Epistemology

Discussion

Rejoinder to Gregory M. Browne, "The 'Grotesque' Dichotomies Still Unbeautified" (Fall 2006)

A Beauty Contest for Dichotomies: Browne's Terminological Revolutions

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I am largely in agreement with the approach that Gregory Browne takes in his book *Necessary Factual Truth* (2001)—a fact that may have been obscured in my review (2005b) by the natural tendency of reviewers to focus on areas of disagreement. Still, those areas of disagreement are deep and abiding. I regard Browne as mainly on target in his Rand-inspired treatment of reference and necessity, as well as in his rejection of the "unbeautified" analytic-synthetic dichotomy; but in addressing his reply (2006) I shall argue, first, that he is mistaken in throwing some other vital distinctions, such as the a priori / a posteriori distinction, out with the analytic-synthetic bathwater; second, that he is nevertheless implicitly committed, under different terminology, to these very distinctions that he purportedly rejects; and third, that his treatment of kinds and definitions leads him both to misdescribe and to misprescribe ordinary language use, and also saddles him with a commitment to an unnecessarily high degree of semantic incommensurability.

Below I take up Browne's responses to my criticisms, following his reply section by section.

1. Deep Kinds

Browne claims that the membership of a Deep Kind is specified by *all* the qualities shared by the paradigm exemplars. I've argued that Deep Kinds should instead be specified by just the *explanatorily*

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fundamental qualities shared by the paradigm exemplars. But really that's not quite the right way to describe our disagreement. I don't wish to argue with Browne about how the membership of Deep Kinds is specified; after all, "Deep Kind" is his term, and he is free to stipulate whatever meaning he wants for it. (In effect, "Deep Kind" is a Shallow-Kind term.)

So let's draw a distinction. Call Browne's Deep Kinds "Deep$_1$ Kinds"; their membership is specified in whatever way Browne likes, and if possessing all the qualities shared by the exemplars is what Browne likes, so be it. I then hereby specify what I shall mean by "Deep$_2$ Kinds"—namely, kinds that are just like Deep$_1$ Kinds except that they are specified not by all the properties shared by the exemplars but only by the explanatorily fundamental properties (whatever they turn out to be). The issue between Browne and myself, then, is, first, whether natural-kind terms in ordinary language typically pick out Deep$_1$ Kinds or Deep$_2$ Kinds, and secondly, whether there is, or would be, anything wrong with their picking out Deep$_2$ Kinds.

My objection to regarding *water* as a Deep$_1$ Kind was that it would imply the absurdity that nothing would count as water unless it were found in the same geographical location as the exemplars. In reply, Browne seeks to block the undesired inference by excluding spatiotemporal (along with some other) attributes from the list of common qualities to be shared. Well, OK; as I said, Browne can specify the criteria for Deep$_1$ Kind membership in whatever way he chooses. But I can't see how this answer helps him, for it's easy enough to change my example to one involving attributes that Browne does *not* exclude.

Suppose that when the term "human" was first coined, every human the coiners had ever encountered was malnourished (not an implausible assumption for a primitive state of society). Then Browne's account would seem to imply that no creature who was not malnourished could count as human. This seems to me to be just the same sort of absurdity as in the water case; but being malnourished is not a spatiotemporal position (nor a haecceitas, nor a mere class membership), so my objection stands. As ordinary language is actually used, the term "human" would still apply to non-malnourished humans even if the term had originated in the context I described; this would be impossible if *human* were a Deep$_1$ Kind, but just what one would expect if *human* is a Deep$_2$ Kind.

Likewise, since the average human height has increased over the centuries, there was probably a time when no human being was over six feet tall. Now being under six feet tall, while of course it has something to do with space, is not a spatiotemporal position; so on Browne's view, if the term "human" had been introduced during the period when all humans were under six feet tall, then being under six feet tall would have to have been one of the criteria for membership in the human species. But on my view (which I take, on this point, to be Kripke's and Putnam's also), as being under six feet tall was not one of the explanatorily fundamental characteristics shared by the exemplars, it was never one of the criteria for being human, and the absurdity is avoided.

