. He wrote a note to himself: "Huxley objects to domestic vars. not being sterile. I answer in addition to my Book. Too ignorant of causes of sterility in species."[277] In the commentary on this note (and on the letter to Huxley), the Darwin Correspondence Project says that the book in question was *The Variation of Animals and Plants under Domestication*.[278] It could as easily have been the *Origin*. Darwin ended up adding a paragraph to the section he had asked Huxley to read in the first edition. It didn't mention Huxley, but it did explicitly address Huxley's arguments, and in doing so, it took another approach to his own argument by analogy. (It also explicitly referred to the answer Darwin gave in his note to himself.)

Again, Huxley claimed that the analogy would work only if artificial selection through domestic breeding could lead to varieties that became infertile when crossed, and that would then be considered new species. Darwin responds, in the revised paragraph of the sixth edition, "The real difficulty in our present subject is not, as it appears to me, why domestic varieties have not become mutually infertile when crossed, but why this

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[276] Darwin, *Origin*, 271-72.
[277] Darwin, personal note. Jan. 11, 1860.
[278] Darwin Correspondence Project. https://www.darwinproject.ac.uk/letter/DCP-LETT-2649.xml

has so generally occurred with natural varieties, as soon as they have been permanently modified in a sufficient degree to rank as species. We are far from knowing precisely the cause . . . ."[279] Darwin is now stepping away from the analogy and making a more direct statement. Perhaps artificial (domestic) varieties have not become mutually infertile. But natural varieties have, and that's more important for a theory of natural selection. Thus, we can, in this sense, skip over the information about artificial selection and jump directly to what we know about natural selection: natural species are in fact "generally" infertile with each other.

The paragraph continued to break from the analogy even further. Darwin mentioned that wild animals and plants are often rendered sterile when taken into captivity.[280] He carried this argument further in the *Variation*, giving eleven pages of examples.[281] Domestic animals, obviously, are not sterile in captivity. It would seem, then, that in issues related to sterility and fertility, there is no connection that would allow us to make an analogy between artificial and natural selection. In Whately's terms, this is a difference that destroys the analogy. But the analogy as a whole is not nullified. There is still a connection between artificial and natural selection. It just doesn't extend to Huxley's objection, which was based purely the failure of artificial selection to create varieties infertile with one another. And in any case, we're still too ignorant of the causes of sterility—whether in varieties or in species—to base any argument on that. Darwin made that point in the letter to Huxley, in his note to himself, and in the paragraph he

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[279] Darwin, *Origin*, 6th ed., 257.
[280] Ibid.
[281] Darwin, *Variation*, Vol. II, 131-142.

added to the *Origin* after he failed to persuade Huxley—even as he tried to argue from what little information he had available.

To sum up, then, Darwin set forth what Whately would classify as an argument by analogy, based on the connection between artificial selection and natural selection. One way to refute an argument by analogy is to argue for an invalid connection, and Whately devotes several pages to this approach. Huxley took the opposite approach, accepting Darwin's connection but arguing that the connection itself led to a weakness in the argument. If artificial selection really is analogous to natural selection, then natural selection must not be an adequate explanation for the origin of new species, because artificial selection cannot create new species.

We've seen two ways Darwin responded. One was to argue that the analogy was still valid, because the distinction Huxley made between varieties and species was false. The other was to argue that even if this weren't the case, and even if the analogy itself were invalid, the validity of natural selection stood undiminished. In both cases, responding to Huxley made his overall arguments stronger.

Disagreements on saltation

In Chapter 6 of the first edition of the *Origin*, Darwin mentions "that old canon in natural history of 'Natura non facit saltum ['nature does not make leaps'].'"[282] Calling it an "old canon" helped head off criticism of it; canonical ideas are harder to criticize than new ones. Darwin's next sentence added to his opponents' difficulty: "We meet with this

[282] Darwin, *Origin*, 194.

admission in the writings of almost every experienced naturalist." Anyone wishing to criticize Darwin must also criticize almost every experienced naturalist.

Huxley was up to the challenge. His most famous statement of disagreement with Darwin on saltation was in his letter of November 23, 1859, the day before the *Origin* was first published: "You have loaded yourself with an unnecessary difficulty in adopting 'Natura non facit saltum' so unreservedly. I believe she does make *small* jumps—"[283] When did Huxley decide this? Not as he was reading the *Origin*; whatever objections he may have had to preconceived notions, it's clear that he had a preconceived notion on this matter. In a letter to Lyell, he had written, "The finite and definite limits of species, genera, and larger groups, appear to me to be perfectly consistent with the theory of transmutation. In other words, I think *transmutation* may take place without *transition.*"[284] This lack of transition implies, to me, the presence of jumps.

But although it's clear that Huxley held this opinion before he wrote the above-quoted letter to Darwin, it is not clear how long he held it. Irvine gave the date for this letter as June 25, 1853—more than six years before the *Origin* was published.[285] We know this date was not a typographical error, because Irvine based his analysis of the letter on it. But The Huxley File, put together by Charles Blinderman and David Joyce of Clark University, gave it as June 25, 1859—less than five months before the *Origin*.[286] I don't have a facsimile of the original, and even if I did, it might not help, because letters were often not dated with the years. Full dates given are often added by editors, long after

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[283] Huxley, letter to Darwin. http://www.darwinproject.ac.uk/entry-2544
[284] Huxley, letter to Lyell. http://aleph0.clarku.edu/huxley/letters/59.html Emphasis in original.
[285] Irvine, *Apes, Angels, and Victorians*, 125.
[286] "A Hidden Bond." http://aleph0.clarku.edu/huxley/guide5.html

the letters were written. For what it's worth, the *Life and Letters of Thomas Henry Huxley* also gives a date of 1859. That could simply mean, of course, that both sources pulled from the same inaccurate information.

