ESSENTIAL CONTESTABILITY IN SCIENCE:

TAXONOMY AND THE MANTLE

OF DARWIN

By

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INTRODUCTION

For the last half-century, science has been under an immense philosophical scrutiny. Karl Popper, Thomas Kuhn, and many others have gone to great lengths to explain the inner workings of scientific practice. The focus of their scrutiny has varied from the way science *should* work, to the way science *does* work, with many combinations and comparisons of the two. Following Popper and Kuhn, philosophers of science have started to understand "science" as something that is performed by *scientists*. It is becoming more common for philosophers to account for the fact that scientists are *people*, with families, pets, mortgages, and most importantly, extra-scientific beliefs.

Proponents of the more traditional of these views hold that the extra-scientific beliefs and attitudes can occasionally work themselves into the otherwise immutable and pristine halls of science. The scientific methods, concepts, and theories become corrupted with subjectivity from the individuals' beliefs. Some philosophers of science, however, doubt that scientific methods, concepts, and theories are all that objective, immutable, and pristine to begin with. Throughout the history of science, they note, the behavior of scientists seems to reveal something very "un-scientific" about their practice. The plights of the Copernican, Newtonian, Darwinian, and Einsteinian "revolutions" are not testaments to patient and objective searches for truth; rather, they are tales of great battles. History crowns the successful as the enlightened righteous, while the losers, if not forgotten completely, are deemed ignorant and/or insolent.

It is in the pursuit of success that the role of the scientist *qua* person is most visible. Integral to the success of the individual scientist is the success of the tradition, paradigm, or school of which he or she is a member. When groups of scientists square off on foundational issues, their discourse often becomes rhetorical, and occasionally personal. As David Hull points out, the scientific success these groups desire has a methodological price:

> Scientists can succeed only if they are willing to break a few methodological rules--sometimes every rule in the book. However, they cannot finagle at all costs. Falsifiability does matter in science but not the falsifiability of disembodied propositions. What really counts is the falsifiability of scientists. To be successful, a scientist must be able to recognize clear threats to his or her position and respond appropriately. But the proper response to imminent refutation is not admitting defeat; it is changing one's position while retaining one's original terminology. Successful scientists are those who master the art of judicious finagling.[1]

All of the rhetorical activity in science--the "judicious finagling" Hull speaks of--is usually quite subtle. When the stakes are high, however, such as when the foundations of a discipline are being contested, the rhetorical arguments in science become as important as any arguments based on data.

Thomas Kuhn's theories on "scientific revolutions"[2] explain some of the most heated and important debates in the history of science, e.g., the Newtonian revolution in mechanics. Yet, his theory is virtually inapplicable to smaller scale scientific disputes that occur within relatively stable (non-revolutionary) sub-disciplines. One such field where Kuhn's structure does not seem to apply is taxonomy. Taxonomists have been embroiled in heated, sometimes bitter debates for the last fifty years, and have had to work in a field where the conceptual foundations are in flux more often than not. While the issues may be considered esoteric in comparison to the issues surrounding Newton's or Einstein's theories, they are the basis for all taxonomical work and affect any discipline of biology that relies on zoological classification. Thus, it is no surprise, according to Michael Ghiselin, that taxonomy is a problematic discipline for the philosophy of science:

> Taxonomy is a highly controversial subject, and the issues are inextricably bound up with philosophical disputes which have endured for centuries. The problems are so important that no biologist can totally avoid facing them. They are so controversial that objectivity in their study is perhaps an unattainable goal. And the issues, both biological and philosophical, are often so recondite that an unequivocal solution seems impossible.[3]

In this work I hope to show that the type of explanations given by Popper and Kuhn cannot adequately account for some of the disputes in taxonomy. The debates are far more metaphysical than either theory can account for, even though the rhetoric involved is similar to many well-explained events in the history of science. To give an adequately descriptive account of these disputes in taxonomy, I will argue that consideration must be given to factors normally not thought of as belonging to scientific discourse, and that the best explanation of these types of disputes, given the nature of the issues, will be in terms of what social and political theorist W.B. Gallie called "essential contestability."

I

One of the most overarching problems in biological taxonomy concerns the classification of birds, mammals, and reptiles. Birds have been traditionally placed in the *class* Aves, the reptiles placed in the *class* Reptilia, and mammals in the *class* Mammalia. These three equally diverse sets account for most of our experience with animal life. Yet according to a different school of taxonomy, since birds and mammals descended from early "reptilian" *classes* (Archosauria and Synapsida, respectively), they are more appropriately placed in lower ranking classificatory groups: *order* Aves and *order* Mammalia. In this chapter, specific cases will be given to illustrate how this disagreement over the proper ranking of birds, reptiles, and mammals is representative of fundamental differences between two schools, namely "Evolutionary" and "Cladistic" taxonomy. These examples will also show how the disputes between these two schools center around the "Darwinian" tradition. In order to ground the issues at hand, some of the history of the methods and principles of taxonomy will be given. If the issues are clear, we should then be able to determine if current theories on science explain why there are differences not only in the classifications themselves, but in the way these schools believe that classifications should be made.

A classic example of disagreement in taxonomy is the case of *Archaeopteryx*, a fossil specimen of the late Jurassic period. *Archaeopteryx* is unique in that it has several reptilian characters: a long jaw with sharp teeth, skull shape and openings typical of dinosaurs of that period, a long tail, and longer hind limbs than forelimbs, all characters which belong to the traditional *class* Reptilia, *sub-class* Archosauria. Upon examining the specimen, 19th-century comparative anatomist Richard Owen observed the imprints of feathers on the fossil

Archaeopteryx. Owen concluded that the feathers must be a key feature of the fossil, and therefore, one of the first birds.[4] According to the traditional "Evolutionary" classification, because of the presence of feathers and regardless of the dominant *sub-class* Archosauria traits, *Archaeopteryx* is an ancestral bird, belonging to the *class* Aves. But, according to the later "Cladistic" school, since *Archaeopteryx* has the above-mentioned characters of the *class* Archosauria, the presence of feathers only serves to place it in the lower ranking taxonomical *order* of Aves. For both schools, *Archaeopteryx* is an ancestral bird in any normal or colloquial sense of the word, but each school ranks it differently.

A much more recent debate in taxonomy is over a much older fossil. *Cotylorhynchus*, a mammal-like reptile of the Permian period, was recently displayed as an "early ancestor" in a mammal exhibit at the American Museum of Natural History.[5] According to Evolutionary taxonomists such as Ernst Mayr, *Cotylorhynchus* is a member of the *class* Reptilia, *sub-class* Synapsida, *order* Pelycosauria, and, therefore, has no business being shown with mammals. Mayr wrote in a letter to *Science* that "*Cotylorhynchus* has always been classified with that primitive group of reptiles, the [ital. sic.] *Pelycosauria*" and further that "even though *Cotylorhynchus* is on the [genealogic] branch that ultimately gives rise to the mammals, it is definitely *not* a mammal."[6]

In a subsequent issue of *Science*, Cladistic taxonomist Kevin Padian did not defend the display of the fossil, rather he challenged the validity of Mayr's objection. Padian replied that "[c]ontrary to what Mayr says, *Cotylorhynchus* is not (and never was) a reptile, but a synapsid [*class* Synapsida]; it does not belong to the 'Pelycosauria,'... [which] unless rigidly circumscribed to *Dimetrodon* [another mammal-like reptile] and a few other forms, is not even a monophyletic taxon [a group consisting of a common ancestor and all its descendants]."[7] As seen with *Archaeopteryx*, the problem is not with the specimen but with *how* the specimen is categorized. Cladistic taxonomy ranks Aves and Mammalia as *orders* instead of *classes*; Reptilia is not a recognized nomenclature. Thus, *Cotylorhynchus* is grouped by Cladists in the *order* Synapsida, distinct from Mammalia.

Both Mayr and Padian ostensibly agree that *Cotylorhynchus* should not be regarded as a mammal. They cannot agree, however, that *Cotylorhynchus* is a reptile, *properly construed*. This disagreement serves as evidence for both Mayr and Padian that the other is grossly confused about the fundamentals of taxonomy. Padian goes on to claim that Mayr is using Linnaean essentialism to group *Cotylorhynchus* and misrepresents the grouping by calling it a "Darwinian" classification, adding that "...Darwin's name should not be invoked to endorse a system that [Darwin] regarded as an abhorrent convention."[8] Mayr responds that "Padian, by implying that the Linnaean and Darwinian systems are the same, ignores the history of taxonomy,"[9] and then, as a polite reminder, proceeds to summarize the history of taxonomy.

