Introduction

According to a celebrated philosophical tradition that has enjoyed prominence for more than a quarter of a century, so-called "natural-kind terms," like 'oak', 'water', and 'mammal', refer to kinds with theoretically interesting essences. According to the tradition, scientists learn by empirical investigation what those essences are. Scientifically informed conclusions about kinds' essences are discoveries, not stipulations. Scientists do not change the meaning of a term like 'water' or 'mammal'; they simply discover the essence of what speakers past and present have been calling "water" and "mammal," respectively.

The essence of water, scientists have discovered, is the chemical composition H_2O: Nothing could possibly be water without being H_2O or H_2O without being water, so long as scientists' empirical facts are right. If earlier speakers ever called something "water" that was composed of another chemical, they were wrong. In similar fashion, earlier speakers were wrong to call whales "fish." Scientists have corrected ordinary speakers about this matter. Empirical investigation into essences has led to the discovery that whales are mammals, not fish.

In this book, I examine the familiar tradition described above. The tradition is so well established that it is typically taken for granted in high-profile philosophical discussions from a wide range of diverse philosophical areas. Nevertheless, the tradition is mistaken. After showing that it is mistaken, I examine consequences. Here I provide a chapter-by-chapter overview of the book. Then I specify salient items not on my agenda.

AN OVERVIEW OF THE BOOK

In Chapter 1, I clarify my subject matter. For the most part, I discuss biological taxa. Biological taxa include species, such as the lion (*Panthera leo*), as well as higher taxa, which contain any number of species. Higher taxa that contain the lion species include the felines (a family) and the mammals (a class).[1] I argue that species and other taxa are natural kinds. I also defend the claim that various chemical substances are natural kinds, though my primary interest here, and indeed throughout this book, is with biological kinds. Some biologists and philosophers have argued that, contrary to the view I take, species and other taxa are not kinds at all but rather individuals. Other philosophers have argued that biological taxa and chemical substances are not natural. I argue otherwise.

In Chapter 2, I address essentialism, particularly with respect to biological kinds. Essentialists have not been well informed about biology, and I argue that some commonly held essentialist theses should be abandoned in light of contemporary biology. Nevertheless, I argue that some forms of essentialism about biological taxa are highly plausible in view of contemporary biological systematics and cladism in particular.

In Chapter 3, I address the further claim that biologists' conclusions about the essences of our kinds have been *discovered* to be true. Here I part with the familiar tradition, according to which scientists have not altered the use of key terms in sentences like 'Whales are mammals, not fish' to make such essence-revealing sentences true by stipulation; scientists have just found sentences like this to have been true all along. I argue that scientists' conclusions are not, in general, discovered to be true in this way. They are stipulated to be true.[2] Contrary to the received view, scientists change the meanings of kind terms.

In Chapter 4, I show that the lessons I draw in Chapter 3 about biological kinds apply more broadly. In particular, they apply to the other widely discussed group of natural kinds: chemical kinds. Chemists do not report the discovered essences of our chemical kinds any more than biologists report the discovered essences of our biological kinds.

The view that scientists' conclusions about the nature of our kinds are discoveries seems wrong. After showing this, I inquire into consequences. In Chapter 5, I address consequences for incommensurability. According to incommensurability theorists, rival scientists talk past one another. Words change their meanings as science progresses, so future theorists and past theorists who appear to disagree are really talking about different matters. Some friends of incommensurability go so far as to say that there can be no rational choice between competing theories. This threatens our ordinary, intuitive view of science's progress, according to which scientists advance by discarding older theories and replacing them with improvements.

The causal theory of reference to natural kinds is supposed to *redeem* the natural account of science's progress in the face of worries raised by incommensurability. That it redeems science's progress is supposed to be a major achievement of the causal theory. According to the causal theory, we do not define natural-kind terms by theoretical descriptions, which would be expected to change over time with our theories; otherwise the meanings of our kind terms would change with our theories, as incommensurability theorists say that they do. Rather, we identify samples of a kind, apply a name to the kind they exemplify, and then discover the essence of the kind. Theories can come and go without the reference of our natural-kind terms changing at all, for it is the samples in the world, and not our theory, that determine what our terms refer to. Thus, given the causal theory of reference, worries about incommensurability do not arise because worries about meaning instability do not arise.

