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FOR PLURALISM AND AGAINST REALISM ABOUT SPECIES*

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I argue for accepting a pluralist approach to species, while rejecting the realism about species espoused by P. Kitcher and a number of other philosophers of biology. I develop an alternative view of species concepts as divisions of organisms into groups for study which are relative to the systematic explanatory interests of biologists at a particular time. I also show how this conception resolves a number of difficult puzzles which plague the application of particular species concepts.

1. Introduction. The argument of this paper proceeds in two steps. The first establishes that we ought to be pluralistic with respect to species concepts: that is, no single ground for drawing species divisions has exclusive authority. I make this case by pointing out independent and legitimate explanatory demands in biology which require distinct conceptions of species and classificatory schemes.

The second step of the argument shows that this pluralism is incompatible with a realist view of species. More specifically, I show that the grounds which should make pluralists of us all prevent us from being realists. The explanatory interests of biologists define the legitimate species concepts at any particular time, and these have changed dramatically over the course of history without corresponding changes in the physical world they classify. Thus, our resources for crafting a pluralism we can live with will not allow us to regard the species it generates as real. I conclude by demonstrating how abandoning a realist view of species renders impotent a set of conceptual difficulties which arise in applying particular species concepts. The dissolution of these puzzles is a final com-

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elling piece of evidence that we would be better off without carrying around this particular bit of realist luggage.

2. A Plea for Pluralism. Let us begin with Philip Kitcher's arguments (1984a,b; 1987) for pluralism. Although his is only one among a variety of pluralist positions (e.g., Ruse 1973, Mishler and Donoghue 1982), Kitcher's arguments are compelling and particularly relevant to this paper.

Kitcher (1984b) diagnoses the troubles of monism. The history of species concepts, he suggests, is a succession of proposals designed to accommodate different kinds of biological inquiry. The defenders each maintain that their species concept serves the needs of *all* biological investigation (see Naef 1919, cited in Hennig [1950] 1966, 32; and Mayr 1987, 165–166); this latter claim is fundamentally misguided.

As an instructive example, Kitcher considers Mayr's Biological Species Concept (BSC), which claims that "[s]pecies are groups of interbreeding natural populations that are reproductively isolated from other such groups" (Mayr 1976, 518). According to Kitcher, this conception of species is ideal for considering some important kinds of biological questions, such as distinguishing groups of sympatric, morphologically similar, but noninterbreeding organisms, but is not at all useful in other biological contexts, for instance, when one is interested in paleontological evidence or asexual populations. These difficulties have led Mayr either to consider asexual organisms to be "aberrant cases" (cited in Kitcher 1984b) or to recognize their importance, but deny that they constitute species (Mayr 1987). Mayr further acknowledges that special taxonomic provisions must be made for the ecologist. Perhaps unintentionally, Mayr showcases the principal failing of monism by identifying legitimate biological interests which are not served by the BSC.

Kitcher views the case of the BSC to be merely symptomatic of the problems of monism as a strategy for classification. The presumption that "there is a fundamental feature of organismic diversity, common to all groups of organisms" (1984b, 318–319) is misguided. The practice of biologists indicates a number of important patterns of biological diversity and a number of independent interesting questions about organisms and their relationships to one another. Kitcher concludes that several species concepts are required.

Specifically, Kitcher divides biological investigation into structural and historical questions. Neither inquiry is prior to nor more fundamental than the other, and each further subdivides into variations appropriate for significantly different biological explanatory demands. For example, structural concepts of species might focus specifically on examining commonalities of genetic or chromosomal structure, or of developmental

programs. Historical concepts employ criteria of reproductive isolation,¹ ecological distinctness or morphological distinctness. This last criterion actually provides a potentially infinite set of morphological species concepts, each relative to some precise degree and respect of morphological similarity. The difficulty is not fatal, however, as precisely the same general criteria of acceptability developed in section 3 for evaluating any proposal for dividing organisms can be applied to specific and local schemes of morphological division. My claims do not depend upon the details of any particular scheme; thus, “morphological species concept” will be a blanket term covering *any* proposal created by dividing organisms with respect to some particular anatomical or physiological feature, and some precise degree of similarity.

Each of these three historical interests is subordinated either to a consideration of the main types of evolutionary discontinuity between species or to a principle of grouping organisms by recency of common ancestor. Kitcher claims that each of the nine species concepts so generated (three structural and six historical) represents a legitimate biological explanatory demand and that each demand *necessitates* the use of its corresponding species concept. To privilege any one of these explanatory demands and force all other investigations to take place on its terms is a mistake. The BSC, for example, will not provide a useful framework for distinguishing relevant similarities between organisms when one is interested in independent, legitimate questions about their morphology, or as we saw earlier, for any interesting questions at all about groups of asexual organisms.

Let us now consider Kitcher’s central claim: that certain explanatory demands are *inextricably bound* to certain species concepts. We might ask whether rejecting this claim will merely pare our conceptual tools down to one (or a few) which does the same work as Kitcher’s nine species concepts. The question is whether Kitcher’s tool box is filled with various wrenches, pliers, and screwdrivers or merely with nine hammers.

Desirable as the simplicity of a single or just a few species concepts would be, it seems unattainable without abandoning many of the powerful explanatory resources of modern biology. This is because when one is interested in some feature exhibited systematically by organisms, it is best to divide up the objects under study with respect to that feature, and a division based upon something else will often hinder the inquiry. We have no reason to suppose that organisms will segregate into exactly the same

¹Mayr’s BSC is, however, nondimensional. Thus, species concepts which employ criteria of reproductive isolation are historical because they are particularly well suited to addressing historical (e.g., evolutionary) *questions*, not because the concepts themselves have some inherently historical dimension.

groups with respect to every biologically interesting feature they possess, and I show that, as a matter of fact, they do not.