One would think that there must be some substantive dispute here to warrant such an exchange. At a fundamental level, there is. Evolutionary taxonomists hold that classifications should be based upon *genealogies* to determine the relation between specimens and to some extent the categorical rank, but *similarities* (or lack of) can override genealogy in determination of rank. Cladistic taxonomists hold that *all* matters of classification, relation and rank, should come from genealogy. In matters of relation between specimens, the two schools often produce identical results. But in matters of categorical rank, as with

Cotylorhynchus and *Archaeopteryx*, there are sharp points of contention. Clearly, the differences in the categorizations reflect differences in the fundamental principles. What is not so clear is the nature of the arguments for each of these sets of principles being the *proper* basis of classification.

Many of the arguments between schools of taxonomy, like the ones above, begin as arguments over specific classifications, which lead to terminological confusion, and then eventually to name-calling.[10] In the *Science* letters, Mayr's initial letter claimed that "[e]veryone knows what the mammalian characters are--hair, warm-bloodedness, nursing the young with milk, a mammalian jaw and mammalian teeth, and many other characteristics by which mammals differ with ancestral amniotes [the group that contains all vertebrates except amphibians], usually classified with the reptiles."[11] Padian complained that most of the characters Mayr listed as belonging to mammals are not available through the fossil record, and are useless, if not misleading, in classifying a fossil specimen. Padian probably overreacted in assuming that Mayr's description constituted the conditions by which mammals should be classified. Mayr, however, failed to realize that Padian was pointing out his descriptions as including "essential" characters of a mammal, as Padian claims Linnaeus would, rather than the "genealogical" characters upon which, Padian claims, Darwin would insist.

It would be an insult to both taxonomists to suggest that they simply misunderstand the others' position. Yet, there must be something at stake other than museum displays. It is worth noting that even within the informal context of a letter to an editor, Padian and Mayr seem to exaggerate each other's discrepancies, especially when attempting to conclude who is the "Darwinian." This is an especially difficult problem in evolutionary biology, as David Hull explains:

> Part of the problem in deciding what actually counts as Darwinian and what as anti- or non-Darwinian evolution is that scientists are engaged in the ongoing process of jockeying for recognition in science. Some scientists exaggerate their differences with the received view to emphasize how original their contributions are, while others exaggerate the similarities between their views and those of contemporary Darwinians in order to throw the mantle of the great Darwin around their own shoulders. Their opponents then attempt to unmask these exaggerations.[12]

While established taxonomists such as Mayr probably do not need to "jockey" for scientific recognition, that he would engage in such behavior says something. It may be that Mayr values the taxonomic tradition he represents, or it may be that he is engaged in a routine process of scientific debate. It may be both, or neither, but it is clear that there is *something* going on here over and above a methodological misunderstanding.

The disputes between the Evolutionary and Cladistic taxonomists are not the only debates where the "mantle of the great Darwin" is contested. Within Cladistic taxonomy there is a sub-school[13] whose members have totally disassociated their methods from evolutionary biology, and proudly claim to be free of the theoretical constraints of evolution, or in other words, they are "non-Darwinian." Another dispute in evolutionary biology is the revitalized argument between Darwin and T.H. Huxley over gradualistic speciation. The theory of "punctuated equilibrium," as formulated by Stephen Gould and Niles Eldredge,[14] spawned a host of arguments over the necessity of gradual speciation as an essential tenet of "Darwinian" evolution. Here some of the disputes concern whether accelerated speciation is "Darwinian," or is an original addition by Gould and Eldredge, or whether Darwin was initially wrong to suggest gradual evolution.

If set to the task, one can find these disputes throughout evolutionary biology. In each case, there is a methodological and/or theoretical debate which, at some point, focuses on the rightful or wrongful inheritance of the Darwinian tradition. If these fights are not merely a matter of misunderstood methods, but are over the authority of the Darwinian tradition, we should attempt to find what makes the mantle of Darwin so valuable. To that effect, Jacques Roger offers this advice:

> Darwinism is much more than a scientific theory. This is perhaps one of the reasons why we still are speaking of Darwinism today. There are no such things as Maxwellism or Einsteinianism. Only historians speak of Copernicanism or Newtonianism. But there is Darwinism, in the same way that there is Freudianism or Marxism. We are therefore obliged to take the historical phenomenon of Darwinism in its entirety, without neglecting either its socio-cultural or its intellectual dimensions.[15]

If it is not clear that Darwinism is more than a scientific theory, one need only step out of science and view it from the standpoint of a non-scientific opponent.

One noteworthy opponent of Darwinism is theist and anti-naturalist Phillip Johnson. Johnson brilliantly captures the sentiment of a century's worth of fear and outrage with Darwin, evolution, and Darwinian science:

> In the academic hierarchy, authority to describe "the way things really are" belongs to natural science, and the history of life belongs to evolutionary biology. This assignment of authority implies that the question of how living organisms came into existence is a matter of specialized knowledge, knowledge that is not available to persons outside the inner circle of science. Ordinary people thus have no alternative but to accept what the experts tell them about such matters, unless they want to be thought ignorant. If the consensus of opinion among evolutionary biologists is that biological evolution produced very complex living organisms by purposeless processes like mutation and selection, then that is an end to the matter. No one has the authority to say otherwise.[16]

Johnson's mission is, of course, to debunk the authority invested in academic science as misplaced and irrational, since it does not allow for the possibility of supernatural influence. Johnson does make a very clear, though sarcastic, point: "ordinary people" place authority in Darwinian biology (or, perhaps, Darwinian biologists place authority on themselves) to explain *the* way life came to be the way it is. The "mantle of Darwin," from the public's perspective, is not based solely, perhaps not even primarily, on content, but the establishment of epistemic authority. However, the public perception of "Darwinian" is not the issue; rather, it is the scientific community's perception of what it means to be a "Darwinian." Instead of taking Johnson's word for it, the question of "Darwinism" might better be explored by following Roger's suggestion to carefully consider the intellectual and social history of the "Darwinian" phenomenon.

Prior to Darwin's *Origin of Species*,[17] biological classifications were mostly ahistorical. The 18th-century botanist Carl Linnaeus[18] developed a classification system in which organisms were placed into groups by virtue of their "essential" characters. Generally considered to be the founder of modern taxonomy, Linnaeus cataloged and classified the flora and fauna known at the time into a "natural system" of categories. His justification of this system was that each species is immutable by virtue of its creation by God, and thus reflects a divine and necessarily unchanging type. This view was based on the Platonic/Aristotelian notion of an "essence." My dog, for instance, has a tail, two eyes, four legs, hair, paws, and makes the sound "ruff-ruff." These are characteristics that all "dogs" have and are "essential" to being a dog. That is, those characteristics are the unchanging attributes of the form, idea, or *eidos* of "dog." The main task of pre-Darwinian taxonomists was to discover the various characters of the forms of species on which classifications could be based.

In the *Origin of Species*, however, Charles Darwin joined others in insisting that the names of organisms are merely proper names, and have no defining properties outside of being labels. Furthermore, Darwin claimed that the "natural system" Linnaeus had been trying to discover was in fact a system based on the "propinquity of descent." In Chapter 13 of the *Origin*, Darwin claimed that in many cases the classifications Linnaeus and others had been making turn out to reveal this principle of descent. It only followed, Darwin thought, that the principles by which we should continue taxonomic work should be based on descent (genealogy):

> All the foregoing rules and aids and difficulties in classification are explained, if I do not greatly deceive myself, on the view that the natural system is founded on descent with modification; that the characters which naturalists consider as showing true affinity between any two or more species, are those which have been inherited from a common parent, and in so far, all true classification is genealogical; *that community of descent is the hidden bond which naturalists have been unconsciously seeking, and not some unknown plan of creation, or the enunciation of general propositions, and mere putting together and separating objects more or less alike.*[19]

Darwin here has made it clear that the natural system of classification is genealogical, and not one in which a plan of creation is revealed, nor one based on general grouping guidelines concerning (physical or functional) resemblance such as the above-described "essential" characters. Darwin's point is that even though Linnaeus and others had been classifying specimens under the pre-supposition of a divine plan containing the form-types of life, the groups under which these specimens are placed will invariably reveal descent. Consider, again, my dog: She has four legs, a tail, hair, and paws. Even these most basic descriptions considerably narrow what type of thing we are talking about, a rodent, bear, feline, or canine. Here Darwin would claim that we have accidentally described a group with common ancestry.