Or again, suppose that the linguistic community that first coined the term "human" had never encountered anyone with a skin color different from their own. Or anyone left-handed. Or anyone colorblind. (Or anyone *not* colorblind.) Suppose that even today we were to come across a previously unknown race on some remote island, differing from known races by no more than, say, blacks, whites, and Asians differ from one another. In all these cases Browne's criteria would require excluding the previously unknown population as nonhuman. But that's certainly not how the word "human" functions in the real world.

I think it's clear, then, that natural-kind terms in actual language pick out Deep_2 Kinds, not Deep_1 Kinds. But it is still open to Browne to claim that this fact indicates some sort of widespread conceptual confusion; hence even if our ordinary natural-kind terms *in fact* pick out Deep_2 Kinds, perhaps they *should* pick out Deep_1 Kinds instead.

Making such a claim faces an uphill climb, however. For it would seem *prima facie* a good thing, e.g., not to exclude previously unknown populations from the human species. Still, what exactly is supposed to be wrong with Deep_2 Kinds, on Browne's view? He appears to offer two answers.

First, if we exclude some qualities from the criteria of kind membership, Browne says, this will introduce "a necessary-contingent dichotomy within kinds" (Browne 2006, 125). But Browne has done that already. For one thing, Browne's system already includes Shallow Kinds, whose membership is specified by a definite known list of attributes. If being red is on the list and being round is not, then in a certain sense a member of that kind will, *qua* member of that kind, be necessarily red but contingently round. For another, even Browne's Deep_1 Kinds exclude spatiotemporal position, so in that sense, while boiling at 100º C will be a necessary property of water, if it should turn out that all the water in the universe is confined to the Milky Way, its being so will still be a contingent property of water. If this sort of necessary-contingent dichotomy is a problem for Deep_2 Kinds, it's a problem for Deep_1 Kinds and Shallow Kinds as well. On the other hand, if the sort of necessary-contingent dichotomy involved in Deep_1 Kinds and Shallow Kinds is not the sort that's supposed to be metaphysically objectionable, why should it be objectionable in the case of Deep_2 Kinds?

If humans as a class were right-handed, but only contingently so (in the sense that a left-handed person, were one to arise, wouldn't be excluded from the class), that doesn't imply that any particular human being is contingently right-handed, and it was my impression that the latter sort of contingency is the one to which the Rand-Peikoff view was opposed. I don't care, of course, which sorts of thing Browne *calls* contingent. But he shouldn't call *one and the same feature* objectionably contingent when it occurs in Deep_2 Kinds, but not when it occurs in Deep_1 Kinds or in Shallow Kinds.

Second, Browne claims that "trying to separate the fundamental . . . attributes from the other . . . attributes is valueless in the current context anyway, because the former entail the latter" (125). I do not understand this. The fundamental explanatory attributes shared by the exemplars to whom the term "human" was originally applied presumably do not entail being malnourished, or being under six feet tall, or having certain racial characteristics.

Browne mentions in passing that he agrees with Rand and Peikoff that all events other than free will choices are necessitated. I have a hard time seeing how this could be so. Aristotle observed that one cannot make a saw out of wool; I would add that one cannot make a free agent out of matter that acts by necessity. There are two ways to address this problem. One is to deny that free agents are made of matter at all; on this view, a free agent is something separate from, and causally interacting with, its body. This option seems unpromising, as it reduces agency to a transaction between, to paraphrase Rand

([1957] 1996, 939), a ghost and a corpse. The remaining option, which I favor, is to affirm that the matter of which free agents are composed is itself non-deterministic—as in fact quantum physics suggests that it is. This is not to say that subatomic particles have free will (they don't have *will* at all, free or otherwise), nor that the behavior of such particles is uncaused (it is caused by the particles themselves, actualizing potentialities inherent in their nature), nor yet that a free agent's choices are caused by the particles' behavior (being composed of non-deterministic material *enables* our free choices, but doesn't *cause* them; *we* do that).

