

Response to critics

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I'd like to thank my critics for their thoughtful and challenging commentary. I appreciate the time and consideration they have given to my book, and I hope I can do justice to their objections in return.

This project started off in my mind as a way of solving a problem for reliabilist theories of justification, namely, their unacceptably lax treatment of doxastic, or inferential, justification. Clairvoyance cases are just the tip of an iceberg: it seems undeniable to me that some beliefs require argument, that they require inferential, or doxastic support, if they are to be justified. “Simple reliabilism” holds that reliability is sufficient for prima facie justification, thus, in essence, denying that any belief requires inferential support.¹ But take any hard-won item of science or philosophy: the belief that reliabilism is true, that bats are more closely related to primates than to rodents, that the moon is 2178 miles in diameter, and so on. There are many more: my belief that it's likely to rain today, that Christmas is going to be on a Thursday this year, etc. These are beliefs that—for us, at least—require inferential support. *Any* theory of justification that doesn't explicitly single out a class of beliefs as requiring doxastic/inferential justification is in danger of refutation from such examples. Coherentists won't have any trouble here, for they standardly take *all* beliefs to require support from other beliefs. But this goes too far in the other direction—surely it's okay to claim that *some* beliefs are basic, so long as we don't claim that *all* are? Reliability seems sufficient for the justification of those beliefs that result from perception, memory, introspection, and perhaps some other sources, as even BonJour (1985, p. 49) admits.

¹ Simple Reliabilism, as such, has had few if any deliberate proponents. I argue in Chap. 5 that the more sophisticated reliabilist theories don't do much better on this score.

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The examples show that the problem is bigger than clairvoyance, and the target is bigger than reliabilism. Any theory that allows for basic beliefs or immediate justification needs an account of which beliefs require evidential support from other beliefs and which do not—in other words, of which beliefs are basic and which beliefs are nonbasic. And to date, virtually none of these theories has offered such an account.

To see that the problem is not merely a problem for reliabilism, consider what amounts to something of a “received view” in contemporary epistemology; it combines “external object foundationalism” (according to which there are epistemologically basic beliefs about external objects, not only about one’s current mental states), evidentialism (the view that all justified beliefs have evidential bases or grounds), and experientialism (a type of evidentialism that allows nondoxastic experiential states to serve as evidential grounds). The core of the theory is some variant or other of the following principle:

If it seems to *S* as if *p*, then *S* is prima facie justified in believing that *p*.

Because the seeming states are nondoxastic experiences, they can serve as evidence for the basic beliefs without thereby undermining their basicity.²

However, without some restriction on the values for *p* or a great deal more said about what counts as “seeming” in the relevant sense, the core principle is hopeless. Some people (still!) believe that Barack Obama was born in Kenya, because it seems true to them. But clearly they aren’t prima facie justified. Presumably, it seems to Norman that the president is in New York. The very same beliefs that cause trouble for simple reliabilism cause trouble for the received view as well. The defender of the received view might object that I’m equivocating on ‘seems’ here: it doesn’t really *seem* to anyone as if Obama was born in Kenya in the same way it seems to me that the law of the excluded middle is true, and it doesn’t *seem* to Norman that the president is in New York in the same way it seems to me that the sound is coming from my left. But (a) this seems false, and (b) without a clear statement of what seeming is and a specification of the possible content of appearance states (all of which is standardly taken as primitive), such a response amounts to nothing more than taking refuge in the vagueness of the central concept of seeming, or nondoxastic experience.

What I think is going on here is that beliefs about the birthplace or current location of the president are nonbasic (if you’re going to believe that Obama was born in Kenya, you’ll need some kind of argument for it), but any theory that admits basic beliefs needs a theory of basicity that makes good on that claim.

A theory of basic beliefs must solve both the “Delineation Problem” (which beliefs are basic and which are not?) and the “Source Problem” (where does the justification of the basic beliefs come from?). My view has the primal systems theory solving the Delineation Problem and reliabilism solving the Source Problem.

² Perhaps the most detailed and sophisticated defense of the received view was due to Pollock (1986; Pollock and Cruz 1999), who preferred talk of being appeared to F-ly. Audi (1998), Huemer (2001), Pryor (2000), and several others also endorse this, or something similar to it.

