Chapter 6
Naturalized Epistemology and the Genealogy of Knowledge

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6.1 Introduction

Naturalized epistemology aims to study knowledge as a natural phenomenon, or more precisely, as a *natural kind*. Some influential advocates of this view insist that such investigation must rely first and foremost on the methods and results of the natural sciences. In so doing these authors reject other forms of epistemology that focus on the *norms* governing the use of the *concept* of knowledge. This paper is an attempt to clarify the difference between these two positions and to narrow the gap between them.

My central paradigm of a theory that focuses on the concept of knowledge is Edward Craig's research programme as set out in his two books, *Knowledge and the State of Nature* (1990) and *Was wir wissen können* (1993). Several authors have tried to use and develop this programme over the past decade (Beebe 2012; Fricker 2007, 2010; Gelfert 2011; Kappel 2010; Kelp 2011; Kusch 2009, 2011; Neta 2006; Pritchard 2009; Williams 2002). Craig himself calls his approach "conceptual synthesis" or "practical explication" (1990). I prefer the label "genealogy of knowledge" that was first suggested by Bernard Williams (2002) since it better captures the *historical nature* of the approach.

In this essay I shall attempt to defend and reinterpret Craig's project in response to criticism put forward by Hilary Kornblith in his recent paper "Why Should We Care about the Concept of Knowledge?" (2011). Kornblith is the most prolific and insightful defender of "naturalized epistemology" in contemporary philosophy (1993, 2002). His criticism of Craig thus provides a welcome opportunity to explore the relationship between naturalized epistemology and the genealogy of knowledge.

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I want to make plausible four theses. First, Kornblith's interpretation of Craig is mistaken: Craig is not committed to denying that knowledge is a natural kind. Second, Kornblith overlooks that Craig's project has affinities with naturalized epistemology. Third, Kornblith's use of Richard Boyd's theory of natural kinds is puzzling: Boyd advocates "promiscuous realism", Kornblith rejects it. And fourth, Craig's genealogy helps to understand unity and disunity in both concepts and natural kinds of knowledge.

6.2 Kornblith's Criticism of Craig

I begin with a summary of Kornblith's criticism of Craig's genealogy. Kornblith interprets Craig as maintaining that knowledge is not a *natural kind* like water or aluminium, but an *artificial kind* like table or monarchy (2011: 43–44). Kornblith bases this reading on one central but short passage in Craig (1990):

> Couldn't it be that knowledge, like water, is important stuff, and that the purpose of the concept is simply to enable us to think and talk about it? … I am fairly confident that this is mistaken. Knowledge is not a given phenomenon, but something that we delineate by operating with a concept which we create in answer to certain needs, or in pursuit of certain ideals. The concept of water, on the other hand, is determined by the nature of water itself and our experience of it. (1990: 3)

Kornblith's own views on knowledge—in so far as they are relevant here—directly contradict the position he ascribes to Craig. As Kornblith sees it, knowledge is a natural kind since the "category" of knowledge plays a significant explanatory and predictive role in one particular natural science: *cognitive ethology* (the science of animal behaviour). In fact, Kornblith believes that we should let cognitive ethology tell us what knowledge really is. He also insists that "… the kind of knowledge that philosophers have talked about all along is just the kind of knowledge that cognitive ethologists are … studying" (2002: 30). Moreover, just like chemists rightly ignore folk concepts of water or aluminium, so also epistemologists should pay little attention to folk concepts of knowledge or justification: "… our concepts of knowledge and justification are of no epistemological interest" (2006: 12). Our folk concepts and intuitions concerning knowledge and epistemic justification do not tell us what knowledge and justification really are. Here too the investigation must focus on the 'stuff' itself. Kornblith's conception of natural kinds is that of his teacher Richard Boyd for whom natural kinds are homeostatic clusters of properties (Kornblith 2002: 61, cf. 2007a, b, 2011; cf. Boyd 1980, 1983, 1988, 1991, 1999). Applied to the concepts of knowledge this conception suggests the following formulation:

> The knowledge that members of a species embody is the locus of a homeostatic cluster of properties: true beliefs that are reliably produced, that are instrumental in the production of behavior successful in meeting biological needs and thereby implicated in the Darwinian explanation of the selective retention of traits. (2002: 62)

Kornblith rebuts six arguments that prima facie speak *against* his position and *in favour of* the view he attributes to Craig. Some of these arguments Kornblith finds in *Knowledge and the State of Nature*, others Kornblith invents on Craig's behalf.