Let me begin by acknowledging the dramatic explanatory achievements of the BSC in the investigation of reproductive relations among naturally occurring populations. This approach to species has illuminated entire classes of explanatory enterprises, including (among others) much of population biology, the phenomena of gene flow, speciation events in peripheral isolates, founder effects, and the nature and adaptive value of reproductive isolating mechanisms themselves. A more specific example of the explanatory success of the BSC is the identification of reproductively isolated sibling species of *Anopheles* mosquitos, which was used to successfully account for the distribution of malaria in Europe, now a classic case of evolutionary explanation (see Mayr 1963).

The BSC, with its criterion of reproductive isolation, was a particularly important resource in the *Anopheles* case. The sibling species of mosquitos are similar morphologically and inhabit overlapping ranges, so neither a morphological nor an ecological criterion of species division could have been of much use in attempting to unravel this particular mystery.

But it is equally clear that there are explanatory contexts in which the BSC is highly problematic. As noted above, a criterion of reproductive isolation is of little practical use in reconstructing the phylogeny of species based upon paleontological evidence, and is completely unhelpful in explaining anything about asexual organisms. The latter case is especially problematic, since morphological and ecological features of organisms cannot be taken to be possible *indicators* of reproductive isolation as they might in the case of paleontological evidence. This problem is also more robust than is usually recognized, as asexual reproduction is not limited to prokaryotes, but is the normal form of reproduction in *fungi imperfecti* such as *Penicillium*, in a great many plants such as *Rubus* (blackberries), and in several groups of animals, such as the bdelloid rotifers (Futuyma 1979). The BSC is literally powerless when confronted with such organisms, which are therefore organized for investigation on morphological and ecological grounds.

Hybridization between taxonomic species poses another problem for the BSC. This difficulty is sometimes dismissed or ignored, perhaps because of the rarity of hybridization among mammal species. Hybridization, however, occurs with significant evolutionary consequences in butterflies, in leaf hoppers, in fishes, and in many kinds of birds (lists of natural hybrids are given by Gray 1958 and 1954 for both birds and mammals, respectively, while reviews of natural hybridization include Hubbs 1955, Mayr 1963, Remington 1968, and Moore 1977). In the plant kingdom, however, hybridization achieves full generality: naturally-occurring hybrids are found in every major plant group, and are common among

higher plants (see Grant 1981, part 5, for a fuller discussion of hybridization in plants, including the California white oak syngameon). Furthermore, many plant hybridizations regularly produce (as in a significant number of animals) nonsterile and even fully fertile offspring. The production of fertile offspring often results in the formation of *hybrid swarms*, that is, highly variable, but stable populations consisting of members of the two parent species, hybrids, and the offspring of hybrid backcrosses with the parent species. An even more complex arrangement is the *syngameon*, in which natural hybridization links anywhere from a few to a very large number of species together in one inclusive interbreeding unit. For all its complexity, the syngameon is relatively common in plants; in western American flora, for example, syngameons often occur more frequently than well-defined biological species (*ibid.*, 74).

The BSC is, of course, of little taxonomic help for hybrid swarms or syngameons. In the syngameon, we find species in continuous and stable reproductive interaction with large numbers of other species from which they are sometimes morphologically and ecologically completely distinct. In the California syngameon of white oaks (*Quercus*), for instance, *Q. garryana*, a forest tree with deeply cleft leaves, and *Q. dumosa*, a low shrub with small spiny leaves occurring in arid chaparral regions, are linked by *direct hybridization*.

Reproductively, a syngameon behaves like a biological species: It is reproductively isolated from other syngameons and other well-defined biological species. However, the considerable morphological and ecological differences between the component members of syngameons have led to their classification as distinct taxonomic species, despite the lack of reproductive isolation. This supports the contention that there are independent, biologically interesting questions about the morphology and ecology of organisms which do not turn on the issue of reproductive isolation. Similarly, we sometimes have good reasons to draw phylogenetic divisions on the basis of such morphological and ecological divergences, rather than restricting ourselves to a single principle of phylogenetic division which sometimes applies, but sometimes does not. If we are interested in the anatomy or physiology of organisms, whether as a principled way to draw phylogenetic divisions or a question of independent interest, it would seem to be a *grave error*, not to mention an unnecessary and frustrating impediment to a systematic vocabulary, to put a forest tree with deeply cleft leaves and an arid-dwelling shrub with spiny leaves in the same basket. What conclusions might we derive about the physiology and morphology of the "leaves" of such a chimera? Obviously, we should do what plant biologists have *already* done, and divide species within syngameons on morphological and ecological grounds, but we need not apologize for abandoning the BSC in explanatory enterprises in which

it is merely a hindrance. We should not, however, believe that a morphological species concept can *replace* the BSC wholesale, as we have already noted significant explanatory achievements, like the *Anopheles* mosquito, to which the BSC was clearly more useful than any morphological principle of species division might have been.

Hybridization pleads a particularly interesting case for the utility of an ecological principle of species division. Some species live sympatrically without hybridizing in some areas, but form full hybrid swarms in others. When such situations occur, the hybridization is frequently associated with disturbed habitats (Futuyma 1979). For instance, *Iris hexagona giganticaerulea* lives in exposed tidal marshes, while *I. fulva* lives on shady stream banks and in woods. In undisturbed natural habitats, the two occur sympatrically and do not hybridize. In disturbed habitats, however, the two produce partially fertile, introgressive (backcrossing) hybrid offspring with various combinations of characteristics from the parent species; populations of this hybrid occur in swampy areas lumbered and drained by humans (Riley 1938). Here hybridization gives us a reason to discriminate species on a principle of ecological division. Reproductive isolation is clearly inadequate (this is a fertile hybrid which backcrosses with the parent populations), and morphological differences are similarly unhelpful, as the hybrid population contains widely varying combinations of the morphological characteristics of the original species. The one consistent mechanism for discriminating these three groups of organisms seems to be the ecological niche they inhabit.