Unfortunately, the foregoing account of reference stability is naïve. The meanings of our kind terms do shift, even given the causal theory, as I show in Chapters 3 and 4. Part of the problem may be that the causal theory does not completely discard descriptions, as I explain shortly. But even apart from descriptions and any instability they can cause, there is instability in reference. Often scientists find that it is not clear whether a term refers to this or that kind exemplified in samples: Empirical exploration uncovers many plausible candidates. At that point scientists often refine the use of the term in light of information that could not have been anticipated by the dubber of a term. Scientists change terms' meanings.

Thus, I argue that the causal theory does not rid us of meaning instability or ward off the threat that instability raises for the progress of science. I present a different defense of scientific progress. The main examples of progress upon which I focus are progress through the Darwinian revolution and progress through the rejection of vitalism.

The causal theory of reference cannot rescue us from problems of incommensurability, which threaten scientific progress. Neither can the causal theory rescue us from problems that threaten necessity, as I show in Chapter 6.

The new causal theory *appears* to liberate necessity from problems afflicting analyticity. Many acknowledge that Quine has cast serious doubts on analyticity, and in Quine's day, doubts about analyticity were doubts about necessity, also: Analyticity and necessity were not distinguished. Fortunately, in light of the causal theory of reference, it now appears that doubts about analyticity should not carry over to necessity. The causal theory allows for statements to be necessary yet *not* analytic, or even a priori. Thus, the many proponents of the necessity associated with the causal theory appear free to accept necessity and still concede that Quine is right about analyticity. So, given Quine's influence, it is not surprising that in the wake of the causal theory, there is little sympathy for analyticity: "Quine's rejection of analyticity still prevails," as William Lycan (1994, p. 249) and many others testify. But the causal theory, with its attending empirically discovered necessity, does not finesse objections to analyticity, as it is supposed to do. Chapters 3 and 4 strongly hint as much: My objections to essence discovery in those chapters are similar to Quineans' objections to analyticity. The suggestion is that empirically discovered necessity is subject to the same troubles as analyticity. If those troubles are so destructive that analyticity is completely untenable, then something similar ought to be suspected in the case of a posteriori necessity. And if a posteriori necessity is tenable, then analyticity's prospects brighten. Chapters 3 and 4 suggest this connection between analyticity and a posteriori necessity, but these chapters do not directly address analyticity.

In Chapter 6, I offer direct arguments that if causal theorists like Kripke are right about a posteriori necessity, Quine has to be wrong about analyticity. A commitment to the necessary a posteriori is a commitment to analyticity.

We must choose. We can have either the familiar necessity *or* the familiar animadversions to analyticity. We cannot have both, so one must be abandoned. I argue that it is the Quinean arguments against analyticity that should be abandoned. Chapters 3 and 4 may appear to commit me to choosing otherwise. They may appear to commit me to the familiar animadversions to analyticity. But I argue that they do not: It is not a consequence of my claims in Chapters 3 and 4 that analyticity is untenable.

So in the final two chapters of the book, I examine some consequences, real and apparent, of my attack on the view that scientists report the discovered essences of our natural kinds. There are many other possible consequences of interest.[3] But I leave other areas for other occasions.

Some projects that might be *thought* to be part of my agenda are not part of it. In particular, it might appear that I aim to undermine the causal theory of reference and to replace it with an old-fashioned descriptivist account. There is a well-known problem that threatens the causal theory, and this problem bears some resemblance to considerations that I raise in Chapters 3 and 4. But I do not think that this problem, which is known as the "qua problem," discredits the causal theory. In a few brief paragraphs I will explain why I accept some form of the causal theory, and why I think that the theory survives the qua problem. This provides me with an occasion to introduce and clarify the causal theory, which is discussed throughout most of the book, as well as to forestall a possible misunderstanding about my aim.