A special sort of primal system is proposed to solve the problem of the bounds of perception.

My primal systems theories of perception and basic belief strike me as plausible in their own right, but the argument is greatly strengthened by an attack on experientialism. If I can show that nondoxastic experiences don't play the epistemological role generally assigned to them, then I can defend my "zombie epistemology", according to which justified perceptual beliefs are possible without any nondoxastic experience. This creates an opening into which my nonexperientialist, nonevidentialist theory fits nicely.

There is an implicit orthodoxy contained in the received view, though few if any of its proponents address these questions very explicitly: it is that the aforementioned problems (the Source Problem, the Delineation Problem, and the problem of the bounds of perception) are all solved by the nature of the corresponding experiential state. Nondoxastic experiences serve as evidence for basic beliefs (Source Problem); the contents of the basic beliefs are limited by the contents of the corresponding experiential states (Delineation Problem); and perceptual beliefs are just those that are directly based on perceptual, or sensory, experiences (the bounds of perception). Experientialism thus claims to do everything my systems theory-cum-reliabilism promises to do. If the experientialist could give a clear and well-motivated account of what experiences there are, what their contents are, which ones are perceptual, and how they can serve as evidence, my theory would be in trouble. This is why it is so central to my project to resist experientialism.

I take Graham's teleological concerns to offer, as he says, a friendly amendment to my own etiological constraint on primal systems. He and I seem to be in agreement about some of the most important issues here, including the failure of experientialism and the nonnecessity of nondoxastic experiences for justification. Later I will come back to his friendly amendment later and treat it the way helpful gestures are usually treated in philosophy, but for now I want to focus on the pro-experientialist challenges. I'm especially dismayed to have to defend myself against pro-experientialist challenges from Alvin Goldman, when I'd thought I was defending a Goldman-style reliabilism against an experientialist rival!

I say that I'm defending a "zombie epistemology" as a colorful way of highlighting the insignificance of experiential states on my view. Two qualifications are needed here.

First, I don't claim that there's *no* epistemic difference between us and zombies. On the experientialist view, my headache serves as evidence for my belief that I have a headache. On my view, nothing (ordinarily) serves as evidence for my belief that I have a headache. But my headache might nonetheless be indirectly epistemically significant. Suppose I believe via induction that I don't have a headache, when in fact I do. The headache (provided that I'm a reliable introspector) makes for a normative defeater, even if not an evidential defeater. It's a normative defeater in the sense that if I were to introspect, I would believe that I do have a headache, and so I shouldn't rely on induction in this case. Similarly, my view is that perception usually delivers basic (and thus ungrounded) beliefs about external objects. This is not to deny that we can sometimes form external object beliefs inferentially, by noting that we are having such-and-such an experience and inferring the existence of the object from

that. On the rare occasions when we actually do this, we enjoy an epistemic advantage over zombies; zombies are terrible introspectors—they think they have normal experiential states, when in fact they don't have any—and consequently, the indirect inference is unjustified for them.

Second, I am following what I take to be the standard view in supposing that zombies lack conscious experiences of any sort but nevertheless have beliefs. Horgan objects to this, claiming that a creature that didn't have any conscious experiences would therefore lack beliefs. I won't fight this; it is easy enough to restrict my attention to what he calls "partial zombies" (Horgan, forthcoming), where the relevant type of partial zombie is one that is devoid of *nondoxastic* experiences. Substituting this notion of partial zombie in for all my discussion of zombies should leave all the arguments intact. And I get to keep "Perception and Basic Beliefs" as my book title; I'll just have to change the subtitle to "*Partial Zombies, Modules, and the Problem of the External World*". Perhaps Horgan thinks that without nondoxastic experiences, an agent couldn't have genuinely meaningful doxastic states. But this seems implausible, especially given Horgan's concession that percepts have the default status of beliefs, a concession that increases the similarity between us and such partial zombies, who would have percepts as well as nonperceptual beliefs.