2. Truth By Convention

Browne's position is that a sentence like "All bachelors are unmarried" conveys a fact about bachelors, not just about language use—so that although the *meaning* of the sentence might be determined by convention, its *truth* is determined by the world. I suggested that Browne hadn't said enough to allay the worry that "there might turn out to be simply nothing to determining the meaning of the term 'bachelor' over and above determining the rules for the term's correct usage" (Long 2005b, 220)—thus leaving nothing for the world to contribute beyond what convention contributes. I raised this point not as an *objection* to Browne's position, since on this point I'm in agreement with Browne, but as a suggestion that more needs to be said to defend the position we share. (To clarify, Browne and I don't have *quite* the same position, since while we're both realists about the truth of statements like "All bachelors are unmarried," Browne's realism about Shallow-Kind truths appears to me to slide into reflectionism, which my realism about them seeks to avoid; *cf.* Long 2004, 365–67. But explaining our disagreement on that point would take us too far astray at present.)

Browne's reply is that in learning the convention that "bachelor" is to be used only of unmarried men, we *thereby* learn the fact that being unmarried is a necessary property of bachelors, and so the reason the world contributes nothing more than what convention contributes is that what convention contributes *already includes* a contribution from the world.

That strikes me as the right position to hold, but not as a helpful answer to the aforementioned worry. For that worry is precisely the worry that being unmarried *isn't* a necessary property of anything, prior to and apart from the convention in question. It's not as though being unmarried is a necessary property of the entities to which we're proposing to apply the term "bachelor"; those entities can get married, and many of them will. It's only *qua* bachelors that those entities are necessarily unmarried, and the worry is that what it is to be something *qua* bachelor is an artifact of the convention, not a fact about the world.

I believe that this worry can be met, but that the way to meet it is to show that it can arise only from *outside* the linguistic practice in question, and cannot coherently be raised from within it. Thus no one who assents to the proposition that bachelors are necessarily unmarried (thereby participating in the practice) can consistently add "oh, but that's not a fact about the world."

3. Open-Endedness

I don't think I disagree with anything Browne says under this heading.

4. Conceptual Claims and Causal Claims

I had suggested that by defending the existence of Shallow Kinds, Browne had shown how the notion of conceptual truths, and in particular Eric Mack's invocation of the distinction between conceptual and causal claims in his recent (2003) critique of Randian ethics, could be reconciled with the Randian rejection of the analytic-synthetic dichotomy. To this Browne replies that he accepts an entailment theory of causality, and so rejects any distinction between conceptual and causal claims.

But I don't see how my point turns on accepting or rejecting an entailment theory of causality. Suppose I say "you can't become a successful Sumo wrestler unless you follow a high-protein diet." Now you might assume that I mean that success at Sumo wrestling involves certain skills and attributes that it's just not possible to achieve except by means of a high-protein diet—that such a regimen is, as we might say, a causal precondition of becoming or remaining a Sumo wrestler. Call that Case A. It might turn out instead, though, that all I mean is that "successful Sumo wrestler" is a Shallow Kind, and one of the attributes specified in the definition of that kind is that of having followed a high-protein diet—so that no one who hasn't followed such a diet will *count* as being a successful Sumo wrestler, however formidable they may otherwise be. (To invoke Aristotelian terminology, in Case A the diet is *instrumental toward*, while in Case B it is *constitutive of*, the end of being a successful Sumo wrestler.)

Now the difference between Case A and Case B is what I (and, I presume, Mack) mean by the difference between causal and conceptual claims; Case A involves a causal relation, while Case B involves a conceptual connection. If Browne thinks that the causal relationship in Case A is also in some way conceptual, or even that the conceptual relationship in Case B is in some way causal, fine, let it be so; there is still a clear difference between the two cases, and this difference is what is ordinarily marked by the causal/conceptual distinction. If Browne prefers different terminology, I have no objection; but the distinction remains, whatever we call it.