That is, the reason the similarities exist is the common ancestry of rodents, bears, felines, and canines. The more specific our descriptions become, the more recent the common ancestors, and the smaller the group. Thus the "natural system" seems to converge on the principle of descent. But, as he elaborates:

> ... I must explain my meaning more fully. I believe that the *arrangement* of the groups within each class, in due subordination and relation to the other groups, must be strictly genealogical in order to be natural; but that the *amount* of difference in the several branches or groups, though allied in the same degree in blood to their common progenitor, may differ greatly, being due to the different degrees of modification which they have undergone; and this is expressed by the forms being ranked under different genera, families, sections, or orders.[20]

This famous passage reflects a very critical distinction between the *arrangement* of groups and the *amount of difference* as expressed in the *ranking* of groups. The *arrangement* of groups, as Darwin sees it, is based on common descent. That is, the relation between wolves, coyotes, hyenas, and domestic dogs within the *family* Canidae is arranged according to the line of descent of each of these. However, the *ranking* of groups into categories, such as *class*, *family*, and *order*, has to be measured by similarities or resemblance, as they have come about through differing degrees of modification by natural selection. This is basically the claim of Evolutionary taxonomists with respect to specimens like the *Archaeopteryx*, where there seems to exist a feature so divergent as to overrule descent, viz., feathers.

Darwin's views on taxonomy went virtually unnoticed until the mid 20th century. Originated by J. H. Huxley, G. G. Simpson, and Ernst Mayr, the "new systematics"[21] movement in biology sought to embrace Darwin's ideas on taxonomy and make it part of the "modern Darwinian synthesis." Taxonomy up to that point had been more or less a continuation of the principles advanced by Linnaeus and the methods developed by the

19th-century naturalist Georges Cuvier.[22] But the "new systematics" movement "rediscovered" Darwin and began to develop his theory of genealogical, or phylogenetic taxonomy into a method. Holding true to Darwin's views, the "new systematics" school insisted that the arrangement of *taxa* (specimens) needed to be organized according to descent, while the Linnaean ranking continued to be subject to character similarity.

Though the new systematists (proponents of what would later be called Evolutionary taxonomy) often had to defend their view against the attacks of taxonomists holding strictly character-based, or *phenetic* approaches, they were the only significant proponents of *phylogenetic* (genealogical) taxonomy for many years. In 1950 (translated in 1966), the German systematist Willi Hennig[23] published a paper which claimed that a viable taxonomic system could be constructed that considered *only* the arrangement of taxa according to descent. He named this type of taxonomy "phylogenetic systematics" (which would later become *Cladistic* taxonomy). Hennig claimed that categorical organization did not require any special apparatus, but could be determined by genealogical relationships, properly construed.

The Cladistic method suggests that in comparing taxa, we can deduce which two of any three have the more recent common ancestor by finding which two share the greatest number of derived (through descent) characters. A simple case would be to compare my dog, an Iguana, and a Parakeet. The Cladist would point out that the Parakeet and the Iguana have more shared derived characters than either do with my dog. For instance, even though the bird has feathers, both the Iguana and the Parakeet have scaly, taloned feet which the dog does not. Since all three have bones, being a vertebrate is considered to be an *ancestral* characteristic. In fact, any character my dog shares with a lizard, she will also share with the bird. Clearly, the Cladistic method tells us, the Iguana and Parakeet have a more recent common ancestor. We can infer, therefore, that a *speciation event* or *branching* occurred between the ancestors of my dog and the ancestors of the Iguana and Parakeet before there was a branching between the ancestors of the Iguana and the ancestors of the Parakeet.

To perform a strictly descent-based classification, Hennig used these sub-divided branches (or *clades*) as representative of a new categorical ranking. In our example, the ancestor of my dog and the common ancestor of the Iguana and the Parakeet would constitute one categorical ranking. Likewise, the branching between the ancestors of the Iguana and the Parakeet would mark a lower categorical ranking. While fine for large classes, to adequately explain the difference between the multitudes of living things Hennig's system required an extremely high number of categories and a total revision of the existing classifications. Subsequently, Hennig's system was quick to receive criticism for it's seemingly unnecessary complexity and branching-based categorizations. But it was quick to gain a large following due to its simple and consistent method; this following remains large enough to make it the prevailing method for determining taxonomic *arrangements*.

We can now see the beginnings of the contemporary debates. Contemporary Cladists, taking Hennig's position to its conclusion, claim that Evolutionary taxonomists employ "intuition" when concluding that a dissimilarity is significant enough to warrant a change in rank. Cladists claim that the Evolutionary taxonomist intuitively determines that the presence (or absence) of certain characters are more important than other characters, such as feathers on the *Archaeopteryx*. According to the "weighted" characters, a *phenetic* (physical similarity) *ranking* can be given instead of a *phylogenetic* (genealogical) one. It is claimed by Cladists that because these classificatory groups are defined by relationships that can be phenetic *or* phylogenetic, it is difficult to evaluate any evolutionary significance in the groupings. Therefore, in the sense that this taxonomical system can lack evolutionary significance (i.e., ignore descent), it is less *Darwinian* than the Cladistic approach (if *Darwinian* at all).

The main Cladistic criticism of the Evolutionary taxonomical system is that in allowing phenetic data to be a deciding factor in the hierarchical placement of taxa, Evolutionary taxonomists allow for the existence of *paraphyletic* groups. A paraphyletic group is one which contains a common ancestor and most of its descendants, but not all. An example of this would be a family tree that starts with a grandfather and grandmother and contains all their children and their offspring, except for one of the children who is "disowned" because of their being born with six fingers and three eyes. The divergence is so great that the child is no longer considered to be a member of the family. In a zoological example, disowned children born of reptilian parents are the birds. While reptilian in many respects, birds have feathers and thus make for unsightly reptiles. The harshest criticism by the Cladists comes when the birds (or the six-fingered, three-eyed offspring) are elevated to the hierarchical level of the parents (i.e., to that of a *class*). To the Cladist, this seems unnatural, and out of place in a "truly evolutionary" or "Darwinian" system of taxonomy.

The type of group demanded by the Cladists is a *monophyletic* group, which contains the common ancestor and *all* of its descendants. Monophyletic groups, as might be guessed, are a consequence of restricting classifications to phylogenetic inference. If genealogy is the only consideration in establishing the hierarchy of groups within groups, then necessarily all groups will be subordinated by their lineage. This is why the Aves are ranked by Evolutionary taxonomists as a *class*, and by the Cladistic taxonomists as an *order*, subordinated by the *class* Archosauria from which they evolved. This may also be the basis of Padian's complaint that Mayr's grouping does not constitute a "monophyletic taxon." This is a critique that only other Cladists would see as necessarily damaging, since they insist on monophyletic groups, and Evolutionary taxonomists do not.

In their insistence on monophyletic groups and in pointing out what they claim to be a methodological weakness, Cladists directly challenge the principles of taxonomy upheld by Evolutionary taxonomists. While Mayr and other Evolutionary taxonomists can appeal directly to the *Origin* as the source of their truly "Darwinian" principles, Cladists point out that Darwin was mistaken on several points in the *Origin* (for instance, perhaps, gradual evolution) and his views on taxonomy should be included as those that are misguided. The Cladists, in this sense, are appealing to the "intent" or "spirit" of evolutionary theory instead of the "letter" of Darwin's law. Both schools nonetheless insist that they are *the* true "Darwinians."

Some contemporary Cladists are moving in a direction as to make the gap between the "spirit" and "letter" of evolutionary theory even greater. Taxonomists like Kevin de Queiroz and Jacques Gauthier[24] have attacked Linnaean nomenclature as totally incompatible with evolutionary theory. Linnaean categories, they claim, are based on the statement of necessary and sufficient conditions for membership in a particular taxa. This requirement implies that we define what it means to be a member of a particular taxon (true Linnaean essentialism), instead of discovering genealogies and naming what we find in the "natural" phylogenetic system. A consequence of the abandonment of Linnaean nomenclature and categories is that Darwin's (and the Evolutionary taxonomists') *ranking* according to degree of similarity becomes irrelevant and unnecessary. Instead, these "post-Cladists" espouse that nomenclature of taxa can be read directly from their genealogy. To complete the evolutionary taxonomic theory and finish the work begun by Darwin, Mayr, and Hennig, post-Cladists claim that the Linnaean architecture must be abandoned in favor of a truly "Darwinian" system of nomenclature and arrangement, and only then will the "Darwinian" revolution in taxonomy begin.