This brings us to the charge of separatism: the view that the phenomenological and intentional properties of a mental state are independent and separable. I admit that the passage he quotes from p. 70 seems to rely on separatist assumptions, although separatism is not a position I would have deliberately endorsed. But that's okay. I think my argument works without any separatist assumptions. What I *do* need to assume is that perception involves a cascade of lower to higher level states. For example, we have the raw primal sketch, the full primal sketch, the 2 ½ D sketch, the 3-D wireframe, the 3-D geon description, and the "identification": the classification of a distal object (or scene) as falling under a familiar perceptual category. For convenience, I used the standard terms 'sensation' and 'percept' to pick out the early and late states, respectively, though the terminology might mistakenly suggest that there are only two distinct states involved in normal perception, when in fact there are typically many more. Let us assume that each state in the cascade has both intentional and phenomenal properties and that these properties are inextricably linked. Nevertheless, the higher and lower level states are separable from each other, and this is something cognitive neuroscience assures us is true. Given the right kind of brain damage, I could undergo the early states without the late ones, and given the right neurosurgical intervention, I could undergo the late ones without the early ones, even though the phenomenology and intentionality of *each individual state* are interconnected. If all I experience is the last stage, say the "identification" of an object as a cat, then an evidentialist should deny that I am justified in believing there's a cat in front of me. If that's all being appeared to cat-ly amounts to, why should it serve as justifying evidence for my belief that there's a cat nearby? Why think it's even distinct from the belief that there's a cat nearby? (Indeed, I argue in Chap. 3, Sect. 3.2 that it's not, and Horgan seems to agree.) Horgan insists that the identification/percept/belief will still have a phenomenological aspect, a presentational (though presumably nonsensory) phenomenology, but I'd point out that Norman's clairvoyant belief has that as well, so the presentational phenomenology is not sufficient for *prima facie* justification.

In the end, I *am* committed to a kind of separatism, though not the one that Horgan explicitly rejects. Neuropsychological dissociations as well as computational and dynamic theories of perception indicate that the late perceptual states are distinct from the early states—not just logically distinct, or separable in thought, but really, counterfactually, separable; one could, with a carefully trained scalpel or magnet, separate them. Thus, I can grant that the cat identification/percept has its content and its phenomenology inseparably interlinked while insisting that this percept is quite separable from the accompanying sensations.

The separation between early and late perceptual states has deep implications for epistemology. Horgan is right that ordinary experience includes both sensations and percepts, and they seem inseparable. It consequently seems that perceptual experiences are unified wholes and thus that there is one thing that has presentational aspect, conceptual content, additional nonconceptual content, and sensory phenomenology. But the science assures us that this introspectively appealing view is false. The thing that has sensory phenomenology is distinct—separable—from the thing that has conceptual content. And now my dilemma applies: the former can't evidentially justify perceptual beliefs, because of the sensation/perception gap (see Chap. 3, especially Sect. 2); the latter can't because (a) it doesn't, from an evidential perspective, put the agent in a position different from Norman's, and (b) it *just is* the very perceptual belief it's supposed to justify.

Horgan claims that ordinary perceptual experience “includes not just such low-level aspects but numerous aspects of propositional content as well, all inextricably bound up with one another.” But this is no longer a mere denial of separatism; the low level and the high level states *are* extricable, and once we extricate them from each other, we see that none can play the role assigned by evidentialism. So, Horgan's anti-separatism is either the claim that the phenomenological and intentional properties of *a given state* are inseparable, in which case the claim is irrelevant to my argument, or it is the claim that high level percepts are inseparable from low level sensations, in which case it's false.

Nevertheless, there's some agreement here. Horgan says that the perceptual belief that *p* is justified by its (the belief's) being a percept as of *p*. This is very similar to my own view; although I also include a reliability requirement, I hold that an important part of what justifies a perceptual belief is its being a percept, i.e., being the output of a perceptual system.³ We differ, however, as to whether the justification here is evidential or not. Chapter 2 contains a lengthy discussion of the difference between evidential justifiers (like beliefs and, according to experientialism, nondoxastic states) and nonevidential justifiers or J-factors (like reliability, clarity and distinctness, the fact that the beliefs cohere). An evidential justifier is a positively relevant J-factor on which the belief is (at least partly) based. The fact that *x* is a percept is a positively relevant J-factor, but it is not itself part of the agent's *evidence* for *x*. The agent's awareness that *x* is a percept: maybe; the agent's false belief that *x* a percept: maybe; some other mental state distinct from *x*: maybe; but not the fact that *x* is a percept.