5. Newton's Axioms

Browne attempts to derive Newton's laws of motion, or some of them, from what he claims are attributes included in the Shallow-Kind definitions of *body*, *motion*, etc. Against this I argued that even if these are indeed Shallow Kinds, their definitions do not include attributes detailed enough to permit the derivation of Newton's laws. For example, I noted that neither *constant* inertia nor impenetrability to *all* bodies is part of the Shallow-Kind definition of *body*. To these claims —obvious and uncontroversial ones, I had thought—Browne simply says, to my astonishment: "I deny both of these claims" (Browne 2006, 129). Evidently Browne believes that if scientists were to discover, e.g., that inertia is not perfectly constant, competent speakers of English would, or should, conclude that no bodies exist. I find this claim so fantastic that it's hard to know what to say against it; as Brand Blanshard used to observe, you cannot reduce a thesis to the absurd if it is absurd already. But let me give it a try.

When you push an object along a relatively smooth surface and then release it, it keeps moving for a while, but more and more slowly, and finally comes to a stop altogether. We post-Newtonians say that, were it not for friction, the object would have kept going forever. But before Newton the dominant position was impetus theory. This theory came in different versions (some of them, I would argue, consistent with Newtonian inertia), but for many of them the accepted account of these matters was roughly as follows.

It was obvious enough that there was some sort of relationship between how hard you pushed a body and how long it kept going; thus the time it took a body to come to stop was thought to be, in effect, proportional to the force that had acted on it, and inversely proportional to the body's mass. Thus nothing short of a push with infinite (and therefore physically impossible) force could keep the body moving forever. It was known, of course, that the body would move farther on a smoother surface than on a rougher one; but it was assumed that for a given force and mass there was some finite stopping time on a frictionless surface, and increased friction would simply shorten that maximum time still further.

Now this theory was subsequently shown to be unreasonable in the light of laboratory experimentation and sophisticated observation. But I cannot see that it was unreasonable in the light of ordinary experience. Still less can I see that it was unreasonable in the light of linguistic usage. If anything, I would maintain on the contrary that what impetus theory says about bodies and motion is the most natural reading of our ordinary experience, and that Newton's counter-intuitive theory could be justified only on the basis of experimental evidence, and not by deduction from Shallow-Kind definitions. But Browne must apparently claim that when the impetus theorists used the word "body" (or the Latin equivalent), they were *misusing* it, since they clearly did not treat constant inertia as an attribute of bodies—and that, rather than wasting time rolling balls down inclined planes and charting the movements of the planets, Galileo, Newton, and their scientific contemporaries could have refuted impetus theory simply by analyzing the implications of ordinary usage. (Or perhaps the claim must be that the impetus theorists did not really believe in bodies, and that it was Newton et al. who first proved the existence of bodies!) At any rate, Browne's theory appears to imply that the impetus theorists were not competent users of the term "body," even though their use of that term fit far better than Newton's did with everybody's ordinary experience with and understanding of bodies. If this is his claim, the burden of proof clearly lies with him.

Browne wonders why I say that his interpretation would render Newton unable to claim that he was proving something about objects people already knew about. After all, notes Browne, he "took pains to argue that the referents of the terms" that Newton used "were the same as the things referred to by those terms in ordinary language" (131). Whence, then, my objection?

First, though, we've just seen that those terms, *as Browne interprets them*, are *not* what people generally meant by them prior to Newton. But second, even if we grant that Newton's laws really were contained in inferential embryo in the ordinary meanings of these terms, once people came to see that their usage did commit them to Newton's laws, the rational response would be to wonder whether their terms really referred to anything real after all. If the existence of bodies implies so much more than we thought, should we accept these implications, or should we instead question the existence of bodies—since for all these deductive arguments show, our experience could equally well be explained by the existence of *shmodies*, which are just like bodies except that they follow the laws of impetus theory instead of Newton's laws?

That's one reason the action axiom is so important in Austrian praxeology. Once we see how much surprising stuff is deductively implied by the concept of action, we might be tempted to wonder whether there really are any such things as actions, or whether we might make do with some weaker notion—but then the action axiom shows us that we cannot coherently deny the existence of action. In Newton's system there is no analogous axiom proving that we cannot coherently deny the existence of bodies. The inductive evidence to which Newton appeals is, then, not dispensable. On my view it is needed to support the claim that bodies follow Newton's laws; on Browne's view it is not needed for that, but then he must grant that it is needed instead to support the claim that there are any bodies. Once again, Browne's terminological innovations simply relocate or redescribe the problem they were supposed to eliminate.

