Croatian Journal of Philosophy
Vol. IX, No. 26, 2009

Do the Life Sciences Need Natural Kinds?[1]

THOMAS A.C. REYDON
Center for Philosophy and Ethics of Science (ZEWW),
University of Hannover

Natural kinds have been a constant topic in philosophy throughout its history, but many issues pertaining to natural kinds still remain unresolved. This paper considers one of these issues: the epistemic role of natural kinds in scientific investigation. I begin by clarifying what is at stake for an individual scientific field when asking whether or not the field studies a natural kind. I use an example from life science, concerning how biologists explain the similar body shapes of fish and cetaceans, to show that natural kinds play a central epistemic role in scientific explanations that cannot be delegated to other explanatory factors. A task for philosophy, then, is to come up with a theory of natural kinds that adequately accounts for the epistemic role of natural kinds in science. After having sketched the spectrum of available philosophical theories of natural kinds, I argue that none of the available theories adequately performs this task and that therefore the search is still open for a theory that does.

Key words: biology, generalization, homeostatic property clusters, induction, life science, Quine, natural kinds.

> *"At any rate, the presence or absence of natural kinds continues to be a focus of debate with respect to many areas of science, and is a question with major ramifications for how science should be understood."* (Dupré, 2000: 318).

1. *Introduction*

Natural kinds have constituted a recurrent topic in philosophy since its early beginnings. Although the technical term 'natural kind' was intro-

---

[1] Early versions of different parts of this paper have been presented at the 31st Annual Philosophy of Science Conference (Inter-University Centre, Dubrovnik, April 2005) and the 2005 Meeting of the International Society for the History, Philosophy, and Social Studies of Biology (ISHPSSB), University of Guelph, July 2005. I thank the audiences for helpful comments. In particular, I am indebted to Sören Häggqvist for helpful comments on an earlier draft.

duced in the philosophical discourse only in the 19th century (in John Venn's 1866 *The Logic of Chance*), discussions on the idea of natural kinds trace back all the way to Plato's *Phaedrus* (the *locus classicus* of the 'cutting nature at its joints' metaphor) and Aristotle's *Categories*. Notwithstanding its long history, however, the notion of 'natural kinds' remains at the center of much unfinished philosophical business. Do natural kinds exist 'out there' in the world, objectively and independently of human cognition and human classificatory interests? In what way, if at all, are natural kinds and laws of nature connected? Can—and should—natural kinds be characterized by the possession of kind essences that constitute necessary and sufficient conditions for kind membership? In the current discussions on these and related questions two ways of approaching the issue can be distinguished (cf. Häggqvist, 2005).

The traditional approach takes a 'metaphysics first' perspective: the metaphysics of natural kinds is established (or sometimes just postulated) before epistemological questions are taken into consideration. This approach to the issue of natural kinds has been the dominant one through most of the history of philosophy, which is unfortunate for various reasons. For one, it has quite likely rendered the discussion on natural kinds a 'philosophers only' domain in which one encounters preciously few scientists and in which attention is devoted to a limited number of themes—most prominently essentialism, realism, reference theory, conceptual change and concept acquisition by young children (the only part of the debate in which scientists heavily participate). The study of particular natural kinds, however, is foremost a task for science and one would thus expect that science has much to contribute to the discussions. Another reason is that to my mind the traditional approach puts the metaphysical cart before the epistemic horse by trying to force the kinds that feature in the various fields[2] of science into metaphysical schemes that often do not fit very well. Think for instance of the work by Kripke and Putnam, whose elaboration of the causal theory of reference presupposed (without much argument) an essentialist metaphysics for biological species at a time when most biologists and philosophers of biology already rejected species essentialism.

The second, much younger way of approaching the issue of natural kinds takes a more epistemology-oriented perspective. One theme that in the past decades has begun to draw attention, albeit still much less than it deserves, is the epistemic role(s) of natural kinds in scientific practice. What role(s) do natural kinds play in scientific explanation, investigation, prediction and reasoning? How crucial are these role(s) to scientific work and to science's success in achieving its aims? Do the

---

[2] Throughout this paper I use the terms 'field' and 'scientific field' in a non-strict—and perhaps somewhat hand-waving—sense to denote relatively well-demarcated, recognizable domains of investigatory work. I think that this notion of 'scientific field' will be intuitively clear and that for the purposes of the present paper there is no need to articulate a stricter notion.

individual fields of science need to incorporate natural kind concepts into their conceptual frameworks—i.e., to include natural kinds in their ontologies—because of the centrality of the role(s) natural kinds play in scientific practice, or can science as easily do without natural kinds? And if natural kinds are indeed crucial to science, then what is the nature of the natural kinds that feature in the various scientific fields? These are the core questions in the present paper.

My aim here cannot be to answer these questions in depth: this would require much more work than can be undertaken within the confines of a single paper. My aims are more modest: to begin with an exploration of the above questions and in doing so to articulate the claim that natural kinds do indeed play a central role in scientific investigation that cannot be delegated to other epistemic entities—that is, that science has a need for irreducible natural kind concepts in order to be able to function properly. If this claim is correct, philosophical theory should be able to tell us how natural kinds perform their scientific role(s). But, as I shall argue, currently available theories of natural kinds do not perform well on this task. One major challenge for contemporary philosophy of science, then, is to come up with an account of natural kinds that *is* adequate to actual science. The primary explanandum of such a philosophical account would be the epistemic function(s) of those natural kinds that actually feature in the various fields of science. This means that the quest will have to put epistemology on its to-do list before metaphysics: the metaphysics of natural kinds, important as it is, can be addressed only after the epistemology of those kinds that actually feature in science has to a considerable extent been settled.

In this paper I shall focus on the life sciences, because it is in this part of science that the epistemic role(s) of natural kinds in scientific practice as well as the problems with the available philosophical accounts of natural kinds manifest themselves particularly clearly. The structure of the paper is as follows. I begin in Section 2 by considering the epistemic role(s) of natural kinds in science, trying to substantiate the claim that this role(s) is indeed central to scientific work. In doing so I try to elucidate what is at stake *for science* in the philosophical discussions on the topic of natural kinds. Section 3 continues the discussion from Section 2 by considering a 'real life' example, pertaining to how biologists explain the streamlined body shapes of fish and cetaceans (dolphins, porpoises and whales). Although I give just one single example from just one single field of science, the example suggests that natural kinds cannot be eliminated from the ontology of a scientific field without losing some of its ability to explain and predict. As will be obvious, one of my main opponents in Sections 2 and 3 is Quine, who held that mature science could do very well without natural kinds. In Section 4 I turn to metaphysical issues, revisiting the question 'What is a natural kind?' I sketch the spectrum of currently available philosophical theories of natural kinds and assess whether the available theories deliver what they should provide, i.e., an account of how natural kinds function in science. The conclusion, regrettably, will be negative. The positive twist, from a philosopher's point of view at least, is that further work on the topic of natural kinds in science remains to be done.

2. *What is at stake for science in the natural kinds debate?*

It is a truism that generalization is a central feature of science. In every scientific field some notion of kinds is used to group the 'things' (material entities, processes, events, system states, properties, etc.) under study into groups over which generalized statements can be made. Accordingly, the recognition of kinds in particular fields of science is often made dependent on whether or not useful generalized statements can be made over the kind's members.[3] As Colin Allen and Marc Bekoff, for example, say about behavioral categories in cognitive ethology:

> Whether it is useful to recognize a category of behaviour such as play is a question of theoretical usefulness: Are there useful generalizations to be made about the behaviours if they are lumped together in this way? Our view (…) is that the study of play should be approached like the study of any other putative natural kinds. To study play, one ought to start with examples of behaviours that superficially appear to form a single category—those that would be initially agreed upon as play—and look for similarities among these examples. If similarities are found, *then* we can ask whether they provide a basis for useful generalizations. (Allen & Bekoff, 1997: 91; original italics).

This perspective on natural kinds represents a straightforward view of how kinds function in scientific investigation. One begins by postulating on the basis of observed similarities a grouping of the things under study into kinds that can be placed at the focus of investigation ('In nature a number of organisms can be found that are so similar, that we may for now suppose that they belong to the same kind *K*.'). The assumed groupings, in turn, themselves can be considered phenomena awaiting further explanation ('Why is it that all, or at least most, organisms that we have grouped together in *K* exhibit the same behavior *B*?'). As investigations proceed and the factors underlying the observed similarities are determined, these groupings may be revised and refined

---

[3] More specifically, kinds are used in science for at least two distinct purposes: classification and generalization. Classificatory practices, i.e., the ordering of the things under study in a useful storage and retrieval system, do not necessarily also have the aim of generalization in focus. Moreover, it is not necessarily the case that these two roles can be realized simultaneously. Some groupings may be useful for classificatory purposes but not for the purpose of making generalizations, for other groupings this may be the other way round and still other groupings may be useful for both purposes simultaneously. This issue has not been discussed extensively, but some discussion (with focus on biological taxa) can be found with Griffiths (1974: 87), Mayr (1982: 148–149) and Reydon (2005: 138–139). In the present paper only the role of kinds in generalization is taken into consideration; the context of classification is ignored. Any full account of natural kinds should of course also encompass the role of natural kinds in classification and presumably also in other contexts.

in order to better fit the actual state of affairs in nature. As soon as a sufficiently stable grouping is achieved, the kinds that feature in it can function to explain the properties of their individual members without direct reference to underlying factors ('Organism *O* exhibits behavior *B* because it is a *K*; we know that *K*s have a tendency to exhibit *B*.'). Most of the time, of course, no *explicit* postulates are being made regarding which grouping of the things under study is to be adopted for further work. Rather, pre-scientific groupings (taken from folk biology, folk psychology and the like) and/or groupings that are already being used successfully in other fields of science are adopted as provisional working tools that can be adjusted along the way as investigations proceed. Still, these practices can be interpreted as cases in which natural kinds are postulated, be it by means of implicit postulates.

This rough sketch of how scientific investigations proceed, however, does not imply that in the end science has any need for natural kinds. One position is that natural kinds may play an important role at early stages of the development of a field of investigation, but lose their importance as this field matures. According to this view, natural kinds merely occupy provisional positions in the ontologies of scientific fields: as science progresses and discovers the factors that underlie the similarities that obtain between the things of a particular kind, the kind itself can be bypassed, because explanations of observed phenomena can directly refer to these underlying factors without having to take the intermediate step of invoking kinds.

This view was famously advocated by Quine in his much-quoted 1969 paper on natural kinds. Quine recognized the importance of the epistemic role that kinds play in science: "(…) a sense of similarity or of kinds is fundamental to learning in the widest sense (…)", "(...) crucial to all learning, and central in particular to the process of inductive generalization and prediction which are the very life of science" (Quine, 1969: 16 & 19). Nonetheless, Quine says, "[i]n general we can take it as a very special mark of the maturity of a branch of science that it no longer needs an irreducible notion of similarity and kind." (*ibid.*: 22). According to Quine, the notions of similarity and kind that feature prominently in the earlier developmental stages of a scientific field become superfluous once the field fully matures, that is, once a theory has become available that is able to account for the existence of the kinds in question.

Quine's position is similar to the position taken earlier by Bertrand Russell. Both considered the role of kinds in induction: Quine (1969: 5) began his paper by asking "What tends to confirm an induction?", Russell sought "(...) the postulate or postulates required to make inductive probabilities approach certainty as a limit (...)" (1948: 456). Initially, Russell considered the postulate of natural kinds as a promising candidate: "If you are dealing with a property which is likely to be characteristic of a natural kind, you can generalize fairly safely after very few instances. (…) In such cases a generalization has a finite *a priori* probability, and induction is less precarious than in other problems" (*ibid.*: 336; original italics). Yet, notwithstanding the importance of this epistemic role that natural kinds play in science Russell came to the conclusion that "(...) the doctrine of natural kinds, though useful in establishing (…) pre-scientific inductions (…), is only an approximate and transitional assumption on the road towards more fundamental laws of a different kind." (*ibid.*: 462). In Russell's view, as in Quine's, once "more fundamental laws" have been discovered that can account for the existence of the hitherto postulated kinds, the kinds can be reduced to these laws and thereby are themselves rendered superfluous. While natural kinds can be admitted to scientific practice as useful and perhaps even indispensable heuristic tools for finding laws of nature and other fundamental causal factors, ultimately they have no business in a scientific field's ontology: once we have the underlying laws, we no longer need the kinds. Apparently, this rather negative perspective on the importance of natural kinds in science was widely endorsed at the time: the notions of 'natural kind' and 'kind' hardly play any role in mid-20th century accounts of the nature of science.[4]

In contrast, the past two decades or so have witnessed an increased interest in natural kinds in science. Various authors from science as well as philosophy of science have come to see the presence or absence of natural kinds in a particular field of scientific inquiry an important issue—cf. the quote from Dupré at the beginning of this paper. Thus, a number of general discussions of the notion of natural kinds in science have recently appeared (e.g., Dupré, 1993; Griffiths, 1997; Millikan, 1999; 2000; LaPorte, 2004), as well as a considerable number of papers that address the question whether a particular scientific notion can be understood as denoting a natural kind. Examples of the latter pertain to various fields of investigation; they include the notion of species (Dupré, 1993; 1999; Griffiths, 1997; 1999; Millikan, 1999; 2000), cognition (Pylyshyn, 1984), consciousness (Hardcastle, 1995), knowledge (Kornblith, 2002) and concept (Machery, 2005), as well as homologous developmental modules (Wagner, 1996; 2001; Rieppel, 2005), particular emotions, the category of emotion and other categories in psychology (Churchland, 1989: 25–27; Charland, 2002; Griffiths, 1997; 2004a; 2004b) and human kinds as studied in the social sciences (Hacking, [1986] 2002; Cooper, 2004).

A basic intuition that is held by most of these authors seems to be that the presence or absence of natural kinds in a particular scientific

---

[4] To give some examples: the notions of 'natural kind' and 'kind' do not feature in Popper's ([1934] 1959) *The Logic of Scientific Discovery*, his (1963) *Conjectures and Refutations*, Toulmin's (1953) *The Philosophy of Science*, Hempel's (1965) *Aspects of Scientific Explanation* (although 'natural kind' does get a brief mention in the 1976 postscript that was added to the German version) or his (1966) *Philosophy of Natural Science*. Nagel's (1961) *The Structure of Science* mentions 'kinds' only once, and then only in a footnote (pp. 30–31, footnote 2) in which Nagel presents a view similar to Russell's and Quine's.

field in some way determines its scientific status. Pylyshyn, in his 1984 book on the foundations of cognitive science, for example said his search for foundations to be motivated by an "(…) exciting possibility: the prospect that cognitive science is a genuine scientific domain like the domains of chemistry, biology, economics, or geology." (Pylyshyn, 1984: xi). The feasibility of this prospect, according to Pylyshyn, hinges among other things on the issue whether the phenomena that cognitive science studies can be seen as constituting one (or several) natural kinds: Pylyshyn asks whether cognitive science can be understood as the field that studies the natural kind 'cognition' or 'cognitive entity' in the same way that biology is understood as the scientific field that studies the natural kind 'life' or 'living entity'. Similar views are being expressed in more recent work. For example, in a paper that asked whether emotion could be considered as constituting a natural kind, the presence or absence of natural kinds in psychology / emotion research was directly linked to its status as a legitimate scientific field:

> There is much at stake in the question whether emotion is a natural kind (…). At the most basic level, there is the status of emotion as a field of inquiry. Should the diverse phenomena currently grouped under that rubric be united in that way (…) [o]r is the term 'emotion theory' really a misnomer (…)? (Charland, 2002: 512).

The question, then, is whether and if so, in which way, the scientific status of a field of investigation hinges on whether or not this field of work is concerned with natural kinds. Are Russell and Quine correct in saying that fully matured science has no use for kinds, or are the abovementioned more recent authors correct in suspecting that studying (a) natural kind(s) is somehow a prerequisite for a field to be 'good science'?

Without aiming for an exhaustive answer, a number of ways can already be mentioned in which the presence of natural kinds may be important to a field of science. A field that studies one or several natural kinds is

(1) *naturally delimited*, that is, concerned with a part of reality that is delimited primarily by nature and only secondarily by the scientific community and the interests of those working in it;
(2) *internally unified* to a certain extent, in the sense that it is concerned with a collection of phenomena that is sufficiently homogeneous to be accounted for by a single theory or a single theoretical framework;
(3) *progressive* in the sense that the growth of knowledge in this field can be interpreted as a growing understanding of the nature of the natural kind(s) under study as investigations proceed;
(4) *autonomous* to the extent that it possesses empirical generalizations of its own that can serve as the bases for explanations and predictions: the generalizations that can be said to be the 'own' generalizations of a field are the generalizations that range over the natural kinds in question.

A full account of the importance of natural kinds for science would require several issues to be clarified further. The first question would obviously be what exactly is meant by scientific status: is status a measure for scientific respectability, for hardness/exactitude, for reliability (e.g., the reliability of the knowledge the field produces), or for some other attribute? Only after this has been clarified is it possible to assess in which respects and to what degree each of the above four points—and presumably several others need to be added to the list—is relevant to the scientific status of a field of work. I cannot attempt here to give a full account of the importance of natural kinds for science. But for the moment it seems that at least we have good reasons to think that the presence of natural kinds adds to the status of a scientific field. Let me explore this a bit further.

While perhaps considered less important issues among philosophers of science, points (1) – (3) express some of the characteristics that many scientists who are concerned about the state of affairs in their field of work would presumably see as desirable characteristics for a field of science. In biology, for instance, much attention has been—and still is—devoted to the search for a natural system of classification, i.e., a classificatory system using groups that reflected the natural state of affairs rather than human interests (e.g., Mayr, 1982: 198 ff.; see also note 3). In a similar fashion, the absence of laws of nature in biology has led biologists to wonder what progress in biology consists in, if not in the ongoing discovery of new laws of nature. Mayr (1982: 43), for example, answered that biological progress might be seen as conceptual development—where conceptual development encompasses the development of kind concepts, that is, increase in understanding of the kinds under study. On the basis of these considerations it cannot be claimed that the presence of natural kinds is either necessary or sufficient for a field to be characterized as naturally delimited, unified, or progressive. But at least it is clear that *if* natural kinds play any role in science, their presence in a field enhances the chances of the field having these desirable characteristics.

From a philosophical point of view point (4) is more important, since it pertains to the aims of science rather than to characteristics that may or may not be considered desirable by scientists themselves. What is at stake here is not so much the position of any field of science within the whole of science in the way that for instance Mayr (1982: 32 ff.; 2004) was at pains to argue that biology is an autonomous science. What is at stake is whether a field is able to achieve the goals of explanation and prediction. (These issues are related, though: whether or not a field possesses explanatory and predictive generalizations of its own that cannot be reduced to the generalizations of 'more fundamental' fields says something about this field's position within the whole of science.) Most people would agree that a primary aim of science is to explain observed phenomena and to predict new ones (but see Quine, 1990: 128). One may then ask whether natural kinds are indispensable to achieve this aim. Not in all cases, obviously: the law of gravitation, for example, is perfectly sufficient to explain an instance of gravitational attraction between two massive objects without invoking any natural kind.[5] Indeed, it is probably true that if we have laws of nature, we don't necessarily also require natural kinds to build our explanations and predictions on. But this is precisely the problem: many, presumably most, fields of science lack the so much needed laws and do not have much perspective of obtaining them at any time in the future (the life sciences constitute a prime example). Explanations in these fields thus have to take recourse elsewhere—and their best bet is on natural kinds, or so I shall try to show in the following section.

What has been said above, while not providing a rock-bottom foundation, I think does lend plausibility to my claim that natural kinds play a central role in scientific investigation, in contradiction to the views of Russell, Quine and others. Philosophy of science, then, has as one of its tasks to come up with an account of natural kinds that does justice both to the importance of natural kinds for science and to the roles played by those natural kinds that actually feature in the various fields of science.[6] This requirement on philosophical theory, I suggest, calls for some liberalism: philosophical accounts of natural kinds should not place unduly strict conditions on the attribution of natural kind status to potential candidates and, more importantly, should not give metaphysical beliefs priority over scientific practice. I shall explore this suggestion further in Section 4, where the available accounts of the metaphysics of natural kinds are examined. But first I shall try to strengthen my case by considering an example from real life.

3. *Do the life sciences need natural kinds?*

Consider again Quine's arguments against the need for natural kinds in mature fields of science. One example that he uses concerns the (folk) biological kinds whale and fish. Since we have come to know about the occurrence of evolution and the tree-like structure of Earthly evolutionary history, Quine says, we are able to adjust the organism groupings that we use to our explanatory theory by reinterpreting organismal similarities in terms of commonness of descent:

---

[5] Not everyone would share this interpretation. Paul Churchland (1985: 12–13), for example, would probably counter that this example precisely is a case where a putative natural kind surfaces, namely the kind mass. Recognizing the kind mass, however, strikes me as not very useful. The invoked law does not say anything about individual members of the kind: the law of gravitation says nothing about any individual particle with mass, that is, one cannot explain or predict the behaviour of bodies under gravitation by *just* citing their membership in the kind 'mass' or 'material body'—one needs to specify their actual mass value.

[6] This presupposes that we are able to identify the natural kinds of a particular field without first having to take recourse to any philosophical theory of natural kinds. I shall return to this issue in Section 4.

> For a theoretical measure of the degree of similarity of two individual animals we can devise some suitable function that depends on proximity and frequency of their common ancestors. (…) When kind is construed in terms of any such similarity concept, fishes in the corrected, whale-free sense of the word qualify as a kind while fishes in the more inclusive sense do not. (Quine, 1969: 21–22).

The corrected kinds—whale on the one hand and fish in the whale-free sense on the other, or later, progressively corrected versions of these—then, are legitimized by what Quine calls the 'similarity measures' that follow from our scientific theories. But according to Quine, as kinds become fully legitimized they are rendered superfluous at the same time; in Quine's (1969: 22) usual eloquence, "(...) the animal vestige [the kind] is wholly absorbed into the theory."

This theoretical absorption works in the straightforward manner discussed at the beginning of Section 2. Remarkable similarities between organisms in nature are at first explained by postulating that they are of the same kind: organisms O_1, O_2, …, O_n are similar because they are all whales. (I am oversimplifying by talking about whales rather than elaborating my example on the level of species, but oversimplification here has the purpose of following Quine's example as closely as possible.) As theories are formulated that specify the causal factors (mechanisms, events, compositions, etc.) underlying observed similarities, we can safely bypass the kind: organisms O_1, O_2, …, O_n are similar because they all share a common ancestor. This reduces 'superficial' observed sameness (same kind) to 'deeper' natural sameness (same cause): theoretical similarity is more fundamental than kinds in the sense that theory-derived similarity explains kinds (evolutionary theory specifies the processes that in combination with the occurrence of certain historical events explain the similarities that obtain between the organisms that we count as whales), but not the other way round. There is, then, no *strict need* to retain the whale kind as part of evolutionary biology's ontology. As Quine suggests elsewhere, it is all a matter of simplicity—of consciously applying Occam's razor:

> Our acceptance of an ontology is, I think, similar in principle to our acceptance of a scientific theory (...): we adopt, at least insofar as we are reasonable, the *simplest* conceptual scheme into which the disordered fragments of raw experience can be fitted and arranged. Our ontology is determined once we have fixed upon the over-all conceptual scheme which is to accommodate science in the broadest sense (...) (Quine [1948] 1980: 16–17; my emphasis).

Those who share Quine's "taste for desert landscapes" ([1948] 1980: 4) and see ontological thrift as a virtue, are free to eliminate kinds as superfluous (but respectable!) elements of a field's ontology as theoretical similarity measures are established.

Quine, I suggest, has been too optimistic.[7] Scientific theories can achieve much, but they do not necessarily *wholly* absorb the kinds that

---

[7] Häggqvist (2005: 85–86) has criticized Quinean kind reduction by considering examples from chemistry. Here, I offer an argument why in the life sciences Quine's claim constitutes at most a mere hope without much chance of being realizable.

feature in the various fields of science. The general problem is this: while Quinean reduction of kinds works by means of mapping the kinds used in a particular scientific field on theoretical factors that account for the existence of similar things, in many cases no such mappings are feasible. The case of whales and fish can illustrate my point.

Consider the phenomenon that both cetaceans (organisms in the mammalian order *Cetacea*, i.e., whales, porpoises and dolphins) and many fish have streamlined fusiform (torpedo-shaped) bodies. When fully explaining this phenomenon a variety of explanatory factors needs to be cited, pertaining to organismal development, evolutionary history and environmental circumstances. Cetaceans are descended from a common ancestor in which a particular set of developmental pathways evolved that yield fusiform bodies. For fish, the historical situation is less clear: probably fusiform body shape has re-evolved many times, but it is not clear whether on all of these occasions the same set of developmental pathways is involved. At any rate, the fusiform body shape of cetaceans does not arise by means of the operation of the same set of developmental mechanisms that operate in fish, but is due to a different set of developmental mechanisms that newly evolved in the ancestral population of *Cetacea* after its organisms had returned from living on land to living in an aquatic environment (see for example Raff, 1996: 49; 388; 400–404). The different sets of developmental mechanisms for fusiform body shape as well as the different historical events in which these have originated, have spread through an ancestral population and have become fixated, are important factors in the explanation of body shape similarity within the order *Cetacea* and within various fish groups. They however are only parts of the whole story and do not exhaustively explain either body shape similarity within one group of aquatic animals or between different groups.

Another part of the explanation, for instance, invokes hydrodynamic principles: organisms that live in aquatic environments tend to develop streamlined body shapes as a means of reducing the pressure drag that they experience, thereby gaining the ability to maintain a stable position in flowing water and to swim faster and more efficiently.[8] Body shape similarity between, for instance, cetaceans on the one hand and one particular fish group on the other can be explained by invoking these hydrodynamic principles as an instance of convergent evolution of multiple populations that have independently adapted to life in the same environment. Any *complete* explanation of body shape similarity

---

[8] Discussions of the biological and mechanical details can be found in Webb (1988) or McGowan (1999: 197–200; 254–255). See also references in Langerhans *et al*. (2003: 695). This explanation of organismal body shape does not invoke phenotypic plasticity of the individual organism: in this case it is not individual organisms that adapt themselves to their environment as they are immersed in water, but the population that slowly evolves in the direction of a larger percentage of organisms with fusiform bodies. Note that the need for an explanation along these lines was already suggested in D'Arcy Thompson's 1942 classic *On Growth and Form*.

within one cetacean or fish group should also cite these hydrodynamic principles.

The invoked hydrodynamic principles are also important for explaining the many exceptions to the rule that exist. Not all aquatic organisms possess fusiform bodies and for those organisms that do, body shapes are never precisely the same even if they belong to the same kind (species or higher taxa). Pressure drag is an important factor for organisms that are sufficiently large and swim fast enough, but small and/or slow organisms do not have much to gain by reducing their pressure drag and hence will not exhibit an evolutionary tendency toward fusiform body shapes. In addition, even for large and fast aquatic organisms pressure drag may constitute a less important factor in comparison to other factors in their living environment. Rays and skates, for example, spend much of their time on the sea floor. In this case developmental mechanisms that yield body shapes with reduced ground contact forces have overruled mechanisms that yield reduced pressure drag. And also within the same species considerable variation may occur with respect to the actual body shapes of individual organisms and fusiform body shape can be present with some organisms and absent in other organisms, depending on their actual habitat. Langerhans *et al.* (2003), for example, have studied fish of two distantly related species, *Bryconops caudomaculatus* and *Biotodoma wavrini*, that occur in the Río Cinaruco river (Venezuela). For both species, the researchers found that the body shapes of organisms living in rapidly moving water (the river channel) were more fusiform than those of organisms living in comparatively still water (lagoons). This shows that local habitat conditions may heavily influence the actually exhibited body shape, even though presumably all organisms from the same species possess the same set of developmental mechanisms for body shape.

The task for biological theory is to explain both similarities and the exceptions to the rule. This is done by considering how the various factors at work interact, rather than considering the explanatory factors by themselves. The presence of developmental pathway P_1 in a number of organisms does not necessarily always result in the organisms' exhibiting the same trait T. P_1 might be highly susceptible to failures in its operation, or the outcome of the operation of P_1 might depend heavily on environmental factors, or the operation of P_1 might be affected or overruled by the operation of other developmental pathways, etc. In all these cases organisms will lack T even though P_1 is present. In still other cases there might be several different developmental pathways P_1, P_2, P_3, … (operating in different manners, originating in different evolutionary events, etc.) all yielding the same trait T. (In such cases T is an analogous trait, but analogous traits need explanation too.) Exceptions to the rule thus exist where the organism does not exhibit trait T even though it possesses the developmental pathway P_1 that is deemed responsible for the presence of T in most other cases, and where it exhibits T but the presence of T is not due to P_1.

Similarity explanations in the life sciences proceed in more complicated ways than Quine assumed and was sketched at the beginning of Section 2. The kinds that biologists begin their investigations with, that they adjust along the way and that they end up with at the end of the day do not generally correspond in a 1–1 manner to the causes of observed similarities (developmental mechanisms, particular environments, historical events, etc.) and therefore cannot be reduced to these. Rather, they tend to lie at the intersections of various factors that feature in similarity explanations, some of which can count as theoretical similarity measures in Quine's sense whereas others cannot.

From a Quinean perspective, the kind *Cetacea* would for instance be a good candidate for reduction (at least as far as the explanation discussed above is concerned). *Cetacea* is a prima facie similarity group for which we now seem to have a good theoretical similarity measure that explains the similar body shapes of cetaceans while by-passing the kind: possession of the same set of developmental pathways due to descent from the same ancestral population in which these first originated. Possession of the same set of developmental pathways is however no guarantee for actual similarity. Moreover, commonness of descent does not even guarantee the presence of the same set of developmental mechanisms: due to changing environments, for instance, later descendants may lose the developmental pathways that cause fusiform body shapes in cetaceans. The epistemic role of *Cetacea* and other taxonomic kinds in similarity explanations, then, is not to provide measures of similarity but *measures of relatedness* of organisms that enter into similarity explanations only indirectly. While it is true that relatedness often is an indispensable part of explanations of organismal similarity, it never *directly* explains organismal similarities and therefore cannot count as a similarity measure. The directly explanatory factors in similarity explanations like the above are organismal developmental mechanisms (on the organism level) and environmental factors (on the population level). Generalizations that range over taxonomic kinds, i.e., generalizations regarding the relatedness of organisms, at most explain why two organisms that are actually found to possess the same developmental mechanism do so.[9] Taxonomic kinds play an indispensable epistemic role as the building blocks of reconstructed evolutionary history and as such as units that support historical generalizations pertaining to organismal relatedness. As such, they are irreducible biological generalizations—they are the generalizations meant in Section 2 under point (4).

The moral of this section is that there are indispensable explanatory needs for kinds in the life sciences. Although I have given just one example, I think the example has sufficient characteristics that are generally found in explanations of organismal traits, behaviours, etc. to

---

[9] Compare the recent suggestion by Waters (1998), who points to the importance in biology of what he calls distributions: generalizations about how biological properties are distributed throughout the living world.

be able to make the more general (but at this point still insufficiently defended) claim that natural kinds play an ineliminable role in organismal similarity explanations in the life sciences.[10] More work would need to be done to further make my case, but I shall have to leave this for elsewhere.

4. *Again, what are natural kinds?*

Judging by the vast differences that available textbook definitions of the term 'natural kind' and encyclopaedia articles on the topic exhibit, philosophy still is very far from a standard answer to the question 'What are natural kinds?' Still, some ideas on natural kinds are widely endorsed: groupings of things into natural kinds do not depend on human interests but refer to some objective feature of nature (cf. point (1) above); natural kinds feature in laws of nature, or, at least there is some intimate connection between natural kinds and laws of nature; natural kinds play an important role in scientific investigation as tools for organizing the things under study (i.e., natural kinds function as units of classification—cf. note 3); and natural kinds support inductions (natural kinds function as units of generalization; point (4) above). It is doubtful whether all these ideas—and perhaps there are several others that belong in this list—can simultaneously be upheld; presumably some will have to be abandoned. The general question is how natural kinds are to be understood if they are maximally to live up to the expectations we have of them. I place emphasis on the role of natural kinds as units of generalization and thus see the function of those kinds that actually feature in generalizations in the various fields of science as the primary explanandum of a philosophical theory of natural kinds.[11] Philosophical theory should provide a framework that (among other things) tells us what science is like with respect to the kinds that feature in it, but without forcing the kinds that feature in science into any a priori metaphysical (or other) straightjacket.

A complicating factor is that kinds in science are not unequivocally pre-given: kinds are the 'workmanship of men' as much as they are the workmanship of nature.[12] Natural kinds are the workmanship of men,

---

[10] I emphasize again that in this paper 'natural kind' is to be read in a broad sense as 'unit of explanatory generalization'. Of course, species are not natural kinds in the traditional sense (see Section 4) and if higher taxa are not real (as many authors hold), these are not natural kinds in any strict sense either. It is a matter of terminology that I do not want to address here whether one wants to use 'scientific kind', 'relevant kind' (Goodman, 1975: 63) or 'natural kind' interchangeably, or reserves 'natural kind' for some special subgroup of scientific kinds.

[11] Cf. Boyd (2000: 66, original emphasis): "(…) *the* task of the philosophical theory of natural kinds is to explain how classificatory practices contribute to reliable inferences (…)".

[12] The metaphysical position that I (provisionally) adopt here attempts to navigate in between the two extremes of the nominalist view that kind are entirely of our own making and the realist view that kinds are wholly given by nature. This is in

since scientists are the actors who group the things under study into putative kinds and do so with particular purposes in mind. The explanatory force of natural kinds, however, derives from the workmanship of nature: what science seeks are the factors in nature (in a broad sense, including mechanisms, historical events, etc.) that underwrite explanatory generalizations. In order to be able to specify how kinds are able to function in explanatory generalizations, a philosophical theory of natural kinds should provide criteria to distinguish the contribution of men from the contribution of nature. While it is a task of science to uncover the individual factors that underwrite its explanatory generalizations, it is a task of philosophy to connect these factors to practices in science of classification, explanation, induction, etc. Any philosophical theory of natural kinds, then, should give at least some guidance to the individuation of natural kinds: what sorts of factors can make a kind a natural kind in the sense of the present discussion and can unite particular things into one natural kind?

So, do the available accounts deliver what they should minimally provide?

Even though it may well be the case, as Hull ([1997] 2001: 214) suggested, that "[o]f course, no two philosophers meant the same thing by 'kind' or 'natural kind' and their cognates in other languages", the actual number of competing philosophical accounts of natural kinds is quite limited. Three main currents of thought on natural kinds can be distinguished: nominalism, essentialism and projectibilism.[13] Most accounts that have been advanced by individual authors can be seen as variations on one of these three themes. In what follows, I shall not consider purely nominalist accounts (cf. note 12) but only include accounts that presuppose a minimal amount of objectivism, that is, presuppose that natural kinds minimally represent some features of the world 'out there' that science can study. The remaining two types of account can be seen as the not sharply delimited poles of a continuous spectrum, in between which various intermediate positions are possible (see Table 1). Although they represent profoundly different approaches to the problem of natural kinds, I do not see essentialism and projectibilism as constituting a dichotomy between two diametrically opposed camps. The difference between these two types of account is mainly one of emphasis and perspective: while essentialists give primacy to the metaphysics of natural kinds and projectibilists to their epistemology, there is no reason in principle why two investigators starting out from the two extremes could not end up at the same intermediate position.[14]

---

line with Boyd's (2000: 66) thesis of 'bicameralism'. Interestingly, Goodman seems to suggest a similar view when he writes that "(…) worlds are as much made as found (…)" (1975: 72). Further elaboration of my metaphysics of natural kinds is to be left for future work.

[13] I have taken the term 'projectibilism' from Häggqvist (2005), who has provided a clear discussion of the differences between essentialist and projectibilist positions.

[14] Häggqvist (2005: 77–78) rightly suggests that there is a fundamental tension between essentialism and projectibilism, because the latter is able to account for many

explanatory strength	philosophical view	explanandum	explanans	natural kinds are	character
strong					limitative
↕	traditional essentialism	observable sameness	shared property essence	the furniture of the world	↕
	strict HPC-theory	observable similarity	causal mechanism	the bases for generalizations	
	liberal HPC-theory	observable similarity	any natural factor	the bases for generalizations	
	bare projectibilism	observable similarity	similarity remains unexplained	representations of generalizations	
weak					liberal

Table 1. The spectrum of available views of natural kinds.

Much has been written on the essentialist tradition, so my discussion will be brief. The central idea, to be found in the classic literature on natural kinds as well as with contemporary representatives of the essentialist tradition (among the most prominent are Wilkerson, 1988; 1995: 30–33; Ellis, 2001: 21; 2002: 26–27), is that every natural kind is associated with its own particular kind essence that specifies both the nature of the kind's members and the necessary and sufficient conditions for an individual thing to be counted as a member of the kind. Usually, these kind essences are understood in terms of intrinsic properties of the kind's members. Ever since Locke's 1689 *Essay*, micro*structure* is the usual suspect for playing the role of kind essence, but identifying kind essences with the microstructures of kind members turns out to be deeply problematic—a more promising candidate is micro*composition*.[15] To use a time-worn example, the kind essence of gold is commonly understood to consist in microcomposition: all and only those atoms that have 79 protons in their microcomposition are members of the kind gold. The microcomposition of gold atoms is the primary explanatory factor in explanations of the properties and behavioral dispositions of gold atoms. Factors other than microstructure or microcomposition can however also count as kind essences. Some of Putnam's writings, for

kind predicates that the former is not, and that this tension has been underrated by most authors. It should neither be overrated, though, for it is not a tension in principle: there is nothing in the overall positions of essentialism and projectibilism that makes the two intrinsically incompatible (incompatibilities arise from the ways in which various authors have concretely filled in their positions). The tension that Häggqvist points to is a tension in practice: there just happen to be many kind predicates that are susceptible to projectibilism but not to essentialism. (As will be illustrated below, I use 'essentialism' in the traditional sense, denoting the position that there are necessary and sufficient conditions for kind membership.)

[15] For a brief discussion involving chemical kinds, see Häggqvist (2005: 72–73). Häggqvist also pointed to an important distinction between microstructuralism and microessentialism.

example, suggest sameness of law-governed behaviour as the candidate for kind essences rather than sameness of microcomposition (cf. Häggqvist, 2005: 73). Process essentialists in biology propose to identify generative processes underlying organismal morphology as kind essences for biological kinds (e.g., Webster & Goodwin, 1996: 75–80). Yet another suggestion is that for some natural kinds a common environment, rather than a set of intrinsic properties, should be identified as the kind essence (Elder, 1995; although they are not essentialists in the traditional sense themselves, Griffiths (1997: 190–191) and Millikan (1999: 55; 2000: 20) have made similar suggestions). But whatever sort of factor is identified as kind essence, the core idea remains that having this essence (possessing the proper microcomposition, instantiating the right generative process, being subject to the right law(s), etc.) is a necessary and sufficient condition for membership of the kind in question. On this view, science progresses as it uncovers the kind essences that underlie the various natural kinds found in nature and, in doing so, uncovers the natures of the various kinds of things that can be found out there in the world.[16]

The essentialist view of natural kinds has a limitative streak in the sense that it only counts candidates that meet the strict criteria that follow from metaphysical considerations as eligible to be attributed the honorific status of natural kind. First the nature of natural kinds is established before candidate kinds can be assessed in how far they meet the adopted criteria. If a candidate fails to meet these criteria, as in the case of species, the conclusion must be that it is not a natural kind. Often no or only very few candidates can live up to the strict requirements that are imposed for the attribution of natural kind status and conclusion is that by far the majority of scientific fields do not study natural kinds.

The case of species in biology is perhaps the best illustration of essentialism's limitative streak. Species have long been counted among the prototypical examples of natural kinds, along with the chemical elements and compounds and the kinds of elementary particles that constitute the subject matter of particle physics. (And today, several authors still list species among the prototypical natural kinds.) However, while the philosophical tradition seems to function well for these other prototypical cases, in the case of species some severe problems were encountered.[17] First, there is the link between natural kinds and laws of nature. On the traditional view natural kinds are the subjects of laws of

---

[16] See for instance the discussions in LaPorte (2004) on the progressive discovery of kind essences in biology and chemistry.

[17] What I sketch here may count as a crude form of the received view among historians and philosophers of science. Several authors have questioned this 'received' view on various points. Some have expressed doubts whether essentialism works even for chemical elements and compounds. Others have suggested that commonly endorsed pre-Darwinian views of species and higher taxa were in fact not at all essentialist. At this point, I do not wish to enter into these discussions but follow the received view for whatever it is worth.

nature, but no laws have been found for any particular species. (While in this line of thought the lack of laws is taken to imply a lack of kinds, I would suggest that a lack of laws is better taken as implying a need for kinds—recall the preceding sections.) Second, essentialism with respect to species was found untenable: while organisms within the same species often exhibit considerable variation in both morphological and genetic properties, remarkable morphological and genetic similarities are also often found between organisms from different species. Moreover, the very idea that a species of organisms can be characterized by essential properties that all and only the members of the species possess is in stark conflict with the necessity of intra-species genetic variation as a prerequisite for the occurrence of Darwinian evolution.

The case of species instantiates a conflict between on the one hand a historically deep-rooted philosophical theory, natural kind essentialism, and on the other hand the current state of affairs in science. As David Hull pointed out, such conflicts are resolvable by adjusting either of the two opponents:

> Two alternatives are open at this point. The first is to deny that species are natural kinds. After all, they have none of the characteristics traditionally used to define "natural kind". The second alternative is to claim that the notion of natural kind has itself evolved. (…) However, as far as I can tell, the second alternative has not occurred. (…) This leaves the first alternative. (Hull, 1988: 501–502; cf. [1997] 2001: 216).

Hull's first alternative entails an unduly limitative view of natural kinds that does not agree well with the view of those authors mentioned in Section 2, who hold that there is much at stake for a field of science whether or not this field is concerned with natural kinds. And it does not agree well with the state of affairs in biological science. The species of organisms that biologists study do perform the epistemic role of kinds in the sense that they serve as groups of organisms over which explanatory and predictive generalizations can be made. Biologists commonly generalize the findings from the study of a limited number of organisms of one species to all of the organisms that belong to this species and Hull's first alternative fails to account for this role of species. If the status of a scientific field is at least for some part dependent on whether or not this field can be said to study natural kinds, a less limitative and more liberal perspective of natural kinds is called for.

The principal alternative to essentialist views of natural kinds is Richard Boyd's homeostatic property cluster theory (henceforth HPC-theory). HPC-theory is the main representative of a comparatively recent (tracing back to the mid-20th century) change in perspective on the problem of natural kinds. While in the old tradition the core question was one of ontology—What sorts of things constitute the furniture of the world?—the problem of natural kinds has itself gradually changed into one of epistemology: What sorts of support do we have for inductions? Nelson Goodman's 'new riddle of induction' is usually given a pivotal role in this turn of attention towards induction, but of course there were several earlier authors who saw questions about kinds as questions about induction (John Venn in 1866, for example). “Induction”, Goodman (1975: 63) says, “requires taking some classes to the exclusion of others as relevant kinds. (…) The uniformity of nature we marvel at or the unreliability we protest belongs to a world of our own making.” The question then is what makes the groupings of things, that we ourselves make and use in inductive generalizations, suitable to be used as such: what, apart from our own interests, determines which classes are relevant for induction? This worry demands an approach to the issue of natural kinds not from a ‘metaphysics first’ perspective (as was the case in the essentialist tradition), but from considerations that begin by regarding the epistemic uses of kinds. The explanandum is not in the first place what natural kinds *are*, but how they *function*. Since this ‘inductive turn’, the central issue in the discussion on natural kinds is whether natural kinds are at all required to support inductions—recall that this was also the question that Russell (1948) and Quine (1969) considered and answered negatively—and if so, how they then are to be conceptualized.

Since the time Hull’s above quotation appeared in print, exploration of his second alternative has commenced, most importantly by the development of HPC-theory. Since its presentation in a series of papers from the late 1980s onward (Boyd, 1988; 1989; 1991; 1999a; 1999b; 2000; Keller *et al.*, 2003), HPC-theory has widely gained popularity among philosophers and scientists alike. Very briefly, the idea is this.[18] Natural kinds are conceived as just any collections of things over which useful and well-founded generalizations can be made. As Griffiths (2004b: 903) recently put it, natural kinds are “(…) categories which admit of reliable extrapolation from samples of the category to the category as a whole. In other words, natural kinds are categories about which we can make inductive scientific discoveries.” What underlies this possibility, according to Boyd, is the repeated co-occurrence of a collection of properties—Boyd speaks of “(…) a sort of homeostasis”—either due to the fact that the presence of some of the co-occurring properties tends to favour the presence of others, or to underlying mechanisms that cause property co-occurrence, or due to both (Boyd, 1999b: 143). Natural kinds then are defined not by essences in the traditional sense as classes of identical things (identical in the sense that all members of the kind have a certain set of properties in common, the possession of which is separately necessary and jointly sufficient for kind membership), but by (1) the family F of properties that is found to repeatedly co-occur in nature in combination with (2) the causal mechanism(s) that underlie the repeated co-occurrence of the properties in F (Boyd, 1989: 16; 1999b: 143). This way of defining kinds is less strict than the way in which traditional essentialism defines kinds: one or more properties in F may be missing or one or more of the underlying mechanisms may fail to oper-

---

[18] Clear and detailed accounts of HPC-theory are available from Boyd (1989; 1999b) and Griffiths (1997: Chapter 7).

ate on a particular entity while the entity is still counted as a member of the kind defined by *F* and the corresponding causal mechanisms.

HPC-theory constitutes a framework for understanding natural kinds that possesses several clear advantages over the essentialist tradition. It fits the actual state of affairs in science better by replacing the search for essences in the traditional sense with the search for the natural factors that underlie observed similarities. In doing so, it also allows more flexibility with respect to future developments in science. HPC-theory does not attempt to force the kinds that feature in the various scientific fields into a metaphysical straightjacket (at least not on a sufficiently liberal reading, see below). And it contains the promise of a unified philosophical account of natural kinds that applies to all scientific fields rather than just to those elite fields that fit the essentialist framework.

Exactly how well HPC-theory fits actual science and whether it will be able to fulfil its promise still remains to be seen, though. One problem is that HPC-theory fails with respect to the individuation of natural kinds. Ereshefsky & Matthen (2005: 18–19) have for example recently pointed out that HPC-theory fails to pick out many of the taxa that biologists in fact use, because it does not take the importance of evolutionary historical factors sufficiently into account. In the example of Section 3, HPC-theory would fail to individuate the kind *Cetacea* because this is a kind that represents relatedness based on history, not similarity based on mechanisms. This, I think, is due to a bigger problem with HPC-theory, namely that there is not much reason to presuppose that for every scientifically useful kind there exists an underlying causal mechanism. In a recent paper, Häggqvist (2005) objected that by requiring kinds to be supported by homeostatic mechanisms, HPC-theory is still too limitative. “It is not at all clear,” Häggqvist (2005: 80) writes, “why the lack of such mechanisms should impair the soundness of a kind.” Häggqvist’s point is well taken: *if* a causal homeostatic mechanism is present, it will be sufficient to ground a natural kind; but surely there is no reason to assume that for every natural kind there must be a causal mechanism. The example in Section 3 has shown this point for the life sciences.

Whether Häggqvist’s objection holds against Boyd’s HPC-theory, however, depends on the strictness with which the theory is read. Any theory that a priori requires natural kinds to be supported by causal mechanisms is indeed too limitative. But Boyd himself has repeatedly hinted that the term ‘homeostasis’ is to be taken metaphorically, although in some cases a literal reading may apply (Boyd, 1988: 197; 1989: 16; 1999b: 143). The implications of this suggestion are not clear. In particular, it is not clear how the metaphorical reading of ‘homeostasis’ reflects on the reading of ‘causal homeostatic mechanism’: is ‘mechanism’ also to be read metaphorically on many occasions? Some authors have on some occasions implicitly suggested a watered-down version of HPC-theory that does not require causal mechanisms for natural kinds but allows just any natural factor that underwrites reliable generalizations. Thus, Millikan (2000: 23, my italics) writes: “(…) a kind is real only if there is *some* univocal principle, the very same principle throughout, that explains for each pair of members why they are alike in a number of respects." (But by only considering similarity, Millikan still expresses a somewhat too limitative view.) At present, however, it remains unclear how far HPC-theory can be watered down without losing too much of its explanatory force: watering down, after all, bears the risk of ending up with a theory of homeopathic concentrations.

Häggqvist's own view of natural kinds, 'bare projectibilism',[19] brings us even further away from strict HPC-theory. The central idea is that we can—and should—accept the existence of a natural kind even if we do not have any explanation for its existence. Demanding any particular way of explaining natural kinds, Häggqvist seems to suggest, is demanding too much. Going from the essentialist end of the spectrum towards the other end, we get closer and closer to Goodman's idea of kinds as the extensions of projectible predicates while getting further removed from the metaphysical straightjacket of essentialism and other such straightjackets. At the natural end of this trajectory, lies a view of natural kinds that is entirely stripped from its metaphysical import, i.e., projectibilism in its barest form. Bare projectibilism thus represents a natural end point for philosophical theorizing about natural kinds.

I am very sympathetic to the liberalism in Häggqvist's original position of bare projectibilism. Nevertheless, I think that a much less radical—that is, less bare—version of projectibilism is preferable to the original version. Bare projectibilism in its radical form is a tenable position with respect to natural kinds, but it is not a very fruitful position. To my mind, it just does too little work. If we abstain from explaining kinds when characterizing them, as Häggqvist suggests, we fail to perform a core task of a philosophical theory of kinds, namely, to tell us why some kinds can and do play (an) important epistemic role(s) and others cannot. To be sure, we can accept the legitimacy of kinds that are projectible even though we do not know why they are projectible (Häggqvist, 2005: 81). But as surely, such acceptance is provisional: kinds that remain unexplained will ultimately be rejected. Where HPC-theory is too strict when it comes to the individuation of kinds, bare projectibilism is not strict enough.

What is needed, then, is a theory of natural kinds that retains the liberalism from Häggqvist's bare projectibilism while at the same time being able to do substantial philosophical work—that is, a middle road between HPC-theory in a strict reading and bare projectibilism. It is important to keep an open mind and accept that in principle any sort of natural factor could underwrite explanatorily important generalizations. But it is as important to remain critical and to accept only those sorts of factors for which a role in explanatory generalizations can be explicitly accounted for.

---

[19] Häggqvist (personal communications) now has moved away somewhat from his original version of bare projectibilism. We seem to agree that perhaps the best approach to take lies somewhere on the spectrum between strict HPC-theory and the extreme version of bare projectibilism.

5. *Summary and outlook*

I have tried to give some plausibility to my claim that the life sciences need natural kinds, on the understanding of natural kinds as kinds over which well-supported explanatory and predictive generalizations can be made. If this claim—and the more general (but as yet undefended) claim that science has a general need for natural kinds as the grounds for explanatory and predictive generalizations—is correct, the challenge for philosophy is to elaborate a theory of natural kinds that both is adequate to the current state of affairs in all domains of science and is flexible enough to accommodate future changes in this state of affairs. There is a wide spectrum of philosophical accounts of natural kinds, but none of the available accounts meet the requirements, or so I have argued. In particular, while some theories (traditional essentialism and strict HPC-theory) represent a too limitative view of natural kinds, others (bare projectibilism) are too liberal. The challenge for philosophy is to find a fruitful moderate liberalism with respect to natural kinds in science—and this challenge remains open.

References

Allen, C. & Bekoff, M. (1997): *Species of Mind: The Philosophy and Biology of Cognitive Ethology*, Cambridge (Mass.): MIT Press.

Boyd, R.N. (1988): 'How to be a moral realist', in: Sayre-McCord, G. (Ed.): *Essays on Moral Realism*, Ithaca (N.Y.) and London: Cornell University Press, pp. 181–228.

Boyd, R.N. (1989): 'What realism implies and what it does not', *Dialectica* 43: 5–29.

Boyd, R.N. (1991): 'Realism, anti-foundationalism and the enthusiasm for natural kinds', *Philosophical Studies* 61: 127–148.

Boyd, R.N. (1999a): 'Kinds, complexity and multiple realization', *Philosophical Studies* 95: 67–98.

Boyd, R.N. (1999b): 'Homeostasis, species, and higher taxa', in: Wilson, R.A. (Ed.): *Species: New Interdisciplinary Essays*, Cambridge (Mass.): MIT Press, pp. 141–185.

Boyd, R.N. (2000): 'Kinds as the "workmanship of men": realism, constructivism, and natural kinds', in: Nida-Rümelin, J. (Ed.): *Rationalität, Realismus, Revision: Vorträge des 3. internationalen Kongresses der Gesellschaft für Analytische Philosophie*, Berlin: Walter de Gruyter, pp. 52–89.

Charland, L.C. (2002): 'The natural kind status of emotion', *British Journal for the Philosophy of Science* 53: 511–537.

Churchland, P.M. (1985): 'Conceptual progress and word/world relations: in search of the essence of natural kinds', *Canadian Journal of Philosophy* 15: 1–17.

Churchland, P.M. (1989): *A Neurocomputational Perspective: The Nature of Mind and the Structure of Science*, Cambridge (Mass.): MIT Press.

Cooper, R. (2004): 'Why Hacking is wrong about human kinds', *British Journal for the Philosophy of Science* 55: 73–85.

Dupré, J.A. (1993): *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*, Cambridge (Mass.): Harvard University Press.

Dupré, J.A. (1999): 'On the impossibility of a monistic account of species', in: Wilson, R.A. (Ed.): *Species: New Interdisciplinary Essays*, Cambridge (Mass.): MIT Press, pp. 3–22.

Dupré, J.A. (2000): 'Natural kinds', in: Newton-Smith, W.H. (Ed.): *A Companion to the Philosophy of Science*, Oxford: Blackwell, pp. 311–319.

Elder, C.L. (1995): 'A different kind of natural kind', *Australasian Journal of Philosophy* 73: 516–513.

Ellis, B. (2001): *Scientific Essentialism*, Cambridge: Cambridge University Press.

Ellis, B. (2002): *The Philosophy of Nature: A Guide to the New Essentialism*, Montreal & Kingston: McGill-Queen's University Press.

Ereshefsky, M. & Matthen, M. (2005): 'Taxonomy, polymorphism, and history: an introduction to population structure theory', *Philosophy of Science* 72: 1–21.

Goodman, N. (1975): 'Words, works, worlds', *Erkenntnis* 9: 57–73.

Griffiths, G.C.D. (1974): 'On the foundations of biological systematics', *Acta Biotheoretica* 23: 85–131.

Griffiths, P.E. (1997): *What Emotions Really Are: The Problem of Psychological Categories*, Chicago: University of Chicago Press.

Griffiths, P.E. (1999): 'Squaring the circle: natural kinds with historical essences', in: Wilson, R.A. (Ed.): *Species: New Interdisciplinary Essays*, Cambridge (Mass.): MIT Press, pp. 209–228.

Griffiths, P.E. (2004a): 'Is emotion a natural kind?', in: Solomon, R.C. (Ed.): *Thinking About Feeling: Contemporary Philosophers on Emotions*, New York: Oxford University Press, pp. 233–249.

Griffiths, P.E. (2004b): 'Emotions as natural and normative kinds', *Philosophy of Science* 71: 901–911.

Hacking, I. ([1986] 2002): 'Making up people', in: Hacking, I.: *Historical Ontology*, Cambridge (Mass.): Harvard University Press, pp. 99–114.

Häggqvist, S. (2005): 'Kinds, projectibility and explanation', *Croatian Journal of Philosophy* 5: 71–87.

Hardcastle, V.G. (1995): *Locating Consciousness*, Amsterdam & Philadelphia: John Benjamins.

Hempel, C.G. (1965): *Aspects of Scientific Explanation, and Other Essays in the Philosophy of Science*, New York: The Free Press.

Hempel, C.G. (1966): *Philosophy of Natural Science*, Englewood Cliffs (N.J.): Prentice-Hall.

Hull, D.L. (1988): *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science*, Chicago & London: University of Chicago Press.

Hull, D.L. ([1997] 2001): 'That just don't sound right: a plea for real examples', reprinted in Hull, D.L.: *Science and Selection: Essays on Biological Evolution and the Philosophy of Science*, Cambridge: Cambridge University Press, pp. 196–221.

Keller, R.A., Boyd, R.N. & Wheeler, Q.D. (2003): 'The illogical basis of phylogenetic nomenclature', *Botanical Review* 69: 93–110.

Kornblith, H. (2002): *Knowledge and Its Place in Nature*, Oxford: Clarendon Press.

Langerhans, R.B., Layman, C.A., Langerhans, A.K. & Dewitt, T.J. (2003): 'Habitat-associated morphological divergence in two Neotropical fish species', *Biological Journal of the Linnean Society* 80: 689–698.

LaPorte, J. (2004): *Natural Kinds and Conceptual Change*, Cambridge: Cambridge University Press.

Machery, E. (2005): 'Concepts are not a natural kind', *Philosophy of Science*, in press.

Mayr, E. (1982): *The Growth of Biological Thought: Diversity, Evolution, and Inheritance*, Cambridge (Mass.): Harvard University Press.

Mayr, E. (2004): *What Makes Biology Unique? Considerations on the Autonomy of a Scientific Discipline*, Cambridge: Cambridge University Press.

McGowan, C. (1999): *A Practical Guide to Vertebrate Mechanics*, Cambridge: Cambridge University Press.

Millikan, R.G. (1999): 'Historical kinds and the "special sciences"', *Philosophical Studies* 95: 45–65.

Millikan, R.G. (2000): *On Clear and Confused Ideas: An Essay About Substrate Concepts*, Cambridge: Cambridge University Press.

Nagel, E. (1961): *The Structure of Science: Problems in the Logic of Scientific Explanation*, London: Routledge & Kegan Paul.

Popper, K.R. ([1934] 1959): *The Logic of Scientific Discovery*, London: Hutchinson.

Popper, K.R. (1963): *Conjectures and Refutations: The Growth of Scientific Knowledge*, London: Routledge & Kegan Paul.

Pylyshyn, Z.W. (1984): *Computation and Cognition: Toward a Foundation for Cognitive Science*, Cambridge (Mass.): MIT Press.

Quine, W.V. ([1948] 1980): 'On what there is', in Quine, W.V.: *From a Logical Point of View: Nine Logico-Philosophical Essays, second edition (revised)*, Cambridge (Mass.): Harvard University Press, pp. 1–19.

Quine, W.V. (1969): 'Natural kinds', in: Rescher, N. (Ed.): *Essays in Honor of Carl. G. Hempel*, Dordrecht: Reidel, pp. 5–23.

Quine, W.V. (1990): 'Comment on Haack', in: Barrett, R.B. & Gibson, R.F. (Eds): *Perspectives on Quine*, Oxford: Basil Blackwell, p. 128.

Raff, R.A. (1996): *The Shape of Life: Genes, Development, and the Evolution of Animal Form*, Chicago & London: University of Chicago Press.

Reydon, T.A.C. (2005): 'On the nature of the species problem and the four meanings of 'species'', *Studies in History and Philosophy of Biological and Biomedical Sciences* 36: 135–158.

Rieppel, O. (2005): 'Modules, kinds, and homology', *Journal of Experimental Zoology B (Molecular and Developmental Evolution)* 304: 18–27.

Russell, B. (1948): *Human Knowledge: Its Scope and Limits*, London: George Allen & Unwin.

Toulmin, S. (1953): *The Philosophy of Science: An Introduction*, London: Hutchinson.

Wagner, G.P. (1996): 'Homologues, natural kinds and the evolution of modularity', *American Zoologist* 36: 36–43.

Wagner, G.P. (2001): 'Characters, units and natural kinds: an introduction', in: Wagner, G.P. (Ed.): *The Character Concept in Evolutionary Biology*, San Diego: Academic Press, pp. 1–11.

Waters, C.K. (1998): 'Causal regularities in the biological world of contingent distributions', *Biology and Philosophy* 13: 5–36.

Webb, P.W. (1988): 'Simple physical principles and vertebrate aquatic locomotion', *American Zoologist* 28: 709–725.

Webster, G. & Goodwin, B.C. (1996): *Form and Transformation: Generative and Relational Principles in Biology*, Cambridge: Cambridge University Press.

Wilkerson, T.E. (1988): 'Natural kinds', *Philosophy* 63: 29–42.

Wilkerson, T.E. (1995): *Natural Kinds*, Aldershot: Avebury.