According to the first argument, knowledge cannot be a natural kind since it is generated by humans. Kornblith replies that we think of water as a natural kind even though we sometimes produce it in the laboratory (2011: 44). The second argument says that knowledge cannot be a natural kind since it has an important social role. Kornblith counters by remarking that natural kinds can have social roles, too. Gold is a case in point. Moreover, the social role of gold can, at least in good part, be explained by its natural properties. One relevant such natural property is the relative rarity of gold (2011: 45). The third argument is that knowledge answers to specifically human needs. Kornblith denies this: animals have knowledge, too. The fourth argument is a variant of the third: flagging good informants is key to knowledge. Again, Kornblith responds by rejecting the idea that knowledge has a special link to humans. Animals can be said to know things, but animals do not flag good informants (2011: 46). The fifth argument amounts to the claim that the concept of knowledge is a social construct. Kornblith grants as much but opposes the inference from this claim to the idea that knowledge itself is social (2011: 47). Finally, sixth, Kornblith imagines the Craigian to emphasise that knowledge is normative—and hence not a natural kind. Kornblith accepts the premise but rejects the conclusion: health too is normative but it still is a natural kind (2011: 48).

6.3 Is Knowledge a Natural Kind?

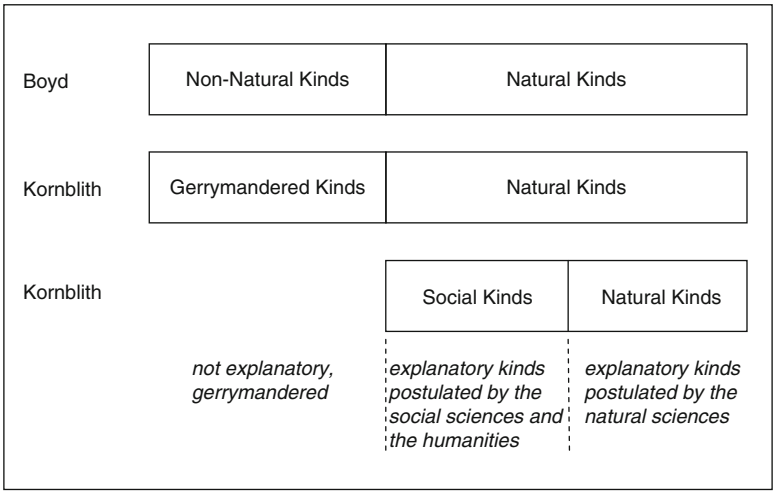
In this section I shall address the question whether knowledge is a natural kind without yet bringing in Craig's genealogy of knowledge.

There are of course philosophers who deny that there are *any* natural kinds—never mind whether we are dealing with knowledge, water or aluminium. For instance, Ian Hacking writes that "some classifications are more natural than others, but there is no such thing as a natural kind" (2007: 203). I sympathise with this scepticism, but I shall not make use of it here. For the purposes of this paper I shall grant Kornblith the idea that there are natural kinds of some sort.