One problem in determining which classifications and which assignments of "Darwinian" are correct, at least between Mayr and Padian in the *Science* letters, seems to be the mixing of colloquial language with technical taxonomic terminology. Everyday language need not be taxonomically specific, and so in general, probably reflects only the basic linguistic classifications commonly made with respect to life as it is today, rather than life as it has been throughout history. Just as Aves, Mammalia, and Reptilia are vaguely referred to as "birds," "mammals," and "reptiles," and just as *Cotylorhynchus* is vaguely referred to as a "mammal-like reptile," there seems to be no way to talk about taxonomy without including at least *some* colloquial language. The difficulty with this is that colloquial language usually consists of "essential" descriptions rather than "genealogical" descriptions, as Padian suggested of Mayr. For Mayr, however, it is a set of characters that define "mammal." That is, by definition, a mammal is the kind of thing that has hair, is warm-blooded, and nurses its young with milk. Mayr would claim that it is by definition, not essence, that these characters describe a mammal. Padian's inaccurate claim that Mayr is using "Linnaean" methods does, however, *associate* Mayr with an "essentialist" view. For Mayr, this confusion is an unfortunate consequence of language, but for Padian, it is evidence of a "non-Darwinian" view of taxonomy.

Given this linguistic barrier, is it possible that the quibble over *Cotylorhynchus* and the historical debate over *Archaeopteryx* can be resolved? A traditional, Popperian view of science would answer that the debate between Mayr and Padian (and by extension, Cladistic and Evolutionary taxonomy), when stripped of its colloquial language and conceptual misunderstandings, should ultimately be resolvable by a rational argument based on some kind of evidence or tests.

A Kuhnian view of science, on the other hand, would claim that these disagreements reveal significant theoretical differences between the two schools. Proponents of this view would explain that what is thought to be common to the schools, namely terms and observations, do not have common meanings since they are dependent upon different theoretical foundations, and therefore can not serve as a basis for rational argument. Subsequently, the apparent inability of these schools to come to agreement on basic issues is to be expected---the two views of taxonomy are *incommensurable* on these points.

While each of these views can be supported in greater or lesser degrees by historical examples, it is not clear that both are addressing the same kinds of issues. Clearly, neither of these positions seems to adequately account for the rhetorical dispute in the *Science* letters or the conceptual disputes over fundamental principles from which quibbles like the *Science* letters arise. But in having established some of the history and methods of two opposed schools of taxonomy, we have seen by an inspection of the debates surrounding *Cotylorhynchus* and *Archaeopteryx* that there is something disputed in addition to fundamental principles, namely, the status of being "Darwinian." In the next chapter we shall see how neither scientific view introduced works for all scientific disputes, even though each might provide good explanations for many events. Specifically, we shall see that with respect to the problems in taxonomy, neither view is sufficient to demarcate the "true Darwinians" nor is capable of solving the classificatory problems with birds, mammals, and reptiles.

II

If one were to ask a group of scientists which view of science they hold, they might respond by saying something about "the only one." That is, there is a fairly strict and static "standard" view of science which most scientists (and many philosophers of science) share. This "standard" view is not, however, the only way of describing how science works. Some believe that science is quite dynamic, and that the methods of science are often reflections of the people applying them. In this chapter, we shall examine both of these views with the aim of determining if either can account for the disputes encountered in taxonomy.

The *standard-view of science* usually refers to a *method of justification*. Karl Popper is generally held to be the authoritative voice of the standard view in its most sophisticated and pristine form. Popper is most known for his view that scientific theories must not be judged in terms of their *verifiability*, rather they must continually be put to the test, and be judged in terms of their *falsifiability*. That is, any proposition or theory that does not lend itself to a possible test, and thus a possible falsification, cannot be considered scientific. The emphasis of the Popperian or "standard" view is, subsequently, on the testing and testability of scientific claims. But as Popper points out, the simple "test" of an hypothesis is an insufficient basis for science. There are several aspects of "testing" which require consideration:

> We may if we like distinguish four different lines along which the testing of a theory could be carried out. First there is the logical comparison of the conclusions among themselves, by which the internal consistency of the system is tested. Secondly, there is the investigation of the logical form of the theory, with the object of determining whether it has the character of an empirical or scientific theory, or whether it is, for example, tautological. Thirdly, there is the comparison with other theories, chiefly with the aim of determining whether the theory would constitute a scientific advance should

> it survive our various tests. And finally, there is the testing of the theory by way of empirical applications of the conclusions which can be derived from it.[25]

The "Newtonian" theory of mechanics provides a good illustration of the various factors involved in testing. One of the most significant contributions by Newton was his development of a calculus in which we could make predictions given certain physical circumstances. His logico-mathematical system of differential calculus, which is still in use, is internally consistent and provides coherent solutions to "real-world" problems. For example, given the speed and weight of one billiard ball, and given the weight of a second motionless billiard ball in the other's path, we can use Newton's calculus to determine the speed of both balls when the first strikes the second, even though to determine this we may rely on the merely "logical" relation of "force."

Newton's theory seems to meet Popper's criteria on all four points. We can make predictions using Newton's theory and compare the conclusions to establish its internal consistency. Secondly, we can establish Newton's theory as a scientific theory given its dependence on a calculus. To know whether Newtonian mechanics provides a "scientific advance," we need only notice that the trajectory of cannonballs became significantly more precise using the calculus (where it had hitherto been mostly a matter of trial and error). Finally, two-hundred years of direct tests of hypotheses based on Newton's theories have failed to produce any significant falsifying evidence, and thus earned some of Newton's claims the monikers of "laws."

With some disciplines, however, direct tests of some hypotheses are impossible: In the first fossil reconstruction of *Brontosaurus*,[26] O. C. Marsh of the Carnegie Museum in

Pittsburgh had no way to know whether he was reassembling the skeleton correctly. The only bases he had for reconstructing the fossil were his theoretical expectations and understanding of reptilian physiology. No test could determine whether or not he was accurately representing the mammoth dinosaur, since all he had was theory and the headless fossil remains. The problem for Marsh and for contemporary paleontologists is that there is no way to test their theories against fact. At best, a paleontologist can test his or her theories against the closest living facts available. Subsequently, the information paleontologists extract from fossilized bones depends on which living analogies (i.e., the various life-forms with which we have experience) paleontologists draw their theories from.

According to the Popperian view, the *scientific method* allows us to build comprehensive theories from individually tested hypotheses. In order to make sense of a number of disjointed testable statements, we must construct logical or theoretical relations between the testable components of theory using non-testable propositions. Propositions such as these, which are difficult or impossible to test, must rely on related, testable hypotheses as grounds for their acceptance. In the case of *Brontosaurus*, Marsh worked from a comprehensive theoretical model of what *Brontosaurus*-sized dinosaurs *should* look like, and assembled the skeleton accordingly. If a comprehensive theory like the one the Marsh used to reconstruct his fossil has a high degree of confidence through confirmations (non-falsifications) of other constituent hypotheses from physiology, geology, etc., the overall theory is usually considered to be solid, even though some of the hypotheses may be difficult to test.

Some philosophers of science consider this description of science to be over-simplistic on several counts. These philosophers note that if parts of a comprehensive theory are tested and falsified, the propositions are modified according to related, but not necessarily confirmed (testable), hypotheses. Furthermore, the credibility that untestable hypotheses gain in their relation to testable hypotheses is not due to the testability, but to the *theory* that relates the hypotheses; and this relation is not always clearly a "logical" one as in the case of Newton's calculus. To wit, they believe that the modifications scientists make to their falsified theories are bounded by the specific "scientific enterprise" through which they are discovered or derived. That is, there exist boundaries within which a scientist frames acceptable problems and solutions, and it is within these boundaries that a scientist modifies a falsified theory or conditionally accepts an otherwise untenable proposition.