Either date would establish that for Huxley, saltation was a preconceived notion. The earlier date would of course mean that he had held this notion for longer, and perhaps suggest that it had become more firmly established for him. Either way, reading the rest of the letter makes clear that Huxley was actively rejecting *natura non facit saltum*, not merely implying an objection to it, as the quoted material above might suggest. He said that "in passing from species to species 'Natura fecit saltum [nature makes leaps].'"[287] To him, there was no reason to believe we must have a series of intermediate steps between forms. "On the contrary, in the history of the Ancon sheep, and of the six-fingered Maltese family given by Réaumur, it appears that the new form appeared at once in full perfection." This is the mindset Huxley seems to have had when he started reading the *Origin*, and to judge by the letter he wrote Darwin afterwards, nothing in the book changed his mind about that. In fact, he used exactly the same examples months later in "The Origin of Species," discussing them in significantly more detail.[288] We see again that Huxley's actual arguments reflect Smart's views on the rhetorical necessity of preconceived notions more than Huxley's own claim to avoid them.

Huxley's certainty about saltation, as he expressed it in that letter to Lyell, depended on the validity of transmutation—and he was much less certain of that. "I by no

[287] Huxley, letter to Lyell. http://aleph0.clarku.edu/huxley/letters/59.html
[288] Huxley, *Darwiniana*, 34-40.

means suppose that the transmutation hypothesis is proven or anything like it. But I view it as a powerful instrument of research. Follow it out, and it will lead us somewhere; while the other notion [spontaneous generation of advanced organisms, such as elephants] is like all the modifications of 'final causation,' a barren virgin."[289] In this respect, the *Origin* did change Huxley's mind. The transmutation hypothesis—evolution—was proven. But this was a much smaller rhetorical step than it might have seemed. He had already considered it better than any other available hypothesis.

So far, we have been viewing saltation and natural selection as separate arguments. For saltation to take place, evolution of some sort was required, so Huxley held off on his final opinion on saltation until he was persuaded of evolution. But he did not believe natural selection was required. He thought you could argue for one without the other, and his advice to Darwin, immediately after reading the *Origin*, was to argue only for natural selection. For Darwin, however, these arguments were not separate:

> Why should not Nature have taken a leap from structure to structure? On the theory of natural selection, we can clearly understand why she should not; for natural selection can act only by taking advantage of slight successive variations; she can never take a leap, but must advance by the shortest and slowest steps.[290]

Accepting his theory of natural selection, with all of its implications, required rejecting saltation. Huxley apparently missed this point when he accepted the true cause described in the first four chapters—that is, natural selection—but then rejected Darwin's views on gradualism.

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[289] Huxley, letter to Lyell. http://aleph0.clarku.edu/huxley/letters/59.html
[290] Darwin, *Origin*, 194.

Conclusion

With the Darwin industry as strong as it is, the need for yet another dissertation on the subject may not be obvious. But when the debate between its two most important rhetors has been so widely misunderstood for so long, it's worth revisiting. We have seen that Huxley was indeed Darwin's bulldog, but he was not merely that. He had a huge influence on Darwin's thinking throughout their interactions. The resulting changes in Darwin's thinking led to further changes in Huxley's thinking. Each man's ideas continuously built on the others, as well as on the ideas of people such as Lyell who also contributed to the discussion.

If this argument were of interest only to Darwin scholars, it would still be worth making. There are, after all, a great many Darwin scholars in a variety of disciplines. But putting this debate in the context of nineteenth-century science and rhetoric shows us that the conventional understanding of the history of rhetoric is, at best, misleading. Science did not only recently become a relevant topic for rhetoricians. It was of interest to them back in the Victorian era and before. Moreover, the rhetoricians' views of science were of interest to the scientists, themselves. In this respect, the rhetoric of science was more powerful than it is today—even though the field did not exist as a separate discipline.

Much remains to be done to explore the arguments made in this dissertation. In the debate itself, I have not yet sufficiently examined the errors that came from the interactions between Darwin and Huxley. Huxley pressed Darwin for an explanation of the mechanism behind evolution—what we would now call genetics. Gregor Mendel's work would have been a step in the right direction, but Darwin knew nothing of it. In the

Variation, he explains that he has "been led, or rather forced, to form a view"[291] to explain this sort of thing, adding:

> As Whewell, the historian of the inductive sciences, remarks:—"Hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of error." Under this point of view I venture to advance the hypothesis of Pangenesis, which implies that the whole organisation, in the sense of every separate atom or unit, reproduces itself.[292]

Darwin would not have been surprised to find that his theory was both incomplete and in error, but there were still useful aspects of it that Huxley was able to pull forth and that were carried over into the modern synthesis. This advancement of the theory is beyond the scope of this dissertation, but it may be of interest to other researchers.

Another point of possible interest to other researchers involves seeing how other scientists viewed the rhetoricians discussed here, especially Benjamin Smart. Michael Faraday, for example, studied under Smart extensively.[293] It would be worthwhile, I believe, to examine the knowledge he gained about rhetoric and how he applied that knowledge to his own very successful communication about science. That is my next major project.

Many others remain. Scholarship on Darwin has many questions left unanswered. I hope this dissertation will contribute to the conversation.

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[291] Darwin, *Variation*, Vol. II, 357.
[292] Ibid, 357-58.
[293] James, "Lecture Theater," 227.

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