What SPECIES can teach us about THEORY*

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Abstract

This paper argues against the common, often implicit assumption that theories are a singular kind of thing. Instead, I argue for *theory concept pluralism*: There are multiple distinct theory concepts which we legitimately use in different domains and for different purposes. We should not expect this to change. The argument goes by analogy with *species concept pluralism*, a familiar position in philosophy of biology. The SPECIES and THEORY concepts share a similar function: Species serve to group organisms as members of the same species, and theories serve to group together different proffered accounts as expressions of the same theory. Biologists, in studying the biological world, use multiple concepts; as do philosophers of science, in studying

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science. None of which concepts could be used in place of all the others and each of which is more useful than the others within its domain of application. I conclude by distinguishing theory concept pluralism from other pluralist positions in philosophy of science; specifically, theory concept pluralism's insistence that multiple theory concepts are legitimate is distinct from the claim that we should embrace multiple, possible even incompatible, theories about some specified phenomena.

1 Introduction

David Hull, writing about species, comments offhand that "philosophy of science deals primarily with theories and their development." [Hul97, p. 371]. Indeed, the question of what theories are is one of the stock issues in philosophy of science. Even when it is not explicitly discussed, it arises; for example, discussing theory confirmation necessarily presupposes something about what these *theories* are that are confirmed.

This paper argues against the common, often implicit view that theories are some specific kind of thing. Instead, I argue for *theory concept pluralism*. The argument goes by analogy with *species concept pluralism*, a familiar position in philosophy of biology. Insofar as the reasons for species concept pluralism can be adapted to the case of theory concepts, the analogy provides reasons for the theory concept pluralism. Even if the reasons are taken to be insufficient in the case of species, the analogy illustrates the kind of pluralism I am advocating.

In §2, I describe species concept pluralism, the source for the analogy. In §3, I argue for the analogy with theories. The core of the analogy is this:

> The concepts serve a similar function: species serve to group together different organisms as members of the same species, and theories serve to group together different proffered accounts as expressions of the same theory. Biologists, in studying the biological world, use multiple concepts; as do philosophers of science, in studying science. None of which concepts could be used in place of all the others, and each of which is more useful than the others within its domain of application.

In §4, I consider some objections. In §5, I consider the relationship between theory concept pluralism and some other pluralist positions.

2 Species concepts

The concept of a particular species organizes individual organisms into members of the species and non-members. For example, the concept DOG sorts the dogs from the non-dogs. I will use capital letters (e.g., DOG) to indicate the concept and lowercase (e.g., dogs) to indicate the plural aggregate. So dogs are animals, and DOG is a concept; both are distinct from the concept SPECIES.[1]

There is some debate over whether the species of dogs is a *set* (such that each individual dog is a *member* of the set) or a temporally-extended *individual* (such that each distinct dog is a *part* of the species.)[2] Regardless, the particular species parses the dogs from the non-dogs.

The concept SPECIES counts CANIS LUPUS as a species, for example, but TACO as a non-species. The kind of sorting that CANIS LUPUS does is a species sorting. Considering two individual organisms, the concept SPECIES can tell us whether there is a species that includes both; are these two critters of the same species?[3] There is disagreement not only about where exactly to draw the lines between different species, but also about which features of organisms are even relevant for doing so. Biologists employ distinct SPECIES concepts with different organisms and for different purposes.

Species concept pluralism is the view that all or many of the these different ways of divvying up species are legitimate. They are equally scientific, they pick out equally real species, and we should neither expect nor want them to be pruned down to a single, monolithic SPECIES concept. Understanding the position requires considering at least some of the different SPECIES concepts on offer. Mayr [May42, ch. 5] lists five SPECIES concepts used by biologists, Mayden [May97] lists over twenty-two, but an exact enumeration is unnecessary here. It will suffice to consider three general SPECIES concepts:

[1] I do not mean to be presuming too much by talking about 'concepts.' Machery [Mac05][Mac06] argues that CONCEPT is not a natural kind — if he is right, then the capitalized expressions can be treated as kinds rather than as concepts.

[2] I use the words 'member' and 'part' without meaning to beg this question. The debate is incidental here, since species concept pluralism is compatible with either metaphysical view.

[3] Of course, if actually asked, one might just look at two dogs and say that there is a species that includes both because one has the concept CANIS LUPUS which applies to both. However, one's having a concept that applies to two organisms is not sufficient for them to belong to the same species. One needs to know whether the concept is the concept of a species, and that requires explicitly employing SPECIES.

phenetic, biological, and phylogenetic.