³ This looms large in Lyons (2005), where I argue thus in defense of (a restricted and reinterpreted version of) the claim that if something looks *F* to *S*, then *S* is prima facie justified in believing it is *F*.

Goldman also claims that a kind of evidentialism survives my attack, that the sensation-perception gap is not a problem for a reliabilist evidentialist. I argue in Chap. 5 that a reliabilist evidentialism of the sort that Alston (1988) or Comesaña (forthcoming) defends won't begin to work: change the example so that a tickle in his left thumb causes Norman to believe that (and reliably covaries with the fact that) the president is in New York (Norman, of course, has no idea that this covariation holds). Goldman claims that this objection to indicator reliabilism is not an objection to process reliabilism. The sensations shared by the novice and expert, e.g., are equally reliable indicators of the belief they also share, yet the expert is justified and the novice is not (here we have another counterexample to the Alston/Comesaña view). The difference is that the expert is using a *generally reliable process*, while the novice is not. So a process reliabilist account of evidence is superior to an indicator reliabilist account of evidence.

This move sidesteps my objections to the indicator reliabilist account of evidence, but there are two problems. The first is that we have no reason to believe that the sensory experiences are actually inputs to the belief-forming processes. It is nearly universal dogma among epistemologists that sensory experiences are among the causes of perceptual beliefs, but to my knowledge, no experientialist has offered any empirical data that support this over the claim that sensations and perceptual beliefs are joint effects of common causes.

Nor does the proposal go quite far enough. Reliable indication is insufficient for the evidential relation, but so is reliable indication-plus-generally reliable process. Norman and Truetemp are using generally reliable processes, and we can assume that these processes take sensations as inputs (this is almost already part of the Truetemp case, since his "tempucomp" doesn't interfere with his ordinary thermoceptive sensations). This is most obvious if we assume that the sensation is intuitively unrelated, like the tickle in the left thumb, but it works even if we alter the example so that Norman has a visual or quasi-visual experience of the president in front of a famous Manhattan landmark. If we understand the case to involve non-primal systems, the reliability of which is utterly unknown to Norman, the intuition of unjustifiedness remains. Once again, these sorts of cases serve as counterexamples to the received view as well, not just to reliabilist views.

Like Goldman, I endorse a reliabilist theory of what makes *e* evidence (for *S*) for *h*, but reliability alone won't do it; I also require that *h* be the output of a primal system that takes *e* as an input.⁴ I agree that "*in such epistemically fundamental cases*, a subject does not need to possess an awareness of the connection between

⁴ More precisely, I allow that anything can be evidence for anything else if the agent is justified in believing that the one is evidence for the other (or that the one renders the other probable, etc.); this is belief-dependent evidence. But belief-independent evidence (*e* is evidence for *h*, even though *S* doesn't have justified beliefs about reliability, evidential connections, and such) requires a primal system. It should be clear from all this that my view is incompatible with "evidence essentialism", the view that evidential relations hold necessarily. On my view, whether one thing is evidence for another depends not just on the reliability of the connection between them, which frequently varies from world to world, but also on the cognitive architecture of the agent. Whether *p* and $p \supset q$ serve me as evidence for *q*, i.e., whether they justify me in believing *q*, will depend in part on whether I'm wired with modus ponens. Modus ponens is equally reliable in any world, but whether it confers justification depends on the contingent makeup of the cognizer.

the internal state and what it indicates” for the former to be evidence for the latter (my emphasis). But I use my primal systems theory of basic *inference* to cash out this otherwise unexplicated notion of epistemically fundamental cases and to distinguish them from the rest. The situation is much like that with simple reliabilism: to say that reliability is sufficient, but only in a select set of cases, is to admit that reliability (alone) isn’t sufficient after all.