Even the most significant falsified hypotheses are modified within the boundaries of the specific theory around which they are formulated. For example, the failure of the Michelson-Morley experiment[27] marks a major shift in the history of physics, yet this shift was far from quick or decisive. A. A. Michelson and E. W. Morley designed an experiment to illustrate the static Newtonian nature of Earth's orbit, and to indirectly confirm the existence of an ethereal medium through which the Earth and light moved. When the experiment produced a negative result, the Newtonian theory was not abandoned or questioned, but the experiment and the conclusions it should have produced were. Several decades later, even years after Albert Einstein had proposed a relativistic view of the universe (which accounts for the Michelson-Morley results), some physicists still held and argued for a static view of the universe which the Michelson-Morley experiment ostensibly falsified.[28]

An alternative to the Popperian view of science holds that "scientific enterprises," like

Newtonian mechanics, go on within a *conceptual framework*. The guidelines set by this framework determine which problems and solutions are acceptable, and what terms are acceptable for expressing these problems and solutions.

Thomas Kuhn's version of this philosophy of science holds that scientists of the same *paradigm*, or conceptual framework, operate within a shared scientific world-view. The normal course of science is to refine the problems and solutions presented by this world-view, and to train new scientists in that tradition. There often comes a point, however, where the traditional solutions and language do not or cannot address certain problems found by later generations. When this occurs, Kuhn claims that the later generations begin to rethink their world-view so that they may solve the new set of problems, forming a new paradigm. These old and new paradigms compete for dominance within the scientific community: the new paradigm trying to become the new "standard" world-view, the old paradigm trying to maintain the tradition.

According to Kuhn, the competition between paradigms must take place within one of the traditions: "...there can be no scientifically or empirically neutral system of language or concepts... the proposed construction of alternate tests and theories must proceed from within one or another paradigm tradition."[29] In the Michelson-Morley experiment, for example, the experiment was designed and performed within the Newtonian paradigm, and the experiment's failure in the Newtonian paradigm is generally supposed to support Einstein's view. Even though the experiment is performed and understood under the conceptual language of Newton, where mass, space, and time are static, it can only be said to support Einstein when understood in the language of Einstein, where mass, space, and time are dynamic. The experiment cannot be commensurated between both paradigms, since each view is theoretically opposed to the other on the very points of contention. Nor could it be understood "objectively," i.e., between neither paradigm, since a third view would require yet another competitive understanding of mass, space, and time. Because of these differences in world-views, it is claimed, the competing scientists will not be able to formulate their arguments in a language the opposing paradigm will understand and/or accept. The paradigms, and the arguments coming from them, are *incommensurable*.

But what of the *Brontosaurus*? Kuhn's view of science gives us the explanatory devices to describe what goes on in science when paleontologists argue over the bird-like or reptile-like nature of this or that dinosaur: they are working from different paradigms. Yet Kuhn would have a difficult time explaining the rest of the *Brontosaurus* story.

Even though the 1870's discovery of *Brontosaurus* was missing a cranium, the museum reconstruction was complete, head and all. The problem with the museum head was that it wasn't a *Brontosaurus* head. On failing to find a head in the original matrix, Marsh's team looked around the site to see if it was nearby. Several miles later they found one, with the bones of a *Camarasaurus*. Thus the *Brontosaurus* displayed at the Carnegie Museum had a *Camarasaurus* head.

Marsh's theories told him that the *Camarasaurus* head found miles away must be, or would at least work for, the roughly similar *Brontosaurus*. And it did, for nearly one hundred years. Some keen investigative work by John McIntosh and David Berman[30] found that Marsh's *Brontosaurus* not only had the wrong head, but that a few years later another *Brontosaurus* specimen had been unearthed, with head, and suppressed since 1915 in favor of Marsh's "guess."

The correction made to *Brontosaurus* did not significantly alter anyone's views on the creature, or views on dinosaurs in general. Some classificatory revisions were made, and a few changes in the arrangement of the *Infraorder* Sauropoda. While the revisions do not bring any great scientific advance, they do raise some serious questions about our view of science. Paleontological reconstruction relies on theory derived from physiology, geology, living analogies, and many other sources, yet even with those things considered, all we really have are the fossils. We have no clear way of confirming or denying most of our hypotheses about what the bones represent. Kuhn's view tells us that our view of the bones is inextricably linked to the way we view the rest of the natural world. The "bird" people and the "reptile" people lack certain common elements to their views of the natural and historical world, and will find it difficult, if not impossible, to communicate on select issues. But the case of the *Brontosaurus* does not fall under this or Popper's views. Here we have a case where a dubious assumption is made and held in the face of contradictory evidence that opposing parties clearly understand. What does this tell us about this type of "science"? The subject of the "head" dispute is scientific, the people presenting the arguments are scientists, so what do we call the justification for the suppression of contradictory evidence?

The blame for the "suppression of evidence" belongs not to Marsh, but more likely to Henry Osborn. Osborn, like Marsh, was a very influential paleontologist during the early 20th century, and had a habit of throwing his influential weight around. Osborn had been studying Marsh's *Brontosaurus* and developing his own theories about its physiology and lifestyle. After the discovery of the complete skeleton, Osborn "dared" the Carnegie Museum director to put up the new, headed version of the *Brontosaurus*.[31] Needless to say, the specimen remained headless until the next museum director refitted the second *Brontosaurus* with a *Camarasaurus* skull, just like Marsh's original.

While Popper seems to be on track about the nature of scientific evidence, and its falsifiability, and Kuhn seems to have a point in saying that members of different paradigms might view the same evidence differently, both assume that scientific evidence will invariably make it to the table so that it can be falsified or viewed differently in the first place. The case concerning the proper head of the *Brontosaurus* illustrates that *scientific authority* has something to do with the admission or suppression of evidence. In so much as this is true, *authority* over science and scientists can overrule any evidence from any paradigm simply by not letting potential evidence become recognized evidence.

As both Popper and Kuhn point out, however, science is not merely a process of collecting artifacts or data, hypothesizing, and testing. Science is also a process of theorizing relations between testable claims. Taxonomy is in fact the science of determining the relations between the natural artifacts of life on Earth. There is no evidence *per se* to be suppressed by authority that could affect *how* one determines the relation between specimens, but one could certainly *manipulate* the methods and principles by which the relations are made. With physical evidence, one must hide or destroy the would-be evidence, disallowing the possibility of witnesses or someone finding the hidden truth (bringing a new meaning to "skeleton in the closet"). Methods and principles are not something that can be suppressed easily, as it is difficult to hide something that is mostly conceptual. The *manipulation* of methods and principles, however, is not at all difficult to envision.

In the preceding chapter, a distinction was made between Evolutionary and Cladistic taxonomy by pointing out that the former allowed *paraphyletic* groups (a group that contains an ancestor and most if not all of its descendants) and the latter allowed only *monophyletic* groups (a group that contains an ancestor and all of its descendants). This is an accurate description, as long as you are a Cladist. For the Evolutionary taxonomist, monophyletic has always meant what Cladists mean when they say paraphyletic. When Willi Hennig published the theory that would become Cladistic taxonomy, he appropriated the term monophyletic from the not-quite-yet unified school of Evolutionary taxonomy. Since then, Evolutionary taxonomists have returned the favor by renaming what Cladists call a monophyletic group a *holophyletic* group. The result from this exchange of terminology is usually chaos. All taxonomists know what is meant when "paraphyletic" or "holophyletic" is used, but the meaning of "monophyletic" can only be known when the beliefs of the speaker or writer are known (i.e., whether he or she subscribes to the views of Cladistic or Evolutionary taxonomy). Given this added dimension to the taxonomy debates, the words of David Hull start to ring (again):

> ...Falsifiability does matter in science but not the falsifiability of disembodied propositions. What really counts is the falsifiability of scientists. To be successful, a scientist must be able to recognize clear threats to his or her position and respond appropriately. But the proper response to imminent refutation is not admitting defeat; it is changing one's position while retaining one's original terminology. Successful scientists are those who master the art of judicious finagling.[32]

No Evolutionary taxonomist believes Willi Hennig to be the anti-Christ of taxonomy. Hennig is regarded by all as one of the important contributors to taxonomy. Hennig's views were not realized only by some extreme Cladistic "movement," Evolutionary taxonomists use Cladistic methods as well, but not *just* Cladistic methods. Mayr and others retained their terminology and altered their positions to take advantage of the new Cladistic methods, but maintained the original principles they share with Darwin, viz., that excessive divergence in descent had to be accounted for in the taxonomical scheme.