6. Einstein and Commensurability

The laws of motion in Einstein's physics differ from those in Newton's physics. Einstein took himself to be discovering something new about the same objects that Newton was talking about; but many philosophers of a more conventionalist bent—including logical positivists on the one hand and Thomas Kuhn on the other—have maintained, in their different ways, that the meanings of terms like "mass" and "force" are determined by the theories and laws into which they enter, so that whenever scientists change their minds about the theories and laws, they are thereby changing the meanings of the terms. This view has the disappointing implications that Einstein was not teaching us something new about the very things that Newton had studied, but was instead changing the subject.

One of the principal selling-points of both the Kripke-Putnam and Rand-Peikoff theories of reference was that they offered a way to avoid the implication that change in theories or laws is always a change in meaning; Putnam in particular was concerned to counter positivist and Kuhnian interpretations of the relation between Einstein and Newton.

Now Browne maintains that Einstein *did* change the meanings of the terms; and so I objected that Browne, in trying to synthesize the Kripke-Putnam and Rand-Peikoff approaches, was giving up one of the most important features of both.

Browne's reply is that *in general* he thinks changes in theory (and presumably laws too?) do not, or at any rate need not, involve changes in meanings, and thus he is not committed to any Kuhnian doctrine of incommensurability across paradigm shifts and scientific revolutions. That is because in the cases where there is no change of meaning, the change in theory involves only change in *nonessential* properties (i.e., I presume, either properties not listed in the nominal definition of a Shallow Kind, or properties not common to all the original exemplars of a Deep$_1$ Kind). But Einstein's innovations, Browne maintains, involved changes in *essential* properties, and so counted as a meaning change. Hence Browne is committed to regarding *Einstein's* revolution as involving a change in meaning, but is not committed to regarding scientific revolutions *in general* as involving a change in meaning.

On this point, as Browne notes, I originally misinterpreted him. In reading Browne's claim that "if we revise some statements we are no longer talking about the same *subject*" (2001, 329), I had interpreted the phrase "if we revise some statements" as meaning "if there are some statements such that we revise them," whereas Browne actually meant "there are some statements such that if we revise them"; in other words, I had read the claim as "(x)(Fx) p" instead of as

"(x)(Fx p)." So my disagreement with Browne on this point is not as radical as I had thought; I certainly agree that continuity of extension cannot survive *all* changes of intension.

But my response to what Browne says about Einstein is, first, that we've been given no good reason to believe (and I claim I've offered good reason to doubt) that the ordinary meaning of "body" before Einstein involved a Shallow-Kind definition detailed enough to entail Newton's laws, and thus detailed enough to make Einstein's theories require an alteration in essential properties; and second, that while Browne may be able to avoid Kuhnian incommensurability for scientific revolutions in general, if he cannot avoid it for the Einsteinian revolution in particular then his theory lacks one of the principal advantages of the Kripke-Putnam theory—and after all, avoiding a Kuhnian take on Einstein is arguably the *paradigm case* of what was supposed to be the superiority of the Kripke-Putnam treatment of scientific revolutions over its conventionalist rivals. Browne's account of Einstein thus represents a move away from Kripke-Putnam in the direction of conventionalism. *Hic sunt leones.*

And the problem goes beyond just Einstein. Given, first, that Browne's Deep_1 Kinds have so many *more* essential features than my Deep_2 Kinds; given, second, that Browne tends to see implicit Shallow-Kind definitions as far more detailed than I do, and so again as making *more* features essential; and given, third, that Browne entirely rejects family-resemblance concepts, thus disallowing yet a third possible source of "give" in a semantic system—from all this it follows that his approach is going to make referential incommensurability across theory change *far more frequent* than mine will. Since multiplying cases of incommensurability seems like a drawback in a theory, this very fact constitutes an objection to Browne's position. In effect, his theory makes semantic relations so rigid as to be brittle, giving the meanings of terms a tendency to shatter under the impact of theory change rather than bending and swaying to adapt to such change. Fortunately, real-world human languages are far more flexible —and thus livable.