Even if his theory of evidence worked, it’s not clear how Goldman could use it to solve the clairvoyance problem. Evidentialist reliabilism entails that Norman must use a reliable evidence-utilizing process. But Goldman rightly rejects evidentialist reliabilism due to its failure to handle the justification of introspection and preservative memory, which don’t seem to be evidence-utilizing. I would add rational intuition and perhaps proprioception to the list; they don’t involve a nondoxastic experience distinct from my doxastic sense that $1 + 1 = 2$, or that my feet are nearly touching right now. These beliefs seem to be justified even though not based on any doxastic or nondoxastic ground.

So Goldman requires evidence for some beliefs, like perception and clairvoyance, but not for others, like memory and introspection. But it’s not at all clear what the rationale is for lumping clairvoyance in with perception, with memory and introspection in another category. What are the principles responsible for this grouping and how do we generalize to unspecified cases: does proprioception get lumped in with vision or with memory? Why? This, of course, is what my theory of primal systems is supposed to do: to categorize certain processes as requiring evidential inputs and others as not, though Goldman and I apparently disagree about which processes are which.

There is a further problem: in addition to an account of which beliefs require evidence and which do not, Goldman will also need an account of which beliefs require doxastic support and which don’t (i.e., which beliefs are basic). Because I endorse the Belief Principle, I claim that there’s just one account needed—all evidence is doxastic. The evidentialist endorses the Grounds Principle, thus claiming that all beliefs require evidence, but still needs to tell us which beliefs (if any) require *doxastic* evidence (i.e., which beliefs are nonbasic). Goldman rejects both the Belief Principle and the Grounds Principle, so he owes us two distinct theories: one about which beliefs require evidence, and one about which of these require doxastic evidence. The second problem here is not easily avoided; the Norman-with-the-tickle case as well as the original Truetemp case strongly argue that *some* beliefs require doxastic evidence, and no reliabilist will want to claim that they all do.⁵

I want to strongly resist this rapprochement with evidentialism; reliabilism is better off without it. Reliabilism has to make concessions to evidentialism, but I think the resulting theory is both more plausible and closer to the original spirit of reliabilism if those concessions are highly delimited. We can’t endorse a pure

⁵ Since Norman’s system is nonprimal, he would need doxastic evidence (e.g., justified belief in the reliability of his clairvoyant ability) to be justified.

reliabilism, but I think we can endorse a pure reliabilism *about basic beliefs*. By doing so, we simplify the theory and avoid entanglements surrounding the possibility of nondoxastic evidence, the nature of experiences, and the like. Evidentialist reliabilism may seem like evidentialism with a slight reliabilist twist, but with evidence completely removed from this part of the picture, the centrality and indispensability of reliability to the overall theory are assured.

I tried in the book to be pretty explicit about the role and status of intuitions in my epistemological theory construction. I want a theory that is acceptable to myself and hopefully to other epistemologists. To this end, it will have to make reasonable contact with as many as possible of the shared intuitions about cases and epistemic principles that epistemologists take to be significant. Where I deny an intuitively plausible principle (e.g., the Grounds Principle), I have work to do, appealing in part to other intuitions, to make that denial plausible. I don't think that the only thing worth doing in epistemology is getting the intuitions about cases just right—I'm not even certain a logically consistent theory could do so—but I want to construct a theory on the basis of things you and I are inclined to think are true (e.g., that unjustified beliefs can't serve as justifying evidence for other beliefs, that Truetemp isn't justified, etc.). Standard epistemological methodology is to offer a theory that explicates, clarifies, refines, and/or improves our pretheoretical understanding of justification. Just as Bayesian epistemology offers an artificially "precisified" theory of rationality, going beyond the fuzziness of the folk understanding, my theory helps itself to (what I claim are) the scientific concepts of perceptual modules and cognitive systems more generally. The necessary and sufficient conditions for justification laid out on pp. 177–178 constitute the "core theory," the aim of which is to give a "tidied up" version of the ordinary folk understanding. This core theory can be "dumbed down" ("folked up"?) to give a more accurate depiction of the folk concept (this is the "descriptive theory") or amended in various ways, including correcting substantive empirical assumptions about which primal systems there are, to develop a more revisionist epistemology (the "normative theory").⁶