Phenetic species (also called morphological or typological species) distinguish membership on the basis of organisms' exhibited characteristics. The species in Linnaeus' 18th-century taxonomy were phenetic, sorting organisms on the basis of their observable properties. In work on algae, phenetic species are still used most often, although the distinguishing properties may be chemical or molecular [JM97]. The PHENETIC SPECIES concept allows for every organism to be included in some species. By appealing to exhibited features, systematists can readily identify organisms and arrange them into named groups. One problem with phenetic species is that there is no luminous threshold indicating how much structural or chemical differences suffices to separate one species from another. More seriously, phenetic species concepts are only snapshots of populations at a time. As Hull explains, they "are designed to individuate time slices of evolving lineages" [Hul97, p. 375]. After Darwin, we think that evolution and history of descent are important — but history is not a phenetic consideration.

Biological species identify organisms as members of a reproductively-isolated, interbreeding group. The BIOLOGICAL SPECIES concept was introduced in the 20th century and has been formulated in various ways. It is useful for many purposes and has a claim to being the true SPECIES concept if any concept does. However, it faces several serious problems. First, it is a complete disaster when applied to asexual organisms; either they are not part of any species at all, or each individual organism is its own species. Second, allopatric and allochronic groups (populations in different places at different times) count as distinct species just because of their separation. To take an extravagant example, imagine a frozen neanderthal thawed out in the present day; it seems wrong to say (as the BIOLOGICAL SPECIES concept must) that he is not part of the same species as his parents and kin. Third, it is difficult to categorize hybrids. Fourth, it is operationally difficult to determine whether populations in the wild do interbreed. Fifth, it is conceptually difficult to say how much interbreeding is enough to make for a unified group; strict criteria for interbreeding would lead one to count more species more course-grained criteria. Despite its successes, then, the BIOLOGICAL SPECIES concept cannot suffice for all biological enquiry.

Phylogenetic species select organisms of common descent in the smallest groups that could be subject to evolution and natural selection. The PHYLOGENETIC SPECIES concept is thus tied directly to evolution. As such, it serves the systematists' aim of discovering the evolutionary relationships

between organisms. Yet it faces several difficulties.

First, the evolutionary vantage point makes it hard to apply in practice. An organism's ancestry is not a readily observable property of it. Even where we have *some* idea of what a creature's evolutionary history might be, phylogenetic classification depends on those auxiliary hypotheses about natural history. These auxiliaries change as we learn more, making the system of classification unstable. As Purvis argues, "Systematics has two principle objectives, namely to communicate the identity of an organism by means of latinized names, and to indicate the probably evolutionary relationships of organisms" [Pur97, p. 129]. The PHYLOGENETIC SPECIES concept effectively abandons the former to pursue the latter.

Second, as Ereshefsky [Ere01] has argued, phylogeny gives us no precise way of distinguishing how large a group counts as a species. It removes any principled distinction between genus, species, and subspecies. The Linnaean hierarchy breaks down, and species are only determined by an arbitrarily specified fineness of grain. Considered in a course-grained way, a large family tree might count as a species; considered with finer criteria, the species might just be a branch of the larger tree. (As we have seen, similar worries plague the other concepts.)

Third, some organism do not have well defined lines of descent. Bacteria trade genetic material in complicated, cross-cutting ways; as Franklin argues, "Because of divergences among the phylogenies of different organismal parts... there are no particular lineages that we can appeal to when delimiting species" [Fra07, p. 71].

The details of species concept pluralism might be handled in different ways. One option is to say (with Ereshesfky [Ere92]) that there is no univocal concept SPECIES at all, but instead there are distinct concepts PHENETIC SPECIES, BIOLOGICAL SPECIES, and PHYLOGENETIC SPECIES. Another option is to say (with Brigandt [Bri03]) that there is a single but complex concept SPECIES which can take in phenetic species, biological species, and phylogenetic species. Perhaps these are just verbal variants. I am not concerned to decide between them. The core of the view is that phenetic, biological, phylogenetic, and perhaps other species concepts are all legitimate parts of biology. As Hull puts it, pluralism requires that "quite different and incomplete species definitions be considered equally good in their own domain" [Hul97, p. 364].