It would seem, incidentally, that Browne ought, on his premises, to say either that Einstein showed that there are no bodies (i.e., that no things exist meeting the Shallow-Kind definition of the ordinary term "body," since that definition entails Newtonian laws that

Einstein disproved) or else that Einstein convinced us all to change what we mean by "body," so that, given what we now mean, nobody before Einstein believed in bodies. It's a credit to Browne's common sense, but not to his consistency, that he seems reluctant to say either of these things.

In connection with the issue of commensurability, I also criticized Browne's account of intensions on the grounds that one of the main points of the intension/extension distinction is to preserve continuity of reference across changes in what is believed about the referent. To this Browne replies that "we can have the concept of intended properties, without equating them with the intension: we can simply say that they are the intended intension, as opposed to the (actual) intension" (Browne 2006, 133). Here once again what looked like a bold change in doctrine dwindles to a mere change in terminology. (Browne's conventionalist take on the Einsteinian revolution seems to have some application to the Brownean revolution also!) While I'm not interested in fighting about terminology, I would note that the technical terms "intension" and "extension" are well-established in philosophy, and it would be rather confusing to start using them differently. Browne thinks, however, that such a change would have the benefit of preventing the inferential slide from *intensions'* being mental to *essential and necessary attributes'* being mental. I doubt that the prevailing use of the term "intension" bears as much responsibility for that erroneous inference as Browne seems to think, but I'm happy to grant him his terminology if he indeed now grants that the intension/extension distinction as traditionally conceived *is* real and important after all, even if he wants to call it something else.

7. Family-Resemblance Concepts

I've argued that Browne is wrong both in rejecting Deep$_2$ Kinds and in identifying the definitions of Shallow Kinds. In my review I also suggested that Browne was too quick to reject Wittgensteinian family-resemblance concepts, since "people often have the semantic intention to specify a class in terms neither of all [as in Deep$_1$ Kinds] nor of the most fundamental [as in Deep$_2$ Kinds] properties of the paradigm exemplars, but rather in terms of overall similarity to the paradigm prototype" (Long 2005b, 218).

Browne's reply is that since family-resemblance concepts do not specify necessary and sufficient conditions, they "cannot pick out a definite extension beyond the actual world," and so cannot count as kinds, since "kinds can have different memberships in other possible worlds" (Browne 2006, 134).

Well, it depends what one means by "necessary and sufficient conditions." There's a straightforward sense in which a family-resemblance concept incontrovertibly has necessary and sufficient conditions for its application: namely, something's being *sufficiently similar overall* to the exemplar(s) is a necessary and sufficient condition for its falling under the concept. Browne might complain that being "sufficiently similar overall" is not very definite or precise. True enough, but ordinary concepts vary greatly in definiteness. It's much clearer whether one has a *quart* of something than whether one has a *heap* of something; but that doesn't mean that "heap" isn't a perfectly serviceable kind-term that ranges across other possible worlds. And "quart" isn't exactly free from indefiniteness either, once one gets down to the microscopic scale. As Rand notes, having fuzzy boundaries or "borderline cases" is a pervasive feature of concepts and does not render them invalid. In any case, family-resemblance concepts can always be made more definite; one might specify ten attributes of the exemplar and then say that something must have seven (any seven) of the ten in order to fall under the concept. Why wouldn't this count as a real, mind-independent kind with members in other possible worlds? (If this thing with six of the properties had had just one more, it would have been an X. . . .)

Browne speculates that the tendency to posit family-resemblance concepts is "an attempt by those in the Dichotomist tradition . . . to respond to implicitly felt difficulties with Dichotomism, which result from its failure to acknowledge Deep Kinds" (134). This hypothesis will have a hard time explaining why many who *do* acknowledge Deep Kinds still believe in family-resemblance concepts. It also oddly ignores the reason for believing in family-resemblance concepts that its proponents actually *give*: namely, the plethora of plausible real-world *examples* of such concepts. Perhaps there *could* be a language that made no use of family-resemblance concepts (though actually I have my doubts about that); but speakers of actual real-world languages do use family-resemblance concepts, and there's nothing objectionable about their doing so, nothing it prevents them from saying or thinking; what, then, is the problem?