Thus, I agree with Goldman that my notion of a primal system is too formal, scientific, and generally esoteric for the purposes of the descriptive theory, though not for quite the reasons he cites. In Chap. 6, I suggest the following (admittedly speculative) story: our epistemological intuitions are themselves the outputs of a primal system, therefore, basic.⁷ But as theorists we might ask how the module operates: what is the nature of the embedded "folk epistemology" this module presupposes? In general, an elucidation of folk theories (folk psychology, folk physics, etc.) is not obligated to restrict its vocabulary to personal level concepts held by the folk. In part, this is because the knowledge structures being elucidated are frequently modularized and thus independent of the general conceptual capacities of the cognizer. But also, it is because the elucidations are aimed at

⁶ This subject is addressed at more length in the second half of Chap. 6.

⁷ I don't suggest a module that specializes in epistemic intuitions; I'm agnostic about what else the module produces.

Marr's (1982) computational level of analysis and are thus to some extent noncommittal about the actual representations that effect the mapping (the representation-and-algorithm level). For both of these reasons, Marr himself is free to invoke zero crossings and 2 ½ D sketches without a hint that the folk possess such concepts. Goldman's constraint on conceptual analysis only makes sense if the project is one of characterizing the folk's potentially conscious, personal-level beliefs about justification, but there's no reason to think that such beliefs directly produce our intuitions. In any case, my goal was never to describe these potentially conscious beliefs.

Nevertheless, a full convergence between the scientific concepts and the intuition-driving knowledge structures would be surprising. The concept of a perceptual system is a sophisticated one, especially when given the full elaboration from my earlier (2001) paper on the cognitive neuroscientific methodology of lesion studies and double dissociations. Still, I think there is clearly a folk *analogue*: some vague notion of a cognitive or perceptual *faculty*. The folk and neuroscientific concepts have a recognizable affinity, much as the folk and biological concepts of *fruit* or *fish*, or the folk concept *weight* with the scientific concepts of *weight* and *mass*. I don't suggest that we can work from the scientific concepts to understand the folk analogues, only that there is enough affinity to make them analogues. Similarly, my account of "normal" etiologies is intended to capture what we pretheoretically expect the histories of the faculties to be. The folk concepts and expectations, I suggest, have been internalized by the module responsible for intuitions about justifiedness.

Connecting up with our intuitions is only part of the argument for the core theory. The whole argument is this: the theory is relatively clear and precise; its theoretical apparatus is scientifically respectable; its antiexperientialism allows us to cut through the Gordian knot of debate concerning the contents of perceptual experiential phenomenology, making progress on detail issues (e.g., do we see things as dogs, as canines, as animals, as quadrupeds?) by turning to the sciences, where centuries of introspection/phenomenology have let us down; it gives the epistemological answers the received view wants to give but can't, without a detailed theory of seemings/appearances (i.e., it classifies the "right" beliefs as basic, as perceptual, etc.); and it doesn't do violence to our intuitions, without smoothing them over with some kind of explanation. It is only this last element that directly calls on the descriptive theory to say what our intuitions are and why we have them.

I agree with Goldman that our intuitions in Norman cases track the perceived similarity between the clairvoyance capacity and ordinary perceptual capacities. In fact, I got the idea from his (1992) paper, and I've tried to incorporate it explicitly into my view. However, we disagree about what the relevant dimensions of similarity are. My Nyrmoon case is supposed to show that it's the developmental history of the faculty (and maybe something even wider—phylogeny, perhaps—we'll return to this) that modulates our intuitions of similarity to perception. The important point for now is that our sense of similarity does *not* seem to depend on sensory experiential phenomenology, as the received view requires and as Goldman suggests here. The difference between Norman and Nyrmoon is intuitively

significant, even without our ever mentioning their respective phenomenologies. It is true that my descriptive theory is bound to get things at least slightly wrong by offering strict necessary and sufficient conditions, if our intuitions are driven by a graded prototype-like similarity judgment. However, I'll be happy if these necessary and sufficient conditions provide a good (if caricatured) articulation of the relevant dimensions of similarity. The cognitive architectural and etiological dimensions seem to be far more relevant to our sense of similarity than the experiential or evidential dimensions.