One may worry that species concept pluralism opens the door for methodological anarchy. If anything can count as a SPECIES concept, then there must

be some species which includes any arbitrary collection of organisms — but that would be absurd. The would-be reductio fails, because species concept pluralism does not have that consequence. John Dupré champions a form of species concept pluralism which he dubs *promiscuous realism*: the view that "there are many sameness relations that serve to distinguish classes of organisms in ways that are relevant to various concerns ... [and]... none of these relations are priveleged" [Dup02, p. 33]. Kellert *et al.*, who themselves champion a kind of pluralism, suggest that "promiscuous realism is hard to distinguish from radical relativism" [KLW06a, p. xiii]. So it seems fair to presume that Dupré's position would yield anarchy if *any* species concept pluralism did. As I argue below, it does not. So the reductio fails.

As part of his promiscuity, Dupré argues that classifications made for human purposes are just as legitimate as those made according to the SPECIES concepts discussed above. We distinguish broccoli from brussels sprouts, for example, despite their botanical similarity; both are *Brassica oleracea*. We group pines together on account of their timber, despite their botanical difference. Nevertheless, Dupré still acknowledges limitations on what might count as a species. We can readily recognize that culinary and lumbering concerns are not the concerns of biology. Dupré's examples are garlic and onions [Dup02, p. 34] and cedars [Dup02, p. 29]. He is explicit that those kinds — though real — are not species.

As far as I can tell, the radical relativist pluralism which accepts *any* concept as a species concept is a straw man. Even though they serve to pick out real kinds, our practical categories do not serve to pick out species. There is no place in biology for a GUSTATORY SPECIES concept divides up *Brassica oleracea* in a way suited for restaurant menus or that categorizes most animals as close relatives of chickens.

Note that Dupré is often misread on this point, as by Kellert *et al.* in the passage quoted above and by Reisch [Rei98]. Reisch worries that species concept pluralism allows creationism to stand on the same footing as evolutionary biology. He claims that Dupré could answer this worry only if he could "show that the epistemic interests and efforts of creationists to structure the world are somehow not legitimate or genuine" [Rei98, p. 341], but this gets the matter backwards. One need only argue that creationist accounts fail to fulfill the epistemic interests of scientific biology. It would be a tangent to pursue this point here, but I will suggest how the argument would go: The three species concepts I have discussed do a better job structuring the description and explanation of organisms and populations, their complexity and

history, than would any creationist alternative. So the DIVINELY-ORDAINED SPECIES concept would have no place in scientific enquiry, and creationism has no place in the science curriculum.

To review: Species concept pluralism is motivated by the fact that scientific practice fruitfully employs distinct sets of criteria for what counts as a species. Which SPECIES concept or set of criteria is appropriate in a given biological enquiry is decided by the particular objects of enquiry (what the organisms are), the available evidence (whether there is any evidence at all of the organisms' ancestry, for example), and the specific questions being asked.

3 Theory concepts

In this section, I argue that THEORY is analogous to SPECIES in the ways that motivate concept pluralism. Although the argument might extend to THEORY in general, I intend SCIENTIFIC THEORY in what follows.

Theories are the basic unit of scientific commitment. Considering two scientists who each have accounts of the same phenomenon, we can ask if they have the same theory. The concept THEORY determines which criteria are relevant to this judgement. This function of the THEORY concept suggests the analogy with SPECIES: Just as the SPECIES concept determines whether two organisms are of the same species or not, the THEORY concept determines whether two proffered accounts are the same theory or not.

Of course, the analogy between the SPECIES and THEORY concepts is not perfect. For example, the ontology of theories is importantly different than the ontology of species.[4] Nevertheless, the features that figure in the argument for species concept pluralism are present in the case of theory: There are several distinct THEORY concepts which are employed by practitioners in science studies and philosophy of science. Some of the concepts are inscrutable in important instances. Some of them depend on an arbitrary fineness of grain. Some overlook important temporal and contextual features. Ultimately, no concept applies usefully in all cases.

In this section, I consider statement, semantic, cognitive, toolbox, and historical conceptions. It is not essential for the analogy that all of these are

[4]As noted above, a species is either a set of organisms or an individual composed of organisms. A theory, even though it can be expressed by individual scientists, is neither a set of such expressions nor composed of such expressions.

ultimately defensible; it will suffice if there is more than one. The point is to show (positively) that each is used productively by philosophers of science and (negatively) that none could do the work of all the others.