Wittgenstein famously pointed to the case of *game*: there is no one feature that all and only games have in common (other than overall similarity to paradigm cases), but there is a family of properties such that each thing we call a game turns out to have *most* (or the most important, or most of the most important), though not all, of those properties, while the things we don't call games turn out to lack most (or the most important, or most of the most important) of these properties. Neither a Shallow-Kind nor a Deep-Kind analysis of "game" makes sense of how we actually use the term, but a family-resemblance analysis does. Yet "game" functions perfectly well as a kind-term, supports generalizations across possible worlds, etc. So there are family-resemblance kinds, QED.

To reply to this, Browne must show either that there *is* some feature that all and only games share (though no one has yet been able to find it), or else that *game* is not a kind (even though having-most-but-not-all of some set of properties is as good a necessary and sufficient condition as anything else). And *game* is of course only one of the many family-resemblance kinds that have been pointed out in the literature. Browne's claim that family-resemblance kinds are alien to ordinary language and support no interesting generalizations flies in the face of actual language use.

Browne takes as a damaging admission my passing remark that it's "hard to say whether such family-resemblance kinds are more similar to Deep or to Shallow Kinds" (Long 2005b, 219). But all I meant was that, e.g., it's not immediately obvious whether the familiar, actually-existing examples of family-resemblance concepts are best understood as including or as excluding as-yet-unidentified properties of the exemplars among those possession of which counts toward membership in the kind. If as-yet-unidentified features are included, family-resemblance kinds are more like Deep Kinds; if they're excluded, such kinds are more like Shallow Kinds.

Browne concludes this section by noting that he doesn't need to accept family-resemblance concepts because he can account for continuity of extension across changes of intension by invoking Deep Kinds instead of family-resemblance kinds. But to me that sounds a bit like a farmer saying he doesn't need to believe in oxen because he has a horse to hitch his plough to. The farmer ought to believe in oxen, not because he needs them for ploughing, but because there are oxen.

8. Apriorism and Newton

Browne is "rather surprised" at my description of his derivation of Newton's laws from Shallow-Kind definitions of "body," "force," etc., as an attempt to prove Newton's laws a priori (Browne 2006, 135). For Browne the a priori / a posteriori dichotomy is tied to the analytic-synthetic dichotomy and falls with it. This is a puzzling claim, though, since Browne's own exposition of Shallow Kinds is precisely what shows how a statement can be a priori without being "analytic" (at least as Dichotomists have understood that latter term). Indeed, in my article "Praxeology: Who Needs It" (Long 2005a), written after "Reference and Necessity: A Rand-Kripke Synthesis?" (Long 2005b) but published before it, I used Browne's own distinctions to explain (300–3) how Austrian apriorism could be defended against Randian criticisms and reconciled with a broadly Randian theory of concepts. (It's also worth recalling that the a priori / a posteriori dichotomy is significantly *older* than the analytic-synthetic dichotomy, so the charge of guilt by association is dubious.)

The puzzlement clears, however, once we realize that Browne's resistance rises, once again, from his commitment to idiosyncratic terminology rather than from a substantive difference in doctrine. Browne holds that there are certain facts we can prove about the members of a Shallow Kind simply by deducing those facts from the nominal definition specifying the criteria for membership. This is what most philosophers would or do call, and historically have called, a priori reasoning. And cases where mere possession of a concept is *not* sufficient to enable us to deduce such facts—cases where we need to seek out further sensory evidence—are those calling for what most philosophers would or do call, and historically have called, a posteriori investigation. Browne doesn't want to use those terms, but given the prevailing meanings of the term "a priori" and "a posteriori," Browne most certainly does accept an a priori / a posteriori dichotomy, since he, just like the rest of us, distinguishes cases in which mere possession of a concept is, from cases in which it is not, sufficient for deriving the relevant conclusion.