An ecological principle of division proves even more plausible and reasonable in an ecological explanatory enterprise. For instance, one might wish to know what it is *about* each of these three groups which permits it to inhabit the niche it does. If so, the morphological differences among hybrid forms become relatively unimportant, as do the reproductive relations between all three groups, and it seems natural to organize the groups for study by the niche each occupies. Such an enquiry could also be crucial to understanding the phylogeny of the organisms in question, for instance, in investigating how the geographical isolating mechanisms between these two *Irises* evolved in the first place.

While the advantages of identifying species by niche are clear in this case, the niches themselves are difficult to identify, and any number of equally legitimate conceptions of niche may apply to the same set of ecological circumstances. In addition, niches are not fine-grained enough to individuate species in the absence of genealogical considerations. This points out another advantage of the pluralist approach in that it permits the *combination* of grounds for individuating species in cases like the ecological one. After all, the classificatory apparatus involved in pursuing one biological interest need not always hamper the pursuit of another.

When individuating ecological species, it seems best to consider niche occupation as a *further refining principle* used in conjunction with genealogical considerations. Different principles of division are not always incompatible, and can sometimes be used together to great advantage; this should further convince us of the wisdom of legitimating multiple classificatory strategies.

Of course, the ecological species concept has its own limitations and problem cases. Independently interesting questions of ecology aside, many cases arise in which ecological considerations are simply orthogonal to the evolutionary history of particular speciation events. Furthermore, the ecological species concept seems to be of no help in discriminating some very distinct kinds of organisms. For instance, when instantaneous speciation occurs in plants by the formation of polyploid offspring which are reproductively isolated from the parent species, the ecological species concept seems powerless to distinguish the two kinds of organisms; nevertheless, they are different in important and systematic ways. Another problem for the ecological species concept occurs in the context of the laboratory manufacture and manipulation of organisms: Here we have no well-defined ecological niches for sorting organisms; nevertheless, we may have good grounds for wishing to distinguish such organisms from each other and from naturally occurring varieties.

The examples considered do not constitute an exhaustive survey of biological explanatory interests and their associated species concepts. They have gone a considerable distance, however, in empirically bearing out my claim that in order to study a particular feature exhibited systematically by organisms, a classification based upon that feature is likely to be most helpful. Perhaps more importantly, these examples support the claim that organisms do not segregate into identical groups with respect to each of their biologically interesting features; thus, we have independent and legitimate explanatory interests in biology which require distinct concepts of species. For classification to be of most assistance to empirical investigation, we must recognize this and legitimate a variety of classificatory schemata. Even if we consider only Kitcher's historical species concepts, he provides six different criteria for individuating such groups, each of which appears to serve a distinct explanatory interest in biology. While his toolbox might turn out to contain a spare wrench or pliers, Kitcher's tools are plausibly heterogeneous and his demand for pluralism well-founded.

It is worth emphasizing that we *could*, of course, simply "stick to our guns". We might say that reproductive isolation (for instance) is just all that there is to being a species, come hell or high water, and members of syngameons, paleontologists and blackberries be damned. We might even soften this a bit and roll out the epicycles of "semispecies", "mi-

crosspecies”, “subspecies”, “polytypic species” and “incipient species”. The point of these examples and my discussion, however, is to demonstrate that we hobble significant investigations in biology by limiting our conceptual and taxonomic resources in this way. We should instead accept the insights of pluralism, and legitimate a variety of criteria for the systematic division of organisms.

3. Biological Interests. Pluralist virtues are only part of the story. Kitcher’s (1984b) thesis is that “[s]pecies are sets of organisms related to one another by complicated, *biologically interesting* relations” (p. 309; emphasis added). That divisions must be “biologically interesting” is essential, because Kitcher inherits the pluralist’s traditional difficulty: replying to Mayr (1987), he recognizes the need to prevent pluralism from legitimating the “*suggestions of the inexpert, the inane, and the insane*” (Kitcher 1987, 190; original emphasis). While he holds that privileging a single criterion for dividing organisms into species is too restrictive, he must not allow just *anything* to constitute a species division. What Kitcher requires is a defense of *moderate* pluralism.

Kitcher’s suggestion is that organisms have a whole host of properties, and that different legitimate groupings can be made on the basis of different properties. The danger is that the division of organisms into groups based on *any* property will be legitimate (e.g., organisms with tails, and those without) and that these groups must count as species. Even if we were to require legitimate classifications to be hierarchically structured (like our own), we could easily envision a taxonomy based upon “means of locomotion” wherein higher divisions might correspond to having cilia, a flagellum, legs, wings, having no means of locomotion at all, and so forth. Middle-level taxa (e.g., in the “legs” group) could distinguish legs into unarticulated and articulated varieties, and low-level taxa might individuate, for example, on the basis of number of legs.² Although our taxonomy is based on a real biological property, we would not wish to call the groups thereby formed biological “species”. (For those who resist this conclusion, I point out that this taxonomy seems unable to separate such distinct organisms as spiders and crabs, or mice and Triceratops or virtually any plants from any other or from sessile animals like clams, into different species without introducing additional criteria.)