NOT IN QUESTION: THE CAUSAL THEORY OF REFERENCE

The causal theory addresses how a word is assigned to its referent by the word's dubber. It also addresses how a word is passed on from the dubber to other speakers: With respect to this latter issue, the causal theory recognizes a chain of communication, but this is of little importance in what follows. The issue of how a word is assigned to its referent is more important. According to the causal theory, a term for an individual is typically coined in the presence of the individual by a formal or informal baptismal ceremony in which the speaker says something like "This person is to be called 'Cicero' " (the speaker points). A term for a natural kind is typically baptized in the presence of *samples* of the kind: "*Tyrannosaurus rex* is the species instantiated by *that* fossilized organism." The causal theory is typically contrasted with an old-fashioned descriptivist account, according to which a kind term refers to something just in case the candidate satisfies descriptions that speakers can list without reference to a sample, such as 'carnivorous', 'reached thirty-nine feet in length and weighed ten tons', 'had two puny forelimbs and two powerful hind limbs', 'had dagger-like teeth', 'lived some seventy million years ago in North America', and so on.

The causal account seems to capture the typical naming practices of biologists. Biologists coin new species terms by providing a *sample*, called a "type specimen." The newly coined term refers to the species instantiated by the type specimen. The type specimen is generally stored in a public location in order to allow biologists to use it for reference. Uniquely identifying descriptions that make no reference to a type specimen are not needed and are often not used. Consider a simple example. Two biologists in the Amazonian rain forest today coin new species terms. Each collects a type specimen, specifies a term, and says, "Let this term name the biological species exemplified in this organism" (the speaker points). Suppose that the two type specimens belong to different biological species: They are not related by evolution, would be incapable of interbreeding, and so on. Then even though the two biologists may have failed to offer uniquely identifying descriptions of their species, the biologists have still coined terms for *two different biological species*. Further investigation could determine this to every biologist's satisfaction.

A uniquely fitting description that makes no reference to the sample is not needed in order to assign a species term to a species. Further, it may not even be available. The type specimen may consist of only a fragment of a skeleton preserved in a fossil, so that the biologist does not have a very clear idea what the species was like. Or the type specimen may be an atypical member of the species (see, e.g., Ereshefsky 2001, p. 261). For these reasons, the type specimen may not provide enough information to allow the species term dubber to give a uniquely identifying description that makes no reference to the specimen.

Old-fashioned descriptivism seems untenable. Paradigmatic samples are used in reference.[4] That is not to say that descriptive information is irrelevant for reference according to the causal theory. Baptisms convey some descriptive information about the referent. When a dubber points and says, "Let this term name the biological species exemplified in this organism," she specifies that the referent is a *species*.

More descriptive information may be offered. A *reference-fixing description* that appeals to a cause may be used to baptize a term and may even be used in place of pointing. For example, one might coin a species term '*S*' by saying "*S* is the species that left footprints in such and such location."[5] Here the description fixes the reference of '*S*', as opposed to serving as a synonym for '*S*'. As it happens, prehistoric artiodactyls may have left footprints in the location, so that the relevant species of artiodactyl is the referent of '*S*'. But it is hardly a necessary truth that this species of artiodactyl, *S*, left prints in that spot. Some predatory species of cat could have settled on the spot, leaving its own prints instead and chasing away all artiodactyls, including specimens of *S*. If 'the species that left footprints in such and such location' were a synonym for '*S*', rather than simply a description to fix the reference, then '*S* is the species that left prints in such and such location' would be a necessarily true statement; instead, it is a contingently true statement.

'Causal theory' should not suggest, then, that no descriptive information is relevant to a term's reference.[6] That descriptive information is relevant for the causal theory helps that theory to avoid various objections, including one that might be mistaken for mine: the objection that the theory suffers from the so-called "qua problem." This problem may be illustrated as follows. Take a term like '*Tyrannosaurus rex*'. Causal theorists would say that the term is "grounded" in a sample *T. rex* specimen that has been fossilized. But if that is so, why does '*T. rex*' apply to *T. rex* instead of to dinosaurs in general or to animals in general or to big things in general? The sample was, after all, a dinosaur and an animal and a big thing as well as a specimen of *T. rex*. The causal theory seems, at least on first blush, to be unable to account for the reference of '*T. rex*' to *T. rex* instead of to these other kinds. It seems to be undermined by a qua problem.