Browne defends his choice of terminology as follows. If one believes, as Browne does, that all concepts are derived from experience, and so are empirical, then if one nevertheless calls truths deduced from such concepts a priori, one is thereby committed, Browne concludes, to the "absurd" claim that certain propositions are simultaneously a priori (because deduced from concepts) and empirical (because the concepts from which they're deduced are themselves empirical).

I note in passing that the claim that all concepts are "derived" from experience is notoriously ambiguous. If it means that one needs experience in order to form the concepts, then even Kant was a conceptual empiricist. (That's the point of Kant's distinction between saying that ideas arise *with* experience and saying that ideas arise *from* experience.) If it means that concepts are *justified* by experience, i.e., that they stand to experience in something like the way that conclusions stand to premises, then it runs afoul of objections like McDowell's (1994). If it means something else, what is it? (For some of the problems this ambiguity raises for the Randian theory of the relation between perception and judgment, see Long 2000, 103–6.)

But in any case, in saying that anything deduced from an empirically acquired concept must likewise be empirical and therefore not a priori, Browne is departing from the usual meaning of the terms "empirical" and "a priori." What looked like a substantive disagreement turns out once more to be terminological. As far as I can tell, Browne does not, strictly speaking, think that most philosophers are saying something *false* when they call deduction from concepts a priori (though his wording sometimes gives that impression); rather, he thinks that the prevailing terminology is *confusing* or *odd*, and so proposes to substitute a terminology he finds less misleading. For example, he writes:

> It seems that by "a posteriori (empirical) knowledge" these philosophers simply mean knowledge from perception, and by "a priori knowledge" they simply mean knowledge from concepts. If so, they should simply call these two types of knowledge "perceptual knowledge" and "conceptual knowledge," respectively. . . . And just count all knowledge as a posteriori (empirical). (Browne 2006, 136)

In this passage Browne apparently admits that his disagreement with prevailing usage is terminological, not substantive. On the other hand, on the very next page he insists that his distinction is not the a priori / a posteriori distinction "by another name," so it's hard to be sure. But if indeed it is *not* the same distinction by another name, he owes us an account of the difference, because the criticism he actually gives of the distinction appears to focus on terminological issues alone.

Now it is admittedly true that under the prevailing terminology, we get the odd-sounding result that deduction from an empirically acquired source can count as a priori. But under Browne's proposed terminology, we get the equally odd-sounding result that discovering a fact merely by analyzing one's concepts counts as a posteriori. It's hard to develop a technical terminology that doesn't have some odd-sounding results. But ultimately it's more important to get straight about the facts themselves, rather than getting overly distracted by disputes about what to call them. I don't deny, of course, that terms should sometimes be abandoned when their meanings become hopelessly confusing or confused (I argue precisely this with regard to "capitalism" and "socialism" in Long 2006a), but I can't see that "a priori" is in nearly such bad shape.

Because I accept the a priori / a posteriori dichotomy, Browne leaps to the conclusion that I probably accept the analytic-synthetic dichotomy as well. This is puzzling, since I had thought that my review of Browne's book made it fairly clear that I agreed with Browne in rejecting that dichotomy—and indeed that I was in substantial agreement with Browne overall. (Would a *defender* of the analytic-synthetic dichotomy have had section titles like "Why the Dichotomy Ever Seemed Compelling" and "How the Dichotomy Was Overthrown"?) For what I take to be the principal defect of the analytic-synthetic dichotomy, see Long 2004, 361–65. Browne then spends a couple of paragraphs trying to convince me that demonstrative reasoning need not always take the form of conceptual analysis. Since I never dreamed of saying that all demonstrative reasoning is conceptual analysis, I happily endorse what he says in those two paragraphs.

There is, incidentally, a tendency for Randians to think of the theory of concepts in general, and the analytic-synthetic dichotomy in particular, as primarily an issue in epistemology. (For example, both my review [2005] and Browne's reply [2006] have been printed in this journal under the heading "Epistemology," as is this rejoinder