Kitcher claims that dividing organisms on the basis of “means of locomotion” is not grounded in any biologically interesting relation. He

²The proposed “locomotion taxonomy” is not historically ridiculous. Linnaeus’s “sexual system” classified plants by the morphology of their reproductive organs. He believed, however, that no change in the morphology of reproductive organs was possible, and therefore, that the taxonomic strategy he proposed would lead to further discoveries about the plant kingdom (Goerke [1966] 1973).

thus limits the division of organisms along any property into legitimate species concepts by introducing a further refining criterion, the explanatory interests of biologists. However, this cannot do the work that Kitcher demands. Claiming that species divisions must be “biologically interesting” merely pushes the problem back one step; it does *not* tell us whose interests qualify. More specifically, it does not provide grounds to include the interests of (for example) ecologists and physiologists, but exclude those of pheneticists and Creationists, as Kitcher would very much like to do. We must specify more concretely whose interests constitute legitimate species divisions.

Kitcher and others provide the raw materials for a more thorough defense of pluralist moderation. Kitcher (1993, chaps. 3 and 4) articulates and defends an account of two kinds of scientific progress: practical and cognitive. Practical progress is directed broadly at “the relief of man’s estate”: predicting and controlling the natural world. Cognitive progress further subdivides into conceptual and explanatory varieties. It is initially tempting to think that improving our classifications of organisms falls under the former category, for “[c]onceptual progress is made when we adjust the boundaries of our categories to conform to kinds and when we are able to provide more adequate specifications of our referents” (ibid., 95–96). I argue in sections 5–7 that this approach is indefensible. Instead, pluralists’ best strategy for defending moderation is to claim that species divisions are related to explanatory progress.

For Kitcher, explanatory progress “consists in improving our view of the dependencies of phenomena” (ibid., 105). He takes it as a scientific commonplace that some phenomena depend on others, and claims that capturing this explanatory order is the goal of our schemata, that is, general argument patterns which articulate the dependencies. In evolutionary biology, for example, the common possession of a trait by two species can be explained by establishing that (1) the species are descended from a common ancestor, (2) the ancestor possessed the trait, (3) the trait is heritable, and (4) no factors intervened to modify the trait along the phylogenetic sequences leading from the ancestor to the descendent species. This general explanatory schema of common descent can be applied repeatedly to explain diverse cases of the possession of some identical characteristic by separate species. An explanation of some particular phenomenon, then, is a schema with its variable terms filled in with the phenomenon being explained and the specific features of the world upon which it depends (ibid., 82–83).

For Kitcher, “our account of the structure of nature . . . [is] embodied in the schemata of our practices” (ibid., 106). These schemata are “better” or “worse” insofar as they capture dependencies: either part of the objective order of dependency of phenomena in nature (for the robust

metaphysical realist) or part of an ideal system for organizing the phenomena (for the less metaphysically strong-minded). Progress occurs when we abandon worse schemata for better.

His discussion of explanatory progress leads Kitcher directly into what is most useful to the defender of pluralist moderation: the account of erotetic progress, progress in the *questions* that we ask. Questions are directly related to schemata, Kitcher claims, in that questions acquire intrinsic significance when answers to them would exhibit the possibility of instantiating an accepted schema (especially in apparently problematic circumstances) or would show the possibility of some controversial presupposition of an accepted schema. Kitcher goes on to point out a further important and often-neglected feature of erotetic progress: that we progress when we render questions more *tractable*.

This, I claim, is precisely the important role in the progress of science played by species concepts. Species divisions are the handmaidens of erotetic progress: *They enable us to make the significant questions through which we extend successful schemata more tractable*. To briefly review, we improve explanatory practice by invoking schemata which fasten onto dependencies obtaining between natural phenomena. Erotetic progress is obtained by asking significant questions which extend, apply, or defend the presumptions of our increasingly successful schemata, and species concepts are accepted or rejected, I suggest, insofar as they render such significant questions more tractable.

This account of progress suggests various grounds on which the moderate pluralist might reject a proposed species division. First, a proposal to divide organisms into species by certain criteria might be *redundant*. That is, it might fail to make any significant question more tractable; indeed, the monist claims that all species divisions, save one, are redundant.

Second, a proposed species division might simply be *boring*. That is, it might advance a schema which fastened onto some dependencies in nature, but these dependencies might not help us to pursue further goals. This is the problem with the “means of locomotion” taxonomy: Surely *something* about organisms depends only upon their means of locomotion, but it is unlikely to be anything useful. This should not be confused with the claim that knowing the means of locomotion of a particular animal cannot be useful. The point is rather that no especially helpful dependencies can be detected which obtain solely or primarily in virtue of animals’ means of locomotion (e.g., the further properties possessed by all and only animals with four, articulated legs). We suppose that the answers to questions about *all* animals employing a particular means of locomotion will be too vague, general, obvious, and unrelated to their evolutionary trajectories to provide us with any helpful guidance in pre-

dicting and controlling the world, or in describing its plausible history. In Kitcher's terms, a boring species division makes no practical progress.

Finally, a proposal to divide organisms might be *wrongheaded* if it renders tractable significant questions whose schemata involve presuppositions we believe are incorrect. In this case the species division is rejected not because it fails to make questions advancing a schema more tractable, but because that very schema is misguided.

If there is a case against pheneticism, it seems to depend upon the claim that its species concept is boring: That is, the natural dependencies identified by the pheneticist's Operational Taxonomic Units are trivial and unhelpful in pursuing any practical end. The opponents of Creationism are more likely to claim that Creationism is wrongheaded, that is, that its explanatory schemata rest upon substantially mistaken presuppositions. The strength of the evidence for or against these claims must determine the acceptability of pheneticist or Creationist species to the moderate pluralist. If pheneticist species divisions are useful in advancing legitimate explanatory schemata, for instance, the moderate pluralist will have good grounds for accepting them.