In the first half of the 20th century, theories were typically treated as axiomatic systems (sets of sentences closed under logical implication) along with correspondence rules that translated the theoretical terms of the theory into observational vocabulary. This was once called the Received View, but that is no longer appropriate. It has more recently been called the Once Received View [Cra02], but to give it a less awkward name we can call it the STATEMENT THEORY concept.[5] A statement theory is something expressible in a logical language; this allowed logical empiricists to use the same methods of formalization and analysis that they applied in so many domains. Yet there are substantial difficulties with the approach: First, it draws the observational-theoretical distinction as a distinction between two kinds of vocabulary. As van Fraassen [van80] shows, however, even the most recondite, unobservable objects can be referred to using observational vocabulary. Second, because it individuates theories as linguistic entities, questions of theory identity become questions of translation. Imagine we are considering two scientists discussing similar experimental systems. They say somewhat different things, write somewhat different formulae on chalkboards, and so on. We want to know whether they are employing the same theory (which they have formulated somewhat differently) or employing different theories. This is essentially the problem of translation from one formulation (utterances and inscriptions) into another; if the formulations are identical under translation, then the scientists have the same theory. So questions of theory identity become hostage to the indeterminacy of translation, a significant problem in its own right. Despair over this problem led Quine [Qui90] to abandon talk of 'theory' entirely, in favor of talking only about theory formulations.

If the STATEMENT THEORY concept had simply failed, then it would not be legitimate even given theory concept pluralism. Recall that pluralism means that there are multiple legitimate concepts, not that any concept is as good as any other. Despite reports of its demise, however, the STATEMENT THEORY concept is still widely used. For example, it is often presumed by philosophers working in Bayesian confirmation theory who construe evidence and theories as sentences, typically in a first-order language. The sentences

[5]It makes no difference for my purposes if we substitute *propositions* for *statements*.

are assigned probabilities by enquiry (rather than truth values) and agents have degrees of belief (rather than univocal beliefs), so one might argue that this is not exactly the Received View. Nevertheless, theories are treated as primarily linguistic entities. It would be a tremendous digression here to offer a defense of the literature on Bayesian confirmation. If the reader grants that at least some of it is worthwhile, then that shows that STATEMENT THEORY is still useful — at least in that domain and for those purposes.

The SEMANTIC THEORY concept (usually called the *semantic conception* or *structural conception*) was developed in response to the logical empiricist view. It treats a theory as a set of formal models or abstract structures. The models stand in mapping relations to the phenomena. This way of thinking avoids problems with translation that arise for theories construed as linguistic. The SEMANTIC THEORY concept was motivated in part by von Neumann's 1932 generalization of quantum mechanics [Sup00, p. S105]. Von Neumann showed that two distinct formulations (Schrödinger's wave mechanics and Heisenberg's matrix mechanics) could be generalized in the language of Hilbert spaces. The SEMANTIC THEORY concept allows us to put it this way: He showed that the two formulations were expressions of the same theory. Yet wave mechanics and matrix mechanics both underwent substantial development between 1926 (when physicists started to treat them as equivalent) and 1932 (when von Neumann proved that they were). Muller [Mul97] has used the structural approach to show that the two frameworks, as they were in 1926, were *not* actually equivalent. This nicely illustrates the resources of the SEMANTIC THEORY concept for judging both theory identity (versions of quantum mechanics in 1932) and non-identity (versions of quantum mechanics in 1926).

Despite aspirations of a unitary approach,[6] the SEMANTIC THEORY concept has shortcomings. Of course, in the crudest sense, any theory can be represented as an abstract structure. Yet there must be something besides the abstract structure itself that differentiates theories. Consider, for example, the diverse range of systems that can be modeled as harmonic oscillators: pendulums, weights on springs, balls rolling in bowls, diatomic molecules, and so on. There would be something odd about calling our accounts of all such systems the *same theory* on account of this. As Knuuttila observes, "the question of representation... becomes acute once we grant that much scientific reasoning operates on other representative means than (propositional)

[6]Here echoing the subtitle of the recent book by da Costa and French [dF03].

language" [Knu05, p. 1263].

SEMANTIC THEORY also has the awkward consequence that it becomes impossible to believe a theory. Belief is a propositional attitude, after all, and semantic theories are not the kinds of things that one can believe. At most, one can have beliefs *about* them. Ordinary claims like 'Mary believes the germ theory of disease' must be reinterpreting as meaning that Mary believes the world is structurally like the theory.

A further shortcoming of the SEMANTIC THEORY concept is that it considers theories as static things. As Suppe admits, "Theories undergo development. This has implications for theory individuation. In present forms the Semantic Conception essentially treats theory development as progression of successive theories" [Sup00, p. S108]. He is optimistic that the semantic conception can be developed to address this shortcoming, but there are reasons to suspect that there are inherent limits to its resources. Mattingly [Mat05] argues that the peculiarities of theory formulation are sometimes crucial for the development of a theory. By abstracting from such detail, the SEMANTIC THEORY concept overlooks the features of theories that are crucial in their reception and extension.[7] This will not always be a problem, but (to take one example) the purely structural point of view would smear out the difference between Feynman diagrams and corresponding differential equations. As Kaiser [Kai05] shows, the difference between them is crucial for understanding the development of particle physics in the latter half of the 20th century. This is just the sort of situation that suggests pluralism; for enquiries in which theory development is focal, other theory concepts may be more appropriate than semantic concepts.

No other theory concepts are as well-developed or as often-articulated as STATEMENT THEORY and SEMANTIC THEORY, but I want to briefly consider three other concepts which have proven fruitful in specific enquiries: cognitive, toolbox, and historical. (The labels 'cognitive' and 'historical' are my own, although the concepts have been advocated by various philosophers.)

A COGNITIVE THEORY concept individuates theories relative to the process of theoretical understanding. A theory is what the agent cognitively grasps, the structure present in the scientist's mind or brain. Churchland [Chu89][Chu98] has championed thinking of theories in this way. Using the

[7] Da Costa and French [dF03] attempt to accommodate theory change within the semantic conception by equating the growth points of theories with neutral analogies, features which are not yet judged to match or diverge from phenomena. It is unclear whether or how this captures formulation-dependent lines of development.

connectionist framework which treats brains as neural networks, he identifies theories as structures in a scientist's neural net.[8]

Because this differentiates theories relative to the cognitive structure of understanding, the COGNITIVE THEORY concept is surely applicable to any theories that humans could understand. Since these structures are not necessarily linguistic, it further allows us to meaningfully attribute theories to non-human animals. Obviously, the STATEMENT THEORY concept could not do this. The SEMANTIC THEORY concept also has difficulty doing so, since the relevant structures must be designated in some way; for example, with set-theoretic predicates.

Perhaps there are theories that are too complicated for a mere human to comprehend, but Churchland's COGNITIVE THEORY concept can handle them by considering the structure of connectionist networks sufficiently large to understand them. If we designed computers to reckon with such theories, however, we might not implement them as connectionist machines at all. Rather, we might implement them as standard rule-based programs. In that case, it would be most natural to represent them using the STATEMENT THEORY concept.

Even considering theories that humans can understand, Churchland's COGNITIVE THEORY concept puts theory membership beyond empirical determination. Although something was going on in Schrödinger's brain and something in Heisenberg's brain in 1926, we cannot say precisely what. So a COGNITIVE THEORY concept makes it impossible to say anything interesting about whether wave mechanics and matrix mechanics were the same theory. The problem is partly historical, because those brain states are over eighty years gone. There is a further problem that, even for scientists in the present, we have no way of scanning brains to measure the structures that embody theoretical understanding. The problem is parallel to the difficulty that arises for the PHYLOGENETIC SPECIES concept: It makes theory (or species) membership well-defined in every case, but it puts membership beyond empirical determination.

A TOOLBOX THEORY concept shifts attention away from theories as the primary unit of analysis. For example, Cartwright has argued that scientific theories considered as general accounts of the world are simply false. Sci-

[8]The details are not important here. He initially identifies theories as the array of weights in an agent's neural net [Chu89]; later, as the partitions in the net's state space [Chu98]. The latter formulation allows him to provide criteria for when two nets have the *same* theory.

entific representation of the world is accomplished instead by constructing models of specific phenomena. Theories provide resources for constructing models, but scientific expertise is always required to make the structures of the theory fit the complications of the phenomenon. Theory is not a general representation, but a toolbox for constructing particular representations.[9]

Cartwright *et al.* [CSS95] criticize (what I have called) the STATEMENT THEORY and SEMANTIC THEORY for presuming a covering-law account according to which models are supposed to be strict consequences of a theory. They write:

> This account gives us a kind of homunculus image of model creation: Theories have a belly-full of tiny already-formed models buried in them. It takes only the midwife of deduction to bring them forth. On the semantic view, theories are just collections of models; this view offers then a modern japanese-style automated version of the covering-law account that does away even with the midwife. [CSS95, p. 139]

Their approach treats all representation as local, and theory is just a tool for constructing the local representation. This approach is especially apt when considering hybrid physical models, parts of which are classical and other parts of which are quantum or relativistic. Such models do seem to be hammered together opportunistically using the resources of different theories.

Yet the covering-law account is separable from the STATEMENT THEORY and SEMANTIC THEORY concepts, and philosophers of science want to do more than just account for the creation of specific models. Scientists also engage abstract theorizing, reckoning with theories as abstract representations. Morrison [Mor07], who concurs with Cartwright *et al.* on the autonomy of models, argues for preserving some sense in which theories are representations. As I would put the point: We need more than just the TOOLBOX THEORY concept.

An HISTORICAL THEORY concept individuates a theory as a moment in an ongoing research tradition. Kuhn's [Kuh70] notion of paradigms and Lakatos' [Lak78] notion of research programmes involve conceiving of theories in this way. This differs from merely considering the features of a theory that are

[9]Regarding the literal falsity of theories, see [Car83]. Regarding model building, see [Car99]. Cartwright *et al.* [CSS95, esp. §2][CS08] further articulate the TOOLBOX THEORY concept.

important for its further development, because it makes its position in the ongoing tradition partly constitutive of the theory. The same formulae or abstract structures in a different tradition would comprise a different theory. This approach is not always rewarding; Kitcher complains that "the game of finding paradigms, protective belts, or research traditions in the actual course of events becomes highly arbitrary and often unprofitable" [Kit93, p. 89]. Yet thinking in this way can be useful in specific analyses; e.g. [SM07]. Its failure in general shows only that it is not always the appropriate way of thinking about theory. That would only be decisive if we were engaged in the monist's project of trying to find the one correct THEORY concept.

Perhaps some THEORY concept could somehow do the work of all these, describing all the myriad theories. If it could, then pluralism — as a methodology — seems like the best way to discover it. If the philosophical community works with several different THEORY concepts, refining each and applying them where they are fruitful, then someone might eventually conceive of this elusive best concept. It this is impossible, then again pluralism is the right way to proceed.

Given the THEORY concepts we actually have, most could not *conceivably* apply to all theories, and none of them can *usefully* be applied in all cases. Nevertheless, each of the ways of thinking about theories has been useful in particular studies. This situation recommends pluralism: There are several legitimate THEORY concepts, useful in different domains, for different enquiries. All have legitimate work to do in science studies and philosophy of science.

4 Some objections considered

In the previous section, I made the *prima facie* case for theory concept pluralism. Next I'll consider two natural objections.

One might object: Pluralism is really just a defeatist refusal to do philosophy. Philosophers have a difficult time giving an account of theory, to be sure, and none of the present arguments are decisive. If *these* arguments do not need to be resolved, however, then the same evasion might be given in every area of philosophy. Philosophers have a difficult time giving an account of ethics, truth, mind, and so on — should we then be pluralists about utilitarianism and deontology? correspondence and deflationism? physicalism and dualism? Obviously, we cannot answer 'yes' to these questions; in each

case, at most one of the two views can be true.

In response: The situation with THEORY concepts is not like the disagreement (e.g.) between dualists and physicalists. A monist adherent of the SEMANTIC THEORY concept need not say that there are no such things as the linguistic structures described by adherents of the STATEMENT THEORY concept, only that those abstract entities are not theories. If this is anything more than a verbal tug-of-war over the label 'theory', then it is a claim about what methodology philosophers of science ought to employ. The adherent of a specific THEORY concept is not arguing primarily about what exists, but instead about which categories can be fruitfully employed when studying science. I have argued that there is no single THEORY concept that will suffice.

The argument that I am giving here does not readily generalize to other areas of philosophy.[10] It relies on specific analogies between SPECIES and THEORY.

Moreover, theory concept pluralism does not simply dissolve the various arguments for and against each THEORY concept. For the monist, these arguments are taken as reasons for and against thinking that a concept is the one, true THEORY concept — but that only makes sense if we presume, with monist, that there is a unitary THEORY concept us to discover. As a pluralist, I refuse to accept that premise. Yet the old arguments for and against each THEORY concept are still of some use. We do not need to decide between THEORY concepts once and for all, but we still need to do so in particular instances. The old arguments can be taken as articulating the kinds of cases in which a concept can be usefully employed. For example, we should not employ the SEMANTIC THEORY concept when considering a case which turns on details of a theory's formulation or history.

So pluralism is a refusal to strain over arguments until the pure form of THEORY descends from heaven, but it is not a refusal to do philosophy. It accepts the burden of trying to understand what theories are, in their complexity.

One might instead object: If species concept pluralism is correct, it is because biologists have a number of distinct and competing aims. Philosophers of science have, as their main aim, giving the correct account of scientific

[10]Reasons for pluralism should be sensitive to the kinds of things we are supposed to be pluralist about. For example, Machery (cf. fn. 1, above) argues for pluralism about concepts along different lines. There would be something suspicious about a wholesale argument claiming to show that pluralism is appropriate in every domain.

explanation. The correct THEORY concept is whichever one figures in that account. Perhaps none of the THEORY concepts now on offer are adequate, but that just suggests that we have not yet arrived at the correct THEORY concept.[11]

The objection presumes too much unity in philosophy of science. Philosophers are interested in explanation, but also in induction, confirmation, evidence, experiment, modeling, questions of realism, differences between special sciences, and many other separate issues. Most of these relate to explanation in some way, but they are not exhausted by that relation. Inference to the best explanation figures in many debates about realism, for example, but not in all of them.

Even granting that a theory is whatever can be offered as an explanation, the argument only defeats theory concept pluralism if there is a single, true EXPLANATION concept. One might just as easily infer *from* theory concept pluralism *to* explanation concept pluralism. Note that this inference would not be because of a general license to be pluralist in every domain. It would only follow if, as the objection posits, THEORY and EXPLANATION are intimately connected.

This is the 21st century, and philosophers will no longer say (for example) that first-order logic with identity exhausts what there is to say about logic. We should not say, either, that any single formal account of theory exhausts what there is to say about theory. Nevertheless, formal accounts are useful. First-order logic is a well-explored and important formal system, even though we recognize that it is only one possible logic among many — we are all logical system pluralists. Theory concept pluralism adopts a similar attitude toward theory concepts, adopting those that are useful where and when they are useful. Perhaps some of the theory concepts I listed in the previous section will ultimately prove dispensable, just as Aristotelean logic was effectively subsumed by first-order, quantified logic. The only way to find out is to keep all of them in play, and see which can sustain progressive research programmes and which cannot.

5 This and other pluralisms

'Pluralism' has become a popular slogan in recent philosophy of science. For many philosophers, it is motivated by the idea that all representations are

[11] This objection was suggested by Ulrich Meyer.

partial. We cannot presume in the advance that the world is simple enough to be represented by a single supreme theory. All that we have now are different disjoint theories which are adequate for some purposes but not others. This is taken to suggest that we have — and should be satisfied with — a plurality of theories. Yet this is not a claim about the THEORY concept itself. The plurality of theories might all be theories in the same sense; semantic theories, for example. So being a pluralist about theories is compatible with thinking that exactly one THEORY concept is adequate; that is, one can be a pluralist about theories without being a theory concept pluralist.

This is not merely a scholastic distinction. Some philosophers accept pluralism of one kind without accepting the other. Consider Ron Giere, who calls for "a dose of pluralism in the philosophy of science" [Gie09, p. 111]. He explains: "So the philosophy of evolutionary theory need not look like the philosophy of quantum mechanics. Of course there would be similarities, the role of some kinds of models being a prime candidate" [Gie09, p. 111]. Giere's point is that, although different sciences yield very different representations, their representations are still importantly the same *kind* of thing. Elsewhere, he complains about the treatment of scientific theories as linguistic entities (what I've called here the STATEMENT THEORY concept.) He sees it as underwriting a fixation on laws as universal generalizations, which in turn he sees as underwriting the view that there must be one true theory. So the theory concept leads to monism about theories; the "monist... understanding of scientific knowledge is facilitated by a particular understanding of the nature of theories and theorizing" [Gie06, p. 32]. So Giere insists that pluralism *requires* thinking about theories and models in a specific way — that pluralism about scientific theories requires adopting a specific theory concept.[12]

In contrast, consider Kellert *et al.*, who identify the 'pluralist stance' [KLW06a]. It is, on the face of it, pluralism about theories: "[A]lternative representations of a phenomenon can be equally correct ... [and]... different accounts, employing different representations, might be generated by answering different questions framed by those different representations" [KLW06a, p. xv]. However, the pluralist stance as they explain it should make one sympathetic to theory concept pluralism. The stance is a general outlook on

[12]Although Giere has explicitly allied himself with the semantic conception of theories, his view might better be seen as a variety of what I have called the TOOLBOX THEORY concept. What matters here is that he argues for a specific THEORY concept while arguing for pluralism about theories.

enquiry. Applied to biology, it leads naturally to species concept pluralism. Writing about the related issue of FITNESS, the authors explain:

> Monism leads many philosophers to search for the concepts that will enable the pieces to fall into a single representational idiom. For example, philosophers were not content to identify a plurality of fitness concepts that could be drawn on to describe different aspects (or even different instances) of evolution. The explicit aim was to clarify *the* fundamental concept that underwrites all explanations invoking natural selection. The unspoken assumption was that there must be some underlying causal parameter, fitness, that would be the basic cause for all cases of natural selection. Pluralism denies this assumption. [KLW06a, p. xxv]

As the authors are aware, philosophy of science is itself a variety of empirical enquiry. The pluralist stance thus has consequences for its method. They write:

> Pluralists do not assume that if we could "get clear" on essential concepts, biologists could empirically determine how everything can be explained by a single account based on a few fundamental principles. By denying such assumptions, the pluralist stance requires us to revise the way we analyze concepts, both those of science and metascience. [KLW06a, p. xxv]

As such, the pluralist stance leads to theory concept pluralism by two paths. First, the pluralist stance leads to species concept pluralism which (I have argued) provides a motivating analogy for theory concept pluralism. Second, the pluralist stance seems to lead to x concept pluralism for all x — and so it leads to theory concept pluralism.

Nevertheless, one can be a theory concept pluralist without adopting the pluralist stance. Suppose we consider subatomic physics. It does not make sense to ask for a single correct theory *simpliciter*, because we might understand 'theory' in the sense of several different THEORY concepts. Yet suppose further that we ask specifically about statement theories. It is compatible with theory concept pluralism that there would be a single best statement theory of the phenomena. That is, it is possible to be a monist about theories while being a pluralist about THEORY concepts.

As I argued in the previous section, the analogy that motivates theory concept pluralism does not generalize in a way that suggests concept pluralism for all concepts. It requires minimally that there be several concept specifications already in use, that none of them can be applied universally, and that the different concept specifications are each more profitable than others for some enquiries.[13]

Yet one might still argue that there is a path from theory concept pluralism to a more far-reaching pluralism. Suppose that theory concept pluralism is correct and each of several THEORY concepts is legitimate for certain purposes. Any particular THEORY concept could profitably be used in some case studies or analyses. As a methodological matter, when one has made good use of a specific THEORY concept, one might begin one's next enquiry by trying that same THEORY concept. If the new enquiry were similar to the prior one, it would be reasonable to expect that this would be successful. Nevertheless, it would be wrong to generalize from this to the conclusion that the THEORY concept employed is *the* correct way of thinking about theories. Any legitimate THEORY concept could support an ongoing research programme in this way. Arguments in one such programme, relying on a specific THEORY concept, could not show that science *always* or *universally* exhibits some features. They could show at most that science *sometimes* or *often* does. Similarly, such an argument could not show that features of science are *necessary* — only that the features are *possible* or in some instances *actual*. In short, theory concept pluralism would limit how much we could generalize about science.

This is a deep implication for philosophy of science, insofar as theories and the THEORY concept play a rôle. It would lead away from sweeping arguments about SCIENCE writ large and toward arguments about specific scientific domains and enquiries. In work with Craig Callender [MC04], I have drawn the distinction between wholesale arguments (which peddle conclusions about all or most of science) and retail arguments (which are directed at specific parts of science); using that locution, the upshot of theory concept pluralism is that wholesale arguments are doomed and retail arguments are the way forward.

This is akin to the pluralist stance as a methodology, but more modest

[13]Recall also that the analogy between SPECIES and THEORY is stronger even than this minimal requirement: Both species and theories serve to group instances as members (of a species) or expressions (of a theory). Cases of the most promising concept specifications require specifying an arbitrary fineness of grain.

in at least two respects. First, it only applies to arguments in which theories as such play a crucial role. Yet, Cartwright *et al.* suggest, "the 'theory-dominated' view of science" was overthrown decades ago: "Under the new regime philosophy of science could no longer be viewed as the philosophy of scientific theory" [CSS95, p. 138].

Second, even this rejection of SCIENCE writ large is compatible with the possibility that there might be a singular best theory (in a specified sense of 'theory') in some specific domain. So theory concept pluralism may be the kind of "modest pluralism" which Kellert *et al.* complain is "difficult to distinguish from a sophisticated form of monism" [KLW06a, p. xiii].

I do not mean to overstate these caveats. Even in the new regime, scientific theory is one concern of philosophy of science. Even if one does not go so far as Hull, whom I quoted at the outset suggesting that "philosophy of science deals primarily with theories and their development" [Hul97, p. 371], one must admit that philosophy of scientific theories is still a going concern. Arguments are often framed presuming a specific THEORY concept, and theory concept pluralism means that we should be wary about generalizing such arguments too far. If this modesty allows for sophisticated monism, that's fine — provided it is a monism suggested by argument rather than presumed without comment.

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