While some "judicious finagling" is evident in the recent history of taxonomy, that there is a battle over methods and principles based on authority is not quite clear. As we have seen, taxonomists are dealing with issues of categorization. "Testing" to see whether *Cotylorhynchus* is a "reptile" may work, provided a testable criterion of "reptile" exists. But "testing" to see if the categorical definition of "reptile" is correct does not seem plausible. That is, the "correctness" of a category does not seem to be the kind of thing that can be resolved by tests. We have also seen that the different groupings are a result of the different fundamental principles on which each school bases its classifications, concepts such as *paraphyletic* and *monophyletic*. But it is clear that the two sides can communicate, understand the different positions, and have formed arguments (terminological confusion notwithstanding). It cannot be entirely correct to say that the two schools are incommensurable.

One of the keys to these debates is the metaphysical nature of classification, namely, the classification of evolving things. Colloquial language promotes the use of essential descriptions of objects. Something is a chair if it has legs, a seat, can be sat upon, etc. These types of descriptions become problematic, however, if the subject tends to change over time. For instance, at what point in the last two-hundred million years was it that species developed enough hair, or sufficiently complex mammary glands, or a specific type of teeth (whatever the characters may be) to qualify as mammals? While criteria can be set, and specimens may be "tested" against this criteria, the *selection* of the criteria is purely arbitrary. As Darwin argued, and as far as all the involved taxonomists are concerned, there exists no "form" by which we can correctly or incorrectly establish criteria for membership in any zoological class. The categorization of living things is an arbitrary construction and has no *necessary* connection to the living things themselves, other than the accidental revelation of descent.

If the criteria for categorical membership is in fact arbitrary, then we might ask *whose* charge it is to establish such criteria. As we have seen from the *Science* letters, this is precisely what is in dispute. The question of *Cotylorhynchus* is not a question of whether it is a "reptile" or not, but whether the Cladistic description of *Cotylorhynchus* as a member of the *class* Synapsida or the Evolutionary taxonomist's description of it as a member of the *class* Reptilia is the better, more natural, or, simply put, more "Darwinian" description.

If it is those who wear the "mantle of Darwin" that have the *authority* to establish the basis for classifications, then, again, we might ask who are in fact the "Darwinians" in taxonomy? Is it the Evolutionary taxonomists who hold a position very similar to Darwin's? Or is it the Cladists who are, perhaps, more in line with contemporary ("Darwinian") evolutionary theory in their approach?

The identity of "the Darwinians" is a perplexing issue which has received a fair amount of attention on its own.[33] The problem with determining who is "Darwinian" is the same type of problem encountered with mammals. How does one form the criteria for membership in something that is constantly changing? If one bases the criteria on essential theoretical tenets or principles, then the number of "Darwinians" is likely to be quite low (if any more than one).

Furthermore, whatever tenets that may have counted as essential to "Darwinism" in Darwin's day most likely will not be considered essential to contemporary "Darwinism." If social groups are used to demarcate "Darwinians," then, again, many otherwise full-fledged supporters will not meet the criteria by virtue of their geographic separation.

It seems as though we are left with the same type of problem encountered with establishing the criteria of zoological categories. As with classification, there seems to be a core concept around which a *notion* of "Darwinian" exists. With classification, the principle of descent provided this notion. It may be the case for "Darwinian" as well.[34] But there are significant differences in the two concepts which makes "Darwinian" not nearly as arbitrary as the criteria for "Reptilia." As seen with the *Science* letters, it seems to matter a great deal who turns out to be the "Darwinian." Those who are "Darwinian," as suggested above, have the ostensible *authority* to establish the foundations of their discipline. Thus in the assignment of "Darwinian," there seems to be an appraisive component. In other words, it means something good (whether for one's posterity or for one's career) to be called a "Darwinian."

What is needed is a view of science which will allow us to deal with these "scientific" disputes. We have looked at two of the dominant views of science in Popper and Kuhn, and found that both views completely overlook scenarios where the appeal to scientific *authority* is used. Furthermore, the debates in taxonomy seem to hinge, at least in part, on the authority of the Darwinian tradition. Clearly, we have left the realm of falsifiability and deductive methods. There is no reason to think, however, that debates over the inheritance of the Darwinian tradition are not, at the very least, pre-scientific in nature. After all, those who end up being "Darwinians" will be the ones writing the textbooks and refereeing the important journals--essentially controlling what is and is not acceptable science, and setting the foundations for what Kuhn calls "normal science."

If we try to explain this using current views of science, we will find that the problems with the classification of birds, mammals, and reptiles can be explained by virtue of the fundamental principles on which they are based. However, these fundamental principles only become fundamental through their acceptance by the "Darwinians." The fundamental principles by which the ownership of the Darwinian tradition is determined are not so easily traced. These determinations are moved by rhetoric and social and political impulses within the scientific community, and should be treated as such.

III

If a scientific dispute boils down to the assignment of a vague, appraisive concept such as "Darwinian," then as far as most philosophers of science are concerned, all bets are off. For the Popperians, we have gone beyond the domain of scientific inquiry, yet for Kuhnians, this is still a crucial and problematic debate. In either case, no scientific explanatory framework exists that could give an account of this activity. There is a possible explanation, but it requires that we resign the objective nature of science in favor of the subjective world of social and political philosophy. In this final chapter, I will present the theory of "essential contestability" in hopes of explaining the problems associated with the identification of "Darwinians." If the problem is sufficiently identified, then perhaps some light will be shed on the actual taxonomic cases such as *Cotylorhynchus* and *Archaeopteryx*.

The most significant evidence for the importance of who or what is "Darwinian" is the actions of the ostensible "Darwinians." The letters in *Science* show how this seemingly scientifically unimportant label can be a large point of contention. Several of the most influential authors in evolutionary biology, including Gould, Ruse, Dawkins, and Lewontin, have tried to determine the precise nature of "Darwinism," but have agreed on very little.[35] Most agree that there is some set of criteria that "Darwinians" meet and "non-Darwinians" don't, but there is no consensus on what these criteria should be. In practice it seems that to say someone or some work is "Darwinian" is to say that this person or work is within the parameters of the accepted practice of biological science. Likewise, to claim that someone or some work is *not* "Darwinian," seems to say that this person or work is not within the bounds of acceptable biological scientific practice and should, therefore, be regarded lightly.

It is clear why this could be an important point for taxonomists, but it is not so clear why the application is problematic. David Hull offers this diagnosis:

> As I see it, the problem these evolutionary biologists are having with "Darwinism" is that they have failed to extend to conceptual systems the same sort of perspective they apply to species. They do not expect species to have an essence--a set of traits that all and only members of a particular species have throughout all time; *but they do expect conceptual systems to have an essence*--a set of tenets that all and only instances of a particular conceptual system have throughout all time.[36]

Hull suggests a way of understanding conceptual systems which requires that we see them as *historical entities* that evolve over time, without essences, *per se*. However, we are not necessarily interested in understanding conceptual systems for what they are. Rather our focus is on giving an explanation of the actions taken by "Darwinians" on the basis of their *own* understanding of their conceptual system, be it confused or not. Furthermore, we wish to explain how "Darwinians" can think of themselves or others as correct or misguided in the application of that name. As Hull points out, this is usually in terms of an essentialist description of "Darwinian" as a conceptual system.

While Hull's advice that we view conceptual systems as historical entities may give a better explanation of who are the "Darwinians," it seems clear that this view is not the one generally used by the debate participants. Can taxonomists such as Mayr and Padian reach an agreement as to the proper application of "Darwinian"? Can there be agreement as to the "essential" characteristics of "Darwinian" taxonomy? Or is this dispute one that is rationally irresolvable?

In academic debate, there has long been the belief that rational arguments about the nature of concepts like "justice," "liberty," and "freedom" can not only persuade opponents, but settle these issues once and for all. But, since 1956, when W.B. Gallie[37] first presented his paper "Essentially Contested Concepts," there has been a growing number of political and social theorists who claim this belief is unwarranted. Debates over concepts like "justice," "liberty," and "freedom," they noticed, do not get settled, and rarely is anyone convinced even by the strongest of arguments. These theorists began to follow Gallie in suggesting that the reason why there never seems to be an end to these disputes is found not in the nature of the arguments, but in the nature of the disputed concepts.