This problem is not fatal. Recall the baptism specified above: "*Tyrannosaurus rex* is the species instantiated by *that* fossilized organism." Given such a baptism, the term '*T. rex*' *has* to refer to *T. rex* instead of to the kinds *dinosaur*, *animal*, or *big thing*, for the simple reason that *T. rex* is the only kind in the group that is a *species*. The dinosaurs as a whole, in all of their diversity, do not constitute a single species, though they constitute a larger taxon (see note 1). Similar words apply to the animals in general. The kind *big thing* is obviously not a species. So if '*T. rex*' was coined as specified above, then it had to be assigned to *T. rex*, as opposed to kinds like these others that also happen to be instantiated by *T. rex* specimens. And '*T. rex*' *was* coined pretty much as specified above. Early in the twentieth century, Henry Fairfield Osborn (1905) published the name '*T. rex*' for the first time, explicitly specifying that it was a name for the *species* exemplified in a certain fossilized specimen.

For this and related reasons, the qua problem is not fatal to the causal theory. That is not to say that the causal theory eliminates indeterminacy. But eliminating indeterminacy is not its job. Its job is to recognize just the indeterminacy that is really in the language, and not more than that. If there is a qua problem, then the causal theory recognizes far too *much* indeterminacy. This would be a problem, but it is not a problem that the theory recognizes any indeterminacy at all. The causal theory does not recognize too much indeterminacy, because some descriptive information can be used in causal baptisms.

Other problems are sometimes thought to afflict the causal theory and seem to admit a similar resolution, but I do not discuss them here. A comprehensive defense of the causal theory is not in my plans. Here I wish simply to affirm that I endorse some version of the causal theory and to distance myself from the foregoing criticism in particular. Although I argue that according to the causal theory or any plausible theory there is often a good deal of interesting indeterminacy in the reference of our terms, this is not a criticism of the causal theory. It is a recognition of indeterminacy in the language. I will not be arguing that causal theorists are committed to recognizing any indeterminacy that is *not* present in the language, as the qua objection does.

Although my aim is not to criticize the broad outlines of the causal theory, I offer substantial refinements. More importantly, I suggest that the causal theory does not alter the philosophical landscape nearly as much as it has been supposed to do. In particular, the theory does not clear up or circumvent famously troubled issues that concern referential instability and that concern necessity because of its connection to analyticity. Despite appearances and despite much testimony to the contrary, these big, fascinating issues about conceptual change are not much affected by the advent of the causal theory.

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More information

1

What Is a Natural Kind, and Do Biological Taxa Qualify?

Before I undertake an examination of natural kinds and reference to them, I will clarify what natural kinds and natural-kind terms are. Philosophers who discuss natural-kind terms seldom offer any analysis of what they are supposed to be. Prominent philosophers tend just to offer examples. Their examples are supposed to help readers grasp intuitively what is intended. The most common examples of natural-kind terms presented in the literature are perhaps terms for biological species and higher taxa: 'Tiger', 'elm', and 'mammal' are all discussed extensively in the literature. Chemical kind terms, such as 'water' and 'jade', are also presented as examples with great frequency.

My primary interest in this book has to do with kind terms from biology. To a lesser extent I also examine the other prominent group of natural-kind terms, those from chemistry, in order to show that many of the central observations that I make about specifically biological kinds are not limited to the realm of biological kinds.

In this chapter, I defend the position that the terms from biology and chemistry that are typically supposed to be natural-kind terms really are natural-kind terms. As I have said, most analytic philosophers accept terms like 'tiger' and 'water' as paradigmatic, as words that are "natural-kind words if any are," as one introductory textbook says of them (Platts 1997, p. 264). Biological species in particular tend to be regarded as paradigmatic natural kinds. "The classification of living things into biological species is one of the paradigm (i.e. indisputable and/or typical) cases of a division into 'natural kinds'," another textbook says (Wolfram 1989, p. 236).

Unfortunately, various obstacles threaten to create problems for textbook examples of natural kinds. Vigorous criticisms have been launched at the dominant perspective from both philosophers and biologists. Some critics charge that many or all of the supposed biological natural kinds are not natural kinds because they are not really *kinds*. Instead, species and perhaps even higher taxa are individuals. Other critics charge that the supposed biological natural kinds and even chemical kinds are not natural kinds because they are not really *natural*. In my view, both criticisms miss the mark. I defend the position that biological taxa, as well as chemical substances, are natural kinds.