We have now made concrete sense out of Kitcher's prescription that acceptable species must stand in "biologically interesting" relations: The proposed division must render more tractable questions which would extend a successful explanatory schema, and it must be neither redundant, boring, nor wrongheaded. In what follows, I use the expression "biologically interesting" in this technical sense.

Nothing about the defense of moderation, however, depends upon accepting Kitcher's view of scientific progress. Alternative accounts of rationality and progress are available, for instance, in the writings of I. Lakatos, L. Laudan, and S. Toulmin, and any of these views offers *some* grounds for excluding certain concepts of species even when we must "rationally" or "progressively" accept more than one. For example, Laudan's account of scientific progress justifies accepting just those species concepts which systematize organismic diversity most effectively for the resolution of various conceptual and empirical *problems*. Legitimate species concepts, according to Laudan, might differ from those defended on Kitcher's account. In general, any account should provide principled grounds for making determinations, and thus for excluding the suggestions of the "inexpert, inane and insane", even in the wake of a thoroughgoing pluralism regarding species.

4. Pluralism and Realism. Grounding judgements about which species divisions are biologically interesting in some account of rationality or progress emphasizes their historical relativity. That is, judgements of what is biologically interesting can only be made relative to a particular time

and theoretical context. In my extension of Kitcher's account, for example, judgements about what species divisions are boring or wrong-headed will necessarily involve evaluating which natural dependencies are most relevant to pursuing our practical goals and which explanatory schemata rely upon substantially mistaken presuppositions. These latter judgements can only be made with respect to a particular, historically contingent set of practical goals and in the context of a particular accepted theoretical background. Thus, we should expect to encounter many past taxonomic divisions which were biologically interesting at the time (and thus constituted legitimate species concepts), but would not be so today. Similarly, some modern species divisions would rightly have been rejected as biologically *uninteresting*, given the practical goals and theoretical presuppositions of biology in the past.

Consider the explanatory enterprise and associated species concept of Georges Cuvier at the turn of the nineteenth century. Cuvierian species were nonevolutionary: Each species represented all of the descendants of the first pair created by God, and no changes in the functionally significant characteristics of species were possible (Coleman 1964, 142–146). This was because certain necessary functional relationships (Cuvier's "conditions of existence") had to obtain between the various organ systems of creatures if they were to function and survive in the world; thus, only certain combinations were possible in actual organisms. Fulfilling these conditions of existence required a "correlation of parts" in an organism, which precluded the transmutation of a species: Cuvier argued that if any of an organism's functioning parts were modified, the creature could no longer exist (*ibid.*, 67–68).

Cuvier's chief explanatory interest was functional anatomy, and his classic studies in this subject resulted from his attempt to discover completely general "anatomical rules" governing the correlation of parts and conditions of existence in all organisms. These laws were of major importance to Cuvier because he believed that while organisms could not exist with those combinations of organ systems precluded by the anatomical rules, all of those which *were* possible actually existed in some organism. Thus, Cuvier believed that we could *deduce*, with mathematical certainty, what organisms and organ systems *actually existed* from the anatomical rules governing which were *possible* (*ibid.*, 68–71).

Cuvier's species concept made the extension of his explanatory practice tractable: The immutability of God's species under the conditions of existence and the necessary existence of all possible combinations of functional characters served to underwrite the possibility of general, *deductively valid* nomological claims regarding functional anatomy in all organisms. And it is equally clear that this explanatory practice was often successful: Cuvier actually managed to explain *and predict* many char-

acteristics of organisms by appealing to rules governing the functional interconnections of their organ systems (see *ibid.*, chaps. 3–5). Furthermore, neither this explanatory practice, nor the species concept it required, were wrongheaded at the turn of the nineteenth century; the facts about species which render it wrongheaded by contemporary lights (e.g., functional anatomical relationships are not immutable, many possible combinations of organ systems are not realized, species did not originate in a special act of creation) were then unavailable. By our pluralist criteria, the Cuvierian species concept was biologically interesting and thus legitimate in 1800.

This historical relativity of judgements of biological interest creates a serious problem, however, for Kitcher's claim that species are *real*. Recall that under moderate pluralism, only the biologically interesting divisions of organismic diversity constitute legitimate species. But as the Cuvierian example illustrates, which divisions are biologically interesting changes dramatically over time without any corresponding change in the organismic world. For the moderate pluralist, Cuvier's taxonomic principles constituted a species division in 1800 because they rendered questions extending a legitimate and successful explanatory practice more tractable. By the same criteria, however, Cuvier's proposal is not acceptable today.³ Similarly, a species concept defined in terms of ecological niches would *not* have helped extend any successful explanatory practices of the nineteenth century, and would probably also have been boring and wrongheaded. As we have already seen, however, the explanatory enterprises of contemporary biology require an ecological species concept.