Gallie suggests that certain concepts are "essentially contested." His claim, in effect, is that disagreements over the true meaning of a certain type of concept are irresolvable by rational argument. These concepts are essentially contestable because they have (a) a normative impact (they are appraisive), (b) several factors which constitute the concept (internal complexity), (c) several possible definitions in which these factors are selected and ordered (initially variously describable), (d) the possibility of modification without changing the goal of resolution (they are open), and (e) proponents who realize that others prefer alternative definitions, but still defend their own as the only correct interpretation (used aggressively and defensively). Because of these attributes, the possibility of a single definition satisfying the demands of all applicable rational arguments is precluded.

"Justice," for example, has been an essentially contested concept throughout the history of philosophy.[38] In Plato's *Republic*, we find Socrates soliciting three definitions of justice: "Telling the truth and returning what you receive," "giving each their due," and "the advantage of the stronger." Socrates argues that none of these views are sufficient in their own right, but when placed in the context of a republic, in terms of obedience, honor, and property, they become jointly sufficient. It would seem, however, that Socrates was not entirely convincing. We can still find these individual views of justice in modern ethical theories: In the duty-based ethics of Kant (telling the truth and returning what you receive), in the libertarian-based ethics of Locke (give each their due), and in the ethical egoism of Hobbes (the advantage of the stronger).

According to Gallie's theory, none of these views of justice will be deemed universally true by virtue of rational argument. While we can agree that certain acts are just and others unjust, it is unlikely that we will be able to produce a universal definition. This is so, Gallie claims, because (a) we think of justice as "good" and injustice as "bad" (appraisive), (b) instantiations of justice depend upon factors like it being "blind," and "swift" (internally complex), (c) one formulation might sacrifice the "blindness" of justice for the sake of it being "swift," where another formulation might rank "blindness" the most important with little or no consideration for it being "swift" (initially variously describable), (d) in a society where the application of justice is made to take place at an uniform rate, the factor of "swiftness" is no longer considered, but the contest is not affected; likewise the contest may expand to include arguments over the inclusion and importance of the "equal distribution" of justice (open), and (e) the proponents of the different views of justice understand their opponents' arguments, yet still maintain that theirs in the only true or defensible understanding of justice. That is, one view might propose that 'while swiftness is important, blindness is the most important consideration, and without that priority, it is not really justice at all' (used aggressively and defensively).

In addition to the five conditions for essential contestability, Gallie also recommended two clauses to make sure that the contest is an essential contest and not just a dispute over a "radically confused" concept. Both of the conditions concern the notion of an exemplar, or paradigmatic "type-specimen." Gallie states that the concept in question must be a derivation from the work of an exemplar, or conceptual figure-head, like a Darwin or Newton, and that all participants acknowledge the exemplar as being authoritative. Given the authority of the exemplar, the contesting schools must be involved in advancing the ideals of the exemplar.

If there was an essential contest in taxonomy, there is certainly evidence that the exemplar exists and is highly regarded. The issue at hand, however, is not who is advancing the ideals of the exemplar, but what those ideals actually are. If the contest over "Darwinian" is an essential contest, it must necessarily be prior to any other essential contests in taxonomy (if there be any), since the authority of the exemplar is otherwise undecided.[39]

We might now ask whether the notion of "Darwinian" is in fact an essentially contested concept. According to Gallie's conditions, the concept must be (a) appraisive. In the case of "Darwinian" this is certainly the case. The authority to establish the foundations of taxonomy ostensibly belongs to whomever has this title. Whether in terms of posterity, vanity, career achievement, or self-fulfillment, the responsibility and honor associated with this label is necessarily "a good thing." The concept must also be (b) internally complex. As we have seen, the Cladists appeal more to the "intent" or "spirit" of Darwinism, where the Evolutionary taxonomists support the literal theory as the primary element. Neither the Cladists nor the Evolutionary taxonomists hold that intent and literal interpretation are unimportant, they are just not *equally* important. Lesser factors, such as social groups, almost certainly have a role which can be variously assessed, but the basic differences in fundamental principles outlined in the previous chapters show that there are multiple components involved in the concept of "Darwinian." Furthermore, since each school places different importance on different components, the concept can be said to be (c) initially variously describable.

To show that "Darwinian" is an (d) open concept, we need only reflect on what Hull said of the struggle for scientific success: "...the proper response to imminent refutation is not admitting defeat; it is changing one's position while retaining one's original terminology." Scientific dominance is often a matter of conceptual modification without a change in resolution. With the concept of "Darwinian," this is probably the rule rather than the exception. If the bestowal of the title is based on the perceptions of the immediate scientific community, then political tactics, such as manipulating one's position to sound the most "Darwinian" to a particular audience, would be commonplace. If one also manipulates the notion of "Darwinian" to match one's taxonomic position, as perhaps Mayr and the Evolutionary taxonomists do, then it seems clear that there are no necessary limitations on how the concept may be applied.

Finally, Gallie insists that an essentially contested concept is (e) used aggressively and defensively. This condition requires that contestants must be able to understand and appreciate their opponents' positions, perhaps even share parts of them, while simultaneously claiming that their opponents have it all wrong. What makes this condition so interesting is that in claiming that the other contestants have it wrong, there is no contradiction, only a preference. In the *Science* letters, Padian clearly displays this preference. In asserting that

Mayr is a "Linnaean essentialist," Padian is expressing his preference for "Darwinian" classification over "Linnaean," while also implying that "Linnaean essentialism" is wrong because it is not "Darwinian." Mayr, on the other hand, refutes Padian's claim by suggesting that it is inconsistent with the history of taxonomy, asserting that Padian has it wrong. Even without the rhetoric of the *Science* letters, it can be seen how the Cladistic and Evolutionary schools share many fundamental principles, yet on a few key points, they are aggressively (and defensively) divided.

If the conditions of essential contestability are sufficiently met, then Gallie's theory states that *no rational argument can determine which definition, or which application of "Darwinian" is correct.* That is, if any one is determined to be a consensus "Darwinian," it will not be by virtue of a rational argument; some other means of persuasion must have been employed. If "Darwinian" *is* an essentially contested concept, then what does this tell us about our views of science and about similar types of problems in taxonomy, such as with *Cotylorhynchus*?

One of the common assumptions made about essentially contested concepts is that the use of such concepts somehow shrouds or denies access to the true meaning. Some defenders of the essential contestability thesis have suggested otherwise. In the case of political concepts, they claim, it is not our access to truth that is blocked, but our access to "political reality." As Richard Grafstein summarizes,

> [p]olitical reality, according to this interpretation, cannot be distorted by the way we conceptualize it since it is first constituted through conceptualization; through, that is, the normatively based conceptual and linguistic organization of experience. ...[A] political concept does not have a separable fund of factual content, an objective link to an independent world. There is no such independent political world on which to report.[40]

We might conclude from this that essentially contested concepts like "Darwinian" are necessarily distinct from truly scientific concepts, which rely heavily on an "objective" link to an "independent world." But as Kuhn points out, scientific conceptual systems do not always represent the same independent world. It is sometimes the case that different explanations of what is presumed to be an objective and independent world are populated with incompatible conceptualizations. In Kuhn's system, two competing conceptual systems are *incommensurable* if either contains concepts referring to things in the independent world to which the other conceptual system does not.

While Kuhn's theory is similar to essential contestability, the claim of incommensurability is far more pervasive. Incommensurable conceptual systems may have homophonic concepts without a common referent, and the failure of the two conceptual systems to corroborate on one conceptual point can destroy the theoretical ties the two systems share, making accurate communication (including arguments) impossible. To some extent, an out-of-context use of *monophyletic* is a case of incommensurability. Without the context of the Cladistic or Evolutionary taxonomy, monophyletic refers to both notions. Without the provided context, a taxonomist reading this work would not be sure whether Padian's claim that Mayr's grouping did not constitute a monophyletic taxon is pejorative or just a statement of his [Padian's] own position.

While some disputes in science may be the result of incommensurable conceptual systems, it would be an entirely different matter for concepts in scientific discourse to become essentially contested. The Newtonian and Einsteinian conceptions of "time," as Kuhn suggests, are incommensurable. The two schools have different concepts expressed by the same name. If this were a case of essential contestability, the proponents would have the same core concept, but disagree as to what constitutes an *instantiation* of this concept. In the case of "time," scientists may argue for the rightful ownership of the concept name (motivated by the pending success of their respective paradigms), but essential contests are over the ownership of the truth of an interpretation of a concept.