I. ARE BIOLOGICAL TAXA KINDS?

The first objection to counting the usual list of natural kinds, or rather the usual list of natural kinds from biology, as a list of genuine natural kinds is that the items on the list are not *kinds*. The objection is that species are individuals rather than kinds (Ghiselin 1974, 1987, 1995, 1997; Hull 1976, 1978, 1980, 1987). Perhaps higher taxa too, are individuals instead of kinds (Ghiselin 1995; see also Ereshefsky 1991 and references therein). Being individuals, species are concrete entities like Mt. Rainier or George W. Bush. They differ markedly from *gold* or *water*, which are kinds and therefore abstract entities.[1]

In the present section (I), I defend the claim that species are kinds. In sections (I.1)–(I.5) I find fault with the usual reasons for saying that species are *not* kinds. In section (I.6) I offer a positive argument for the claim that species *are* kinds and discuss the rival view that they are instead individuals.

I.1. *Evolving Kinds*

The most frequently cited reason for supposing species to be individuals is that they evolve. "If species were not individuals, they could not evolve," Ghiselin (1987, p. 129) insists. Natural kinds, in particular, cannot evolve. Ghiselin sees this as "the most compelling" reason for counting species to be individuals rather than kinds (1981, p. 303). Kinds are deemed incapable of evolving because they are abstract objects, with immutable essences. A natural kind cannot change in any respect. Only particular concrete objects are capable of change. So if species evolve, they must be individuals, not kinds.

This objection is not persuasive. Abstract objects' incapacity for change is certainly a barrier to their evolving, but when we say "Species evolve," we do not mean that any abstract kind evolves; we mean that successive *members* of a kind gradually become different from their ancestors. Similarly, by the claim "One species can evolve into another" what is communicated is not that any abstract object can become a different abstract object but rather that the instances of one species-kind can give rise to instances of another species-kind.

A parallel can be drawn to incontrovertible kinds such as lead and water. Elements like lead were generated from lighter matter. There is nothing paradoxical about this claim. What is communicated by the statement 'Lead has been generated from lighter matter' is not that an abstract kind, lead, has been generated; what is communicated is rather that concrete *members* of the kind came into being by the above generative process.[2] In the same way, when 'Lead can be transmuted into gold' is correctly affirmed, the intent is not that one abstract kind is transmuted into another but rather that lead particles can be transmuted into gold ones. Again, when speakers affirm the statement 'Water can be created with hydrogen and oxygen', what they communicate is not that two abstract kinds can be used to create a third abstract kind. Rather, what they communicate is that instances of two abstract kinds can be used to create instances of a third abstract kind. In none of these affirmations about what are clearly kinds is there any commitment to abstract objects' changing. The same holds for the statement 'Some species of fish evolved into a species of amphibian'. What is communicated by that statement is not that one abstract object changed into another but that instances of the one kind generated, by a process of evolution, instances of another. The argument that species cannot be kinds because they evolve fails for taking idioms too seriously.

I.2. *Historical Kinds*

Another common objection to counting biological taxa as kinds alleges that species and higher taxa are spatio-temporally restricted, whereas kinds are spatio-temporally unrestricted. No matter how similar to our terrestrial horses Alpha Centaurian organisms may be, they are not members of the horse species. Genetic similarity, interfertility, and so on could not establish conspecificity, because the Alpha Centaurians are not historically connected to the terrestrial species. Therefore, the horse species is not a kind. Hull draws a comparison to the undisputed kind gold:

> If all atoms with atomic number 79 ceased to exist, gold would cease to exist, although a slot would remain open in the periodic table. Later when atoms with the appropriate atomic number were generated, they would be atoms of gold regardless of their origins. But in the typical case, to *be* a horse one must be *born* of horse. (Hull 1978, p. 349; see also, e.g., Ghiselin 1981, p. 304)

Hull notes that once a species has disappeared, the impossibility of reappearance is "conceptual" rather than contingent. And this is supposed to reveal that species cannot be kinds, being historically delimited, but rather that they must be individuals.