Thus, what constituted a biologically interesting relation in the past has changed dramatically and we have no obvious reason to believe that it will not continue to do so. This makes Kitcher's realism problematic because legitimate species must then change with the explanatory schemata and practical interests of biologists. Recall that a species concept is not just *any* division of organisms; it is special in that it helps us investigate our pressing questions and problems. Thus, the *very same* division of organisms into groups may be biologically interesting (and therefore a species division) at one time, but not at another. Species can vary without any corresponding change in the physical circumstances of the world; that is, species do not supervene on physical circumstances. Instead, divisions of organisms that constitute species depend, for Kitcher, on the legitimate

³Present-day structuralists have attempted to revive ideal morphology and its species concepts (e.g., Rieppel 1988); however, they appeal to theory-free salient structures to ground their fundamental units. The many contemporary biologists and philosophers who reject the notion of theory-free classification as indefensible will view this proposal as wrongheaded.

interests of the biological community at a specific time, and thus, species are not independent of the states of particular minds. Kitcher (see below) requires natural kinds to be objective and permanent, thus, it is fair to demand mind independence (the classical *sine qua non* for the reality of elements of the external world) of his species, as a condition of their reality. Nonetheless, the historically relative criteria required to defend moderate pluralism will not permit his species to be real. While Kitcher's Kantian metaphysics makes *all* objects mind dependent in a weak sense, we have established that his pluralist species are dependent upon the states of *particular* minds, not simply the nature of minds in general, and this even Kantian reality will not suffer.

Notice that the claim here is *not* merely that species are not real because we have changed our minds about them: Precious little would be real under such a strict standard. Rather, the point is that by Kitcher's pluralist criteria, the legitimate interests of biologists *constitute* those divisions of organisms recognized as species. Thus, as the course of biological inquiry proceeds, we do not decide that we were previously *mistaken* about which groups of organisms were species; rather, as our explanatory and practical interests change, which divisions of organisms *actually are* species changes as well. As we have noted, which principles of division are biologically interesting can vary without corresponding changes in the world of organisms. Kitcher's species therefore lack that property, supervenience on the state of the mind independent material world, which we demand of real objects.

Kitcher (1991) maintains that his account of explanatory unification as a goal of scientific inquiry grounds an approach to realism which permits both pluralism and realism regarding species. It does so by grounding species concepts in experience in a particular way. We turn now to Kitcher's defense of the reality of species on these grounds.

5. Explanatory Unification and Realism. Kitcher (1991) rejects versions of realism which ascribe common essences to members of natural kinds, as defended, for example by Putnam (1975) and Kripke (1980). His reasons for considering these views problematic include general epistemic worries, such as how to justify judgements of properties as essential to certain entities or how to pick out objective similarities. Other problems more specific to particular conceptions of species are how to maintain essentialism in the face of Darwinian evolutionary theory (a problem noted by Mayr and Sober) and how to justify similarities as "in" nature, rather than projections of our ways of viewing it. Kitcher thus explicitly rejects traditional formulations of realism in favor of an alternative predicated on his account of explanatory unification.

According to Kitcher (1989a), explanatory unification as a goal of sci-

entific inquiry amounts to maximizing the number of phenomena which can be explained given a minimal number of argument patterns. On this view, natural kinds correspond to the divisions that are produced when we attain an ideal systematization of our experience, or the best organization of nature. That is, the order of nature is projected by us, but in a nonarbitrary fashion.

Kitcher (1991) defends the consistency between his pluralism and realism on the basis of this framework. He argues that ideal or ultimate biology will prove unable to restrict its species concepts down to one without sustaining corresponding significant losses in explanatory power. Kitcher admits that the difficult investigative work required to determine these tradeoffs is largely undone but at least points the way toward determining whether pluralism is really justified.

The chief flaw in this defense of realism is its dependence on the assumptions that species definitions are converging on a final set and that we are at or near the endpoint of this converging series. Kitcher solves the historical relativity/mind-dependence problem by declaring “real” the species concepts of one particular time: the end of biology. Thus, in order to merit the distinction of being real entities, the species concepts of *modern* biology must fulfill two conditions: First, they must be part of a series which is converging toward some endpoint in the ideal limit of inquiry; and second, they must represent either the endpoint of such a converging series, or something near to it, because:

The explanatory patterns that are objectively correct are those that would be properly accepted in the light of an ideally complete experience. The natural kinds are the extensions of the predicates that occur in the instantiations of these patterns over the corpus of beliefs rationally adopted, given the ideally complete experience. (Ibid., 1)

We reserve the title “real” (or “objectively correct”) for those explanatory patterns and associated natural kinds which are maximally unified *with respect to the ideally complete experience*. If it were possible to discover ideal biology tomorrow, but find that all of its species concepts were equally likely to differ from our own as to resemble them, the explanatory unification account would provide us with *no warrant to call our current conceptions “real”*. Thus the ascription of reality to any conceptions of current scientific practice would require a demonstration that (1) such concepts are converging in the ideal limit of experience, and (2) the ideally complete experience with respect to the phenomena investigated by the theory is here or within easy reach. In place of (2), Kitcher might make do with the weaker claim that the concepts generated at the end of an ideally complete experience are identical or very similar to the ones currently employed.

Not only are these theses controversial, but what might *count* as good evidence for any of them is unclear. After all, many fertile research concepts, such as Cuvier's immutable species, dominated much of the practice of their day. Nevertheless, had we accepted Cuvierian species as real in 1800 on Kitcher's grounds, we would have turned out to be mistaken. Notwithstanding the empirical adequacy, explanatory power, simplicity, fecundity, and other virtues of Cuvierian species, we were not at the ideal limit of experience in 1800, and we are not sure what might count as a good reason to suppose that we are today, or even that our concepts *must* be like those which would arise whenever we do reach such a limit.

Imagining what might count as evidence that species concepts are converging on those of the ideal limit is similarly difficult. Recall that, for Kitcher, convergence does *not* indicate a reduction in the *number* of species concepts; rather, convergence represents their approach to a set of concepts which maximally unify experience in the ideal limit. We can claim only that species concepts have changed over time; whether they are converging is a question that can only be answered when we know that the concepts we have at a particular time will *never* change in response to further evidence or experience. To know this, we would have to know in advance what the evidence will be; that is, we could only know if our species concepts were converging on those of the ideally complete experience *at the end of the ideally complete experience*.