The important distinction to make between incommensurable and essentially contested concepts is that incommensurable concepts are brought about by a theoretical overdetermination of meaning, and an essentially contested concept is brought about by theoretical underdetermination[41] of meaning. That is, the meanings of incommensurable concepts have been made so theory-specific that inter-theoretical use is impossible. The meaning of an essentially contested concept lacks theoretical resolution, i.e., it is persistently vague. Yet, essential contestability goes beyond "ordinary" underdetermination; while underdetermination can cause serious theoretical difficulties,[42] there is the possibility of resolution by scientific means. That is, empirical tests or arguments may be used to make the concept more clear and precise. In the case of essential contestability, however, there is no scientific recourse available: vagueness is inherent in the concept, not in the instantiations of the concept in the "objective and independent world."

As with "Darwinian," the problems with *Cotylorhynchus* do not reside in the "objective and independent world." The difficulty with *Cotylorhynchus* is its *proper* categorization. Questions of *proper* categorization, as suggested above, are not resolvable by arguments based on tests. There is no test to decide which categorization is the *best* categorization. We might well conclude that arguments over the best categorization of

Cotylorhynchus are not simply cases of "ordinary" underdetermination. It is not at all clear, however, that they are essential contests. That is, it is not clear that saying *Cotylorhynchus* is in the *class* Synapsid rather than the *class* Reptilia is saying something *good* about it.

If the proper classification for *Cotylorhynchus* is established by those shrouded with the "mantle of Darwin," and the title of "Darwinian" is in fact essentially contested, then it stands to reason that arguments over *Cotylorhynchus* are related in some way to the essential contest. While there is no appraisive quality that has any obvious ties to the specimen's categorical status, a resolution of this debate will require a prior resolution of the essential contest over "Darwinian." That is, before a debate over a classification can be resolved, *someone* must be in a position to resolve it by virtue of their "Darwinian" authority. Thus, the essential contest over "Darwinian" can be considered an efficacious component to categorical disputes over *Cotylorhynchus.*

With respect to the *Archaeopteryx*, resolution of the specimen's place in the history of life is dependent upon an authoritative position on the status of birds and bird-like reptiles in the history of life. In order for these subjects to become non-issues, the essential contest over "Darwinian" with respect to these issues must be ended. Unlike other events in science, the resolution of an essential contest over "Darwinian" will not be considered a breakthrough, or a significant advance; rather, those who inherit the mantle of Darwin will do it quietly, most likely by attrition or suppression. Recall that Gallie's guarantee doesn't state that essential contests will never be resolved, only that they will not be resolved by *rational argument*.

Thus we are left with a (scientifically) precarious explanation of the behavior of taxonomists. If the arguments over "Darwinian" can be considered a case of Gallie's "essential contestability," we have far more of an explanation than either Popper or Kuhn can provide. Since their philosophies of science can not account for the uses of scientific authority, an explanation that focuses on the rhetorical nature of the concept is our only recourse, especially in light of the driving forces behind the applications of scientific authority. In illustrating that the assignment of "Darwinian" has all of the characteristics of an essential contest and relating these characteristics to the taxonomy that makes the applications of "Darwinian" noticeable, we can see that the essential contest is best described as an efficacious component of the surface arguments over *Cotylorhynchus* and similar taxonomical problems.

SUMMARY

In this work, I have tried to examine a hotly contested field of science which seems to display a dependence on paradigm authority. The dispute over the categorical status of *Cotylorhynchus* is a minor quibble in comparison to some of the debates in taxonomy. But in establishing the cases of *Cotylorhynchus* and *Archaeopteryx*, as well as the history behind Cladistic and Evolutionary taxonomy, we have seen that there is something disputed in addition to theoretical and methodological principles, namely, the status of being "Darwinian." The fundamental principles at the center of the disputes between the two schools seem to coincide with a battle for the authority of the "Darwinian" tradition.

The two main views of science available can give good explanations of many of the important events in the history of science. Yet neither view seems to be able to account for instances when "scientific authority" is the basis for making important judgements on methods, evidence, or principles. The notion of "authority" over science and scientists becomes very important if the resolution of "real" scientific issues (under Popperian standards) depends on the decisions made by those with the authority, who are in our case the "Darwinians." Yet these judgements seem to be moved mostly by rhetoric and social and political impulses within the scientific community, not by tests, rational arguments, or evidence.

Given that social and political impulses are a driving force in at least some of the history of science, I suggest that Gallie's theory of essentially contested concepts be applied to the case of the "Darwinians." Gallie's thesis states that certain types of appraisive and complex concepts are "essentially contested," and that no rational argument should be expected to resolve issues that directly involve these concepts. The concept of "Darwinian," as it has been expressed, seems to meet Gallie's criteria. According to Gallie's theory, "Darwinian" is an essentially contested concept. While the direct influence of the essential contestability of "Darwinian" on the arguments between Cladistic and Evolutionary taxonomy may not be obvious, the connection is fairly clear when considering the assignment of categorical status to specimens like *Cotylorhynchus* and *Archaeopteryx*. The classification systems in dispute are largely metaphysical in nature, thus the disputes are not easily resolved by "standard" scientific methods. Thus, we are led to wonder how these disputes *could* be resolved.

It is my claim that the essentially contested nature of "Darwinian," that is, the struggle for the authority of the "Darwinian" tradition, is one of the major, if not dominant factors in these disputes. In other words, there is no way to arrive at a "scientific" resolution of arguments like those over *Cotylorhynchus*. They are essentially arguments over who has the right to wear the mantle of Darwin. Seeing that these arguments are mostly rhetorical gives a better understanding of how they can often become bitter, personal, and the source of great rivalries. Furthermore, by adding the explanatory framework of essential contestability to our existing views on science, we are able to explore other problematic and normative issues in science with a level of objectivity consistent with the Popperian and Kuhnian systems, and we can do so without having to make evaluative judgements about science in our explanations.

ENDNOTES

[1] Hull (1978), p. 138.

[2] Kuhn (1970).

[3] Ghiselin (1969), p. 79.

[4] Owen (1863), p. 46.

[5] As reported in *Science*. Holden (1994), p. 1688.

[6] Mayr (1994a), p. 1519.

[7] Padian (1994), p. 1017.

[8] Padian (1994), p. 1017.

[9] Mayr (1994b), p. 715.

[10] See Hull (1988) for an extensive analysis of the scientific and social events between the phylogenetic and phenetic schools of taxonomy.

[11] Mayr (1994a), p. 1519.

[12] Hull (1988), p. 202.

[13] "Pattern Cladistics" or "Transformed Cladistics." See Ridley (1986) and Charig (1982) for an overview, comparison, and critique of the varieties of Cladism.

[14] Eldredge and Gould (1972).

[15] Roger (1985), p. 814.

[16] Johnson (1995), pp. 10-11.

[17] Darwin (1859).

[18] Linnaeus (1735).

[19] Darwin (1859), p. 420. Emphasis added.

[20] Darwin (1859), p. 420.

[21] Mayr (1942), Simpson (1944), and Huxley (1942).

[22] Cuvier (1827).

[23] Hennig (1966).

[24] de Queiroz (1988) and de Queiroz & Gauthier (1990).

[25] Popper (1934), p. 100.

[26] Although the legal and correct name for *Brontosaurus* is actually *Apatosaurus*, I use the former because it is far more recognizable, and also in protest of the apathy shown toward one of the greatest dinosaur names ever created: The "thunder lizard."

[27] See Ch. 6 of Feyerabend (1981) for a discussion of the perceived effects of the experiment.

[28] Miller (1922). See also Holton (1975) p. 316-317.

[29] Kuhn (1970), p. 146.

[30] McIntosh & Berman (1975).

[31] Wilford (1987).

[32] Hull (1978), p. 138.

[33] See Hull (1985) and Recker (1990).

[34] See Hull (1985) for a treatment of "Darwinism" as an evolving, historical entity.

[35] As described by Hull (1978).

[36] Hull (1985), p. 777.

[37] Gallie (1956). Also in Chapter 8 of Gallie (1964).

[38] See Chapter 1 of Rhodes (1996) for an agressive treatment of "justice" as an essentially contested concept.

[39] The problem with the exemplar is actually a blessing in disguise. Of all the tenets in Gallie's theory, none has received greater criticism than his treatment of importance and nature of the exemplar.

[40] Grafstein (1987), p. 10.

[41] In this sense, essential contestability can be thought of as a special, normative case of the referent underdetermination described by Philip Kitcher in Kitcher (1978). Here

conceptual meaning as under- or over-determined in theory is understood as dependent on the referent.

[42] See Feyerabend (1975).

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