Both Goldman and Graham note that my Nyrmoon and Vipertemp cases hint at an etiological constraint that is "wider" than the one I explicitly endorse. I favor a "narrow" etiological constraint: one that is only concerned with the ontogenetic history of the organism. Graham emphasizes a wider etiology, one that includes other contemporary and/or ancestral organisms. I really do take this as a more or less friendly amendment. If I can convince anyone that our intuitions in the Norman and Truetemp cases are modulated by *etiological* rather than *evidential* considerations, I'll take this to be a substantial victory, since my chief opponents are experientialists and evidentialists more generally, whether reliabilist or nonreliabilist. I won't resist very vigorously the suggestion that I should have opted for a wide etiology instead of the narrow one.

Still, I'm happy to explain why I chose the narrower etiological constraints. I confess that I have the "wrong" intuitions concerning Graham's "modified Norman" case; the mere fact that Norman is the first of his line to have clairvoyance doesn't affect my intuition that he's justified, so long as we conceive his clairvoyance module to be primal in my sense. I also have the intuition that Swampman has justified perceptual beliefs, even though he has no phylogenetic history. The primal systems view accommodates this take on Swampman, though the historico-teleological theory does not. If I understand Graham's historico-teleological theory correctly, the Truetemp case is easily tweaked to satisfy it (the implanted device is replicated and used because it worked so well in others), but we don't want to say that Truetemp is now justified.

These are just intuitions about cases, and Graham doesn't want an epistemology to be based entirely on intuitions about cases. Neither do I. A more abstract consideration against importing the historical/biological concept of teleology is simply that such a concept seems inappropriate for epistemological concerns. Some biological concepts are functional/ecological (e.g., *wing*, *reptile*) and some are historical (e.g., *clade*, *squamata*). Why—for *epistemological purposes*—should we prefer a historical theory of functions (e.g., Wright 1973) over a causal one (e.g., Cummins 1975)?⁸ If scientists were to synthesize human DNA in the lab, the resulting organism would not be human. She would look like us, and act like us, but she wouldn't *be* one of us—she's not *family*. *Human* is a cladistic concept, and she's not part of the clade, because she doesn't have ancestors. The distinction between her and us is one that a cladist couldn't ignore, but should it matter to an

⁸ A causal understanding of functions would still offer a wider etiological constraint than my own, though one that seems to me less counterintuitive.

epistemologist? To an ethicist? If so, why? Certainly not merely because it's the (I would say "a") concept of *human* employed by the biological sciences.

Furthermore, I don't have the space to defend it here, but I have serious doubts about the possibility of a historical theory of functions to fulfill its most alluring promise: that of providing a genuine naturalization of normativity, making good on the claim that the heart, e.g., is *supposed to* pump blood. The selectionist story makes certain normative terminology (e.g., 'purpose', 'supposed to', 'proper', 'normal') seem natural, but it doesn't really underwrite a robust teleology and therefore doesn't really underwrite a robust normativity. From the fact that hearts are here because they pumped blood, it doesn't follow that they are here *so that they may* pump blood, or, for that matter, that there is any purpose or value anywhere in the world. If we assume that there is some purpose, some genuine teleology, the selectionist history gives a good account of what that purpose is; but if we are agnostic about purpose and value, the history cannot shake that agnosticism. Obviously, this is a large and complex issue, and my brief comments here can do no more than explain why I declined the option Graham suggests.

I, too, am employing a scientific concept and imposing that concept on epistemology. I don't suggest that epistemologists should accept it simply because it's science; I argue throughout the book that epistemologists should accept it because it yields the right epistemological results. The concept of primal systems is not exactly that which drives our epistemological intuitions, but the two have enough affinity that the epistemological theory that reserves a role for primal systems is intuitively acceptable, while the scientific status of the central concept makes the theory a promising one for going beyond mere folk intuitions.

I have been arguing that neither evidentialist nor teleological repairs can help much with the clairvoyance-type problems. Even if they could, my theory would be still be preferable on the grounds that it solves a wider range of problems. Not only does it (a) solve the clairvoyance problem, but it (b) offers a distinction between basic and nonbasic beliefs, (c) offers a distinction between basic and nonbasic *inference*, and it (d) draws a principled distinction between perception and inference. It is a virtue of a theory to be able to solve all these problems at once.

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