A natural defense might claim that we call real whatever maximally unifies the phenomena of our *actual* experience to date. This avoids many difficulties, but also entails the damaging conclusion that what is real changes over time: That is, Cuvierian species were real in 1800 and are not real now, modern species are real now but might not be real in the future. This initially attractive move leaves us with a realism hardly deserving the name. The entities generated by a realism relative to particular experiences lack the kind of objectivity (or permanently real status) Kitcher wants for natural kinds, and this stronger objectivity condition seems to be his reason for embracing the notion of the ideally complete experience in the first place.

Thus, if Kitcher is to persuade us to share his pluralistic realism, he must demonstrate that species concepts are converging and that we are at or near the endpoint of the series. In addition, he must show how anything could count as evidence for these claims, since none of the ordinary sorts of virtues will do. Instead of belaboring the problems entailed by the difficult marriage of Kitcher's pluralism and realism, however, I consider an alternative view which recognizes the insights of Kitcher's pluralism without engendering the problems of attaching realism to it, and furthermore, resolves serious puzzles involved in the application of species concepts.

6. The Alternative View. Let us begin by granting some ontology of organisms and their properties. I would like to claim that species divisions among these organisms are based on a plurality of interest-relative grounds. They are not completely *arbitrary*, however, in that these interest-relative grounds may range only over the recognized properties of organisms. Any recognized property could in principle provide a species distinction, and the actual species concepts at any given time are those defined by the collective legitimate explanatory and practical interests of the biological community. If the fundamental and interesting questions of biology came to focus on organisms' means of locomotion, the locomotion taxonomy might become legitimate—a reasonable and useful way to cut up the world for study—but as things stand, it is not.

This approach relies on the fundamental insight of Kitcher's pluralism, that there are a multiplicity of legitimate explanatory demands in biology with associated schemata for the division of organisms, but avoids the difficulties that plague his realism. In addition, it recognizes the continual evolution of the biologists' explanatory interests by allowing the legitimate ways to divide organisms under study to evolve with them.

"Species" are thus the designations we use to pick out the significant and interesting distinctions between groups of organisms at a given time, that is, the differences that we wish to investigate. As previously noted, however, the useful schemata for dividing up organisms are various, and they shift as the explanatory interests of biologists do, with no corresponding shifts in the physical world that they organize. Species are not independent of the states of particular minds, and are therefore not real or objective in the traditional senses of those terms. They are nonarbitrary insofar as they are predicated on antecedently recognized biological properties of organisms, but no particular sort of property need be the basis of a legitimate species concept.

7. Advantages of Rejecting Realism. To this point, I have urged the rejection of realism in order to countenance both a variety of legitimate explanatory demands (and associated principles of division) in biology and the changing nature of these demands. I now turn to some independent reasons to reject realism, namely, the resolution of some problems that arise for particular species concepts when they are employed with realist assumptions. I claim that when these realist assumptions are abandoned, the features of species concepts which seem problematic cease to be so.

Splitter (1988) and Kitcher (1989b) express similar concerns about speciation criteria in the BSC.⁴ Splitter says the BSC does not meet its own

⁴Although Kitcher actually addresses D. Hull's "historical connectedness" as a criterion for species, he understands Hull to be using reproductive isolation as the division between species. The distinction need not trouble us here.

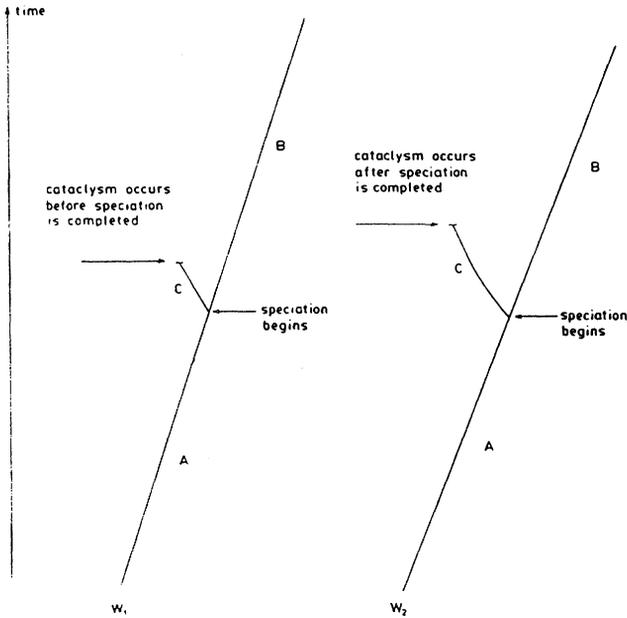


Figure 7.1. The Delayed Cataclysm Problem. From P. Kitcher's "Some Puzzles About Species", in M. Ruse, (ed.), *What the Philosophy of Biology Is: Essays Dedicated to David Hull*. Copyright © 1989 by Kluwer Academic Publishers. Reprinted by permission.

demands because, in cladistic speciation (the symmetric speciation of a parent species into two roughly equal daughter species), it fails to explain the demise of the parent species on any grounds other than formal desirability. That is, the BSC fails to account for what happens to the parent species, independent of what happens to the daughter species. (Mayr, however, is adamantly opposed to cladism, particularly to those features addressed here.) Splitter claims that the demise of the parent must be explained by what happens to the parent, not by its relationship to the two daughter species. His argument explicitly depends upon an intrinsic criterion of identity, "[W]ith respect to the question of whether or not entity *a* survives as (that is, is identical to) entity *b*, all other entities are irrelevant" (1988, 338).

Kitcher (1989b) introduces further puzzles about species concepts, including what I call the "Delayed Cataclysm" problem, best illustrated by an example (see figure 7.1). Consider one population of organisms from which a small group becomes geographically isolated. For those who permit only cladistic speciation, whether the original large population of organisms becomes a new species or not depends on whether the small group attains reproductive isolation from them before being wiped out by

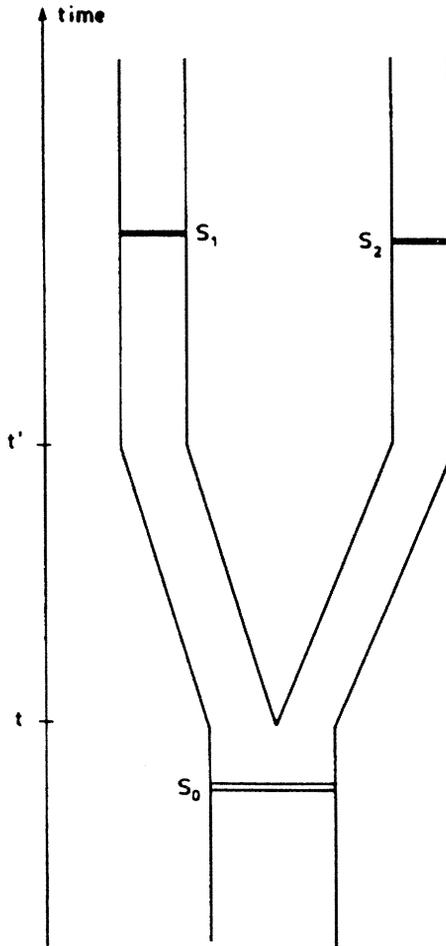


Figure 7.2. The Minimal Speciation Problem. From P. Kitcher's "Some Puzzles About Species", in M. Ruse, (ed.), *What the Philosophy of Biology Is: Essays Dedicated to David Hull*. Copyright © 1989 by Kluwer Academic Publishers. Reprinted by permission.

some cataclysm. That is, no change might occur in the large population, but because the small isolate attained reproductive isolation from them, the ancestral species must be viewed as extinct and both the large and small groups considered new species. Another problem Kitcher introduces I call the "Minimal Speciation" problem (figure 7.2), and it weighs against any scheme of speciation that is proposed (cladistic or otherwise). Consider d as the minimal distance needed (by whatever criterion for a speciation event) to constitute a separate species. Now consider an an-

cestral species *A* which divides into two daughter species *B* and *C*, which are d from each other while each is $1/2 d$ from *A*. Thus if either *B* or *C* were to become extinct, we would view the remaining branch as not significantly different enough from *A* to constitute a new species, but if both survive, we have two new species and the demise of the ancestor.

Kitcher's problems assume his (*) principle that "[a] proposal to count lineage-stages as stages of the same species should depend only on the intrinsic properties of and direct relations among those stages. It should give the same results in cases which differ only in the existence or properties of organisms occupying a different branch of the lineage" (ibid., 200). Thus Splitter and Kitcher's problems exhibit a fundamental similarity: Both point out that particular species concepts violate the commonsense standard (captured by the intrinsic identity and (*) principles) that what happens to something "over here" should not affect how we divide up the world "over there". More specifically, Splitter and Kitcher's principles hold that the coming to be and passing away of an entity must depend only on what happens to that entity and not on what happens to others.

This formulation, however, clarifies the realist assumption that underlies the intrinsic identity criterion and the (*) principle. We should expect such principles to apply only to real entities, that is, those whose constitution is mind independent. Therefore, these principles are inappropriate for entire classes of entities: In particular they will fail to apply to those entities whose existence and classification are not independent of our interests and activities. We have already considered a number of arguments in favor of taking species to be such entities, and the conceptual puzzles generated by applying realist metaphysical principles to species further support this proposal.

What counts as a legitimate species division among organisms depends upon what we find interesting about them, what we wish to explain about them, and what practical goals we have with respect to them, and this in turn depends upon what other organisms exist in the world and what properties they have. We should therefore *expect* species to violate the intrinsic identity and (*) principles. We have considered a number of independent arguments in favor of taking species to be the sort of entity to which Splitter and Kitcher's realist metaphysical principles do not apply, and the conceptual difficulties generated by realism about species constitute further support for this proposal. Once species have shed the mantle of realism, what were intractable puzzles about their behavior dissolve into smoke and mirrors.

If species are not real entities, it is perfectly reasonable to claim that whether both branches of a population survive is relevant to determining speciation: In the case sketched, the survival of both branches was min-

imally necessary to constitute an interesting difference between them. Similarly, we reject Splitter's demand for an explanation of the demise of parent species, independent of formal desirability. Once we forfeit realism regarding species, the formally desirable characteristics of speciation are all that remain to be accounted for. Since the parent species never was a real or independent entity, its demise requires no independent explanation.

This analysis transcends the boundaries of any particular species concept. Equally reasonable rival answers are permitted to particular cases of either Splitter's or Kitcher's puzzles, depending upon one's speciation criteria. For example, a peripheral isolate from the main body of organisms might count as a separate species if a criterion of reproductive isolation is employed, but not if morphological distinctness is used. In any case, these alternative possibilities no longer offend us once a realist posture toward species is abandoned.

Thus we solve a number of puzzles regarding species divisions and speciation by rejecting that there is anything puzzling about them. Once we recognize that species are not real or mind-independent entities, the assumptions which underlie the demands for explanation posed by Splitter and Kitcher are revealed as inappropriate. The inadequacies arise not in particular species concepts, but rather in the realism about species from which misleading demands for explanation quickly ensue.

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