I have already mentioned that Kornblith accepts Boyd's conception of natural kinds. Boyd defines natural kinds as causally important "homeostatic property clusters". These are co-occurring properties such that

1. the presence of some of them tends to favour the presence of others of them; or
2. there are underlying mechanisms that tend to maintain the presence of all or most of the properties; or
3. both (i) and (ii). (Boyd 1988: 197; 1999: 143)

Boyd denies that there is *one unique set* of natural kinds. That is, he explicitly endorses John Dupré's "promiscuous realism" about natural kinds (Boyd 1999: 160; Dupré 1993). Natural kinds are relative to "disciplinary matrices" (scientific disciplines or groups thereof). And it is natural kinds that allow disciplinary matrices to achieve their "characteristic inductive, explanatory or practical aims" (Boyd 1999: 148). Disciplinary matrices are constituted in part by human interests, projects and practices and thus these interest, projects and practices are "partly definitive of natural kinds" (Boyd 1980: 642–3).



Picture 6.1 Kinds of kinds in Boyd and Kornblith

Boydian natural kinds are not confined to the disciplinary matrices of the natural sciences. Intellectual history and the social sciences have natural kinds, too. Thus feudal economy, capitalism, Islam, or Empiricism, are all natural kinds. Philosophy too has its natural kinds: thus disciplinary matrix, natural kind, and scientific knowledge are natural kinds in Boydian philosophy of science. Even individuals qualify as natural kinds. For Boyd there is no essential difference between natural kinds and natural individuals. Napoleon functions like a natural kind in political history (Boyd 1999: 154–6, 162–4).

Although Kornblith claims to be following Boyd's understanding of natural kinds, his writings are not always clear on this point. For instance, in his 2002 book Kornblith works with two different conceptions of natural kind that I shall distinguish as the *wide* and *narrow* reading. On the wide reading, the opposite of a natural kind is a "*gerry-mandered kind*", a kind held together by nothing but "our willingness to regard [it] as a kind" (2002: 10). A gerrymandered kind might consist of the tip of my nose, the Vienna *Hofburg* and the number 255. Boyd thinks of natural kinds as wide. The narrow conception of natural kind does not appear in Boyd: here natural kind contrasts with *social kind*. Both (narrow) kinds of kinds have theoretical unity, but whereas natural kinds are kinds introduced by natural science, social kinds are the kinds whose theoretical unity derives from a social role; presumably social kinds are the kinds of social science (Picture 6.1).

I mention this ambiguity in Kornblith's talk of natural kinds, since at times he seems to me to conflate the two senses. Take for instance his claim that explanations based on social kinds are less deep than explanations in terms of natural kinds (2002: 10). That claim makes sense as long as we think of social kinds as gerrymandered. But it makes little sense if we think of the social kinds of "democracy" or "monarchy" within the disciplinary matrices of political theory or sociology. Or

consider Kornblith's contention that if knowledge were a social kind then epistemology would be less worthy of our attention (ibid.). It is hard to see why this would be so on the understanding of knowledge as a kind possessing theoretical unity. And yet, it has some (at least initial) plausibility on the rendering of knowledge as a gerrymandered kind.

Be this as it may, my main objection to Kornblith in this context is that he writes as if there were only *one single natural kind of knowledge*: knowledge is what cognitive ethology tells us it is. Coming from someone who claims to have adopted Boyd's theoretical framework, this is an odd view to take. After all, Boyd explicitly endorses a promiscuous realism about kinds. From a Boydian perspective, Kornblith's emphasis on cognitive ethology must be countered with questions like the following: Why isn't the kind "knowledge" used in the *sociology of knowledge* also a wide natural kind? Or why isn't the kind "knowledge" as used in our *folk psychology or folk epistemology* a wide natural kind, too? For the sociologist of knowledge, knowledge is "purely … a natural phenomenon". Knowledge is "whatever people take to be knowledge". Knowledge consists of "those beliefs which people confidently hold and live by … which are taken for granted as institutionalised, or invested with authority by groups of people". Knowledge is "what is collectively endorsed" (Bloor 1991: 5). This natural kind of knowledge does not coincide with the natural kind of knowledge investigated by cognitive ethologists. But this does not weaken its credentials within the disciplinary matrix of the social sciences.

A similar case can be made for our talk of knowledge in everyday life. It too qualifies as a natural kind for Boyd since a number of scientific disciplinary matrices (intellectual history for example) rely on this concept for its explanations and predictions. Note also that the everyday concept of knowledge differs from the concept of knowledge as it is used in cognitive ethology. In everyday life we attribute knowledge primarily to individuals, in cognitive ethology primarily to species.

How might Kornblith respond to the Boydian who advocates a promiscuous realism concerning the natural kinds of knowledge? Are there considerations that might be used to defend his idea that the cognitive-ethological natural kind of knowledge is the only one that really counts? Here are four possible answers.

According to the first proposal the cognitive-ethological kind explains the social role and thus the social kind of knowledge. Remember that for Kornblith the natural properties of gold explain its social role. I am not convinced. How does knowledge as species-wide reliable true belief explain (non-factive) knowledge as collectively sanctioned belief in a particular community? Even if it were to explain some parts of it, much would remain unaccounted for.

According to the second answer, cognitive ethology identifies the essential properties of knowledge. Other disciplinary matrices pick out merely its contingent or accidental properties. This too is not convincing. On the one hand, by what criterion are we to say that the ethologists' understanding offers the essential property? And, on the other hand, recall that the central property picked out by ethology—knowledge as reliably produced true belief—has been around in non-scientific armchair philosophy for a long time. That would not be good news for a naturalised epistemology taking its lead from the natural sciences rather than philosophy.

A third argument in defence of the primacy of the cognitive-ethological natural kind of knowledge is this: non-social animals can have knowledge, hence knowledge cannot be a social kind. This argument begs the question at issue. Why should we assume that the natural kind called knowledge by cognitive ethologists is the very same natural kind that sociologists or intellectual historians call knowledge? If they are not all the same kind, then the premise—non-social animals have knowledge—cannot establish the conclusion: knowledge is not essentially social.

The fourth, final, and 'sledgehammer' proposal is that only in the natural sciences do we have successful empirical theories. Fortunately Kornblith does not rely on this scientistic idea.

Kornblith's thesis that knowledge is a natural kind is directly linked to his claim that epistemologists have no reason to study our concept of knowledge or our intuitions regarding knowledge. In pressing this point Kornblith distinguishes two cases: that knowledge is a narrow natural kind like water, and that knowledge is a social kind (though a natural kind in the wider sense). His thesis is more plausible in the case that knowledge is a narrow natural kind. Chemists do not study water by studying our concepts of water (2007a: 39). Note however that it does not follow that our concepts (even our concepts of water) are without scientific interests. After all, concepts are—at least by Boyd's reckoning—natural kinds too: they are central in the disciplinary matrices of cognitive psychology, linguistics, and the philosophy of mind.

Kornblith's thesis—that concepts and intuitions are of little interest—is not very plausible, however, in the case of social kinds (2007b: 160–1). His dismissal of folk concepts and intuitions in the case of social kinds amounts to a rejection of "actors' categories" as important to social science. This is a highly contentious claim. Can we really make sense of, say, "democracy", "Islam" or "Empiricism" as wide natural kinds in political science or history, without paying attention to how these categories were understood by the historical actors themselves? Surely only in some pretty exceptional circumstances.

Let me sum up my difficulties with Kornblith's claim that knowledge is a natural kind. First, Kornblith claims to follow Boyd, but his championing of the ethological natural kind of knowledge contradicts Boyd. Second, Kornblith does not offer a compelling argument for his choice of a privileged disciplinary matrix. Third, Kornblith's dismissal of concepts and intuitions is problematic even if knowledge is a narrow natural kind. And fourth, it is even less convincing if knowledge is a social kind.

6.4 Craig's Genealogy of Knowledge

Up to this point I have explained what Kornblith takes to be problematic about Craig's genealogy of knowledge, and what I find unconvincing about Kornblith's specific brand of naturalised epistemology. I now turn to Craig's programme more directly. I first want to show that there is much more to Craig's overall project than the one ten-line quotation that Kornblith focuses on. My goal here is not a detailed

explanation or reconstruction; my aim is a rough sketch of what I take to be a new interpretation of Craig's genealogy of knowledge.

Note first of all that—especially in his 1993 German-language book—Craig situates his project in the proximity to two somewhat unlikely bedfellows: Wittgenstein and natural science (1993: 37). Wittgenstein is an ally since he opposes conceptual analysis in terms of necessary and sufficient conditions, studies the function of concepts, and introduces the category of family-resemblance concepts. Craig's project has affinities with natural science in its method of hypothesis testing, the search for explanation, and a focus on evolution. Going beyond his wording, I would add *model-building* to the list: the building of simplified (and possibly even distorting) models of complex target systems.

Craig's model construction has two stages: the first focuses on the "epistemic state of nature", that is, a small community of language-using humans, engaging primarily in face-to-face communication, humans who are co-operative, dependent upon one another for information, and of unequal skills and talents. The central question regarding this state of nature is: Why would a concept like "knowledge" be introduced under these idealised—simplified and distorted—conditions? Craig answers that people in this situation have a salient need, to wit, the need to pick out and "flag" good informants. And the concept used to flag good informants is the core—or one central aspect—of our concept of knowledge.

As Craig emphasises more clearly in 2007 than in 1990 or 1993, this is not taken to be a historical thesis: the epistemic state of nature is not a historical period like the Pleistocene (2007: 191). It is rather an ubiquitous and important type of social-epistemic situation that one is likely to find in all human communities, past and present.

Craig goes to great length to show that his model of the epistemic state of nature passes the test of (what the philosophy of scientific models calls) "external validation". He does so by arguing that his model predicts and explains several of the features of our concept(s) of knowledge that have been identified in various philosophical theories. For instance,

- uses of "knowledge" without belief (Radford) (Craig 1990: 15–6)
- the role of counterfactuals (Nozick, Dretske) (Craig 1990: Ch. III)
- the role of causal relations (Goldman) (Craig 1990: Ch. IV)
- the role of methods (reliabilism) (Craig 1990: Ch. IV)
- the role of justifying reasons (internalism about justification) (Craig 1990: Ch. VIII)
- that all analyses have counterexamples (Gettier) (Craig 1990: Ch. VI) and
- the contextual variation in standards (Unger) (Craig 1990: Ch. XII).

These theories are often seen as excluding one another, but Craig thinks that his model can partially vindicate all of them: they contradict each other only if we over-generalise them, and only if we do not see that knowledge is a family-resemblance concept.

The second stage of Craig's model construction adds a *dynamic* dimension to the state-of-nature. The dynamic model takes the epistemic state of nature as its starting

point and seeks to track how the concept of knowledge would evolve and diversify as the simplifications and distortions of the state of nature are step by step reduced. This suggests that the dynamic model is really a form of "de-idealisation". Craig focuses on a process he calls "objectivisation": this is the process in and through which there emerges a variety of uses of "knowledge" that are no longer tied to face-to-face spoken communication, the needs of a specific hearer or questioner, short testimonial chains, and small communities. These new uses differ from the old one, amongst other things, in calling for higher epistemic standards for knowledge, and in breaking the link between knowledge and testimony. Craig stresses that objectivisation is not an ad-hoc stipulation, but a tendency that can be observed in the development of many concepts (1990: Ch. X).

The dynamic model too needs to pass muster as far as external validation is concerned. Craig suggests that it correctly predicts, or at least makes sense of,

- contexts with very high epistemic standards (1990: Ch. X),
- the distinction between know-how and know-that (1990: Ch. XVII),
- intuitions about lottery propositions (1990: XI), and
- our conflicting intuitions about epistemic scepticism (1990: XII–XIII).

The above is only a very rough indication of where I now see the originality of Craig's project. In particular I hope to have made plausible that Craig's project is in the proximity of naturalised epistemology: his explanatory strategy has more to do with model-building in the sciences than with the traditional search for necessary and sufficient conditions for concepts like "knowledge" or "justified belief". It should also be obvious by now why I think Kornblith's exclusive focus on the first few pages of Craig's book is misleading.

6.5 Genealogy and Naturalized Epistemology

In the final step of my overall argument I shall try to relate Kornblith's project and criticism to my interpretation of Craig's programme.

I begin by restating what I have already emphasised at the end of my last section, to wit, that Craig's affinities with natural science, but also the specific parallels with Wittgenstein, show that the intellectual distance between Kornblith and Craig is not as great as Kornblith alleges.

Note first of all that even though Kornblith regards the concept of knowledge as epistemologically uninteresting, what he does say about the concept fits nicely with the Wittgensteinian and Craigian emphasis on diversity and disunity. Kornblith writes that our folk concepts of knowledge "are not terribly unified … [and] we will need to … start presenting them in all their splendid disunity" (2007a: 43). I agree, though I would add that Craig's epistemology offers one of the best means for capturing the disunity and unity in our concept of knowledge.

There is more of a tension between Kornblith and Craig at another point.

For Kornblith—as for Boyd—the central function of the concept of knowledge is to play a role in explanation and prediction. However, at least with regard to the first stage of his model—the static model of the epistemic state of nature—Craig rejects the focus on the need to explain others' behaviour, and instead picks as central the need to flag good informants:

> … the wish to explain, in some fashion, the behaviour of one's fellows, … (It … has been suggested to me, that this idea could help us to see the concept of knowledge as some sort of theoretical construct, useful for explaining why other members of our community behave as they do.) But just how widespread this concern with explanation is … is very hard to say … it would not be advisable to allow ourselves such a starting point before we have exhausted the potential for far less contentious claims about the human situation … (1990: 5)

This emphasis does mark a difference with Kornblith—but only as far as the first of Craig's two models is concerned. Craig's *dynamic* model (or at least a further development of it) leads to the prediction that further needs—in addition to the need to flag good informants—will also leave their marks on the concept. Consider two such needs: the need to explain behaviour, emphasised by Kornblith, and the need to mark the point at which we can terminate inquiry. That the latter is the main function of the concept of knowledge has been suggested by another critic of Craig, Klemens Kappel (2010). It is easy to see how these two needs become important in the process of objectivisation. Taking Smith to be a good informant regarding the location of tigers enables you to explain and predict some of his actions. There thus is a natural route from knowledge as a flag for a good informant to knowledge as essential to explanations of actions. Moreover, it is natural to shift from tagging the good informant to tagging the quality of the information he gives us. This involves a shift from "status" to "state", a shift familiar from social-psychological investigations (cf. Barnes 2000). And thus there is a route also from flagging good informants to marking the point at which inquiry may cease.

But is there not a deep divide between Craig and Kornblith in so far as Craig speaks of knowledge as "… something that we delineate by operating with a concept which we create in answer to certain needs …"? Does this not, as Kornblith alleges, really exclude the option that knowledge might be a natural kind? No, it does not. As Boyd emphasises: "… in a certain sense, human interests, projects and practices are partly definitive of natural kinds" (1980: 642). We build disciplinary matrices to satisfy certain of our needs and the explanatory, predictive and practical aims of disciplinary matrices determine which are the relevant concepts of natural kinds. This gives Craig all he needs to maintain his claim.

6.6 Conclusion

My aim in this paper has been twofold: to defend Craig's programme against Kornblith's criticism and to throw a critical light on Kornblith's own proposal. I hope to have shown that the very heart of Kornblith's naturalized epistemology—his treatment of natural kinds—is beset with problems, and that these problems

undermine his criticism of Craig. But this is not meant as a criticism of naturalized epistemology per se. It is merely to suggest that naturalized epistemology would profit from treating the genealogy of knowledge as a resource and ally rather than as an opponent. A concept-focused form of epistemology—at least Craig's version of it—is not incompatible with naturalized epistemology; on the contrary the two approaches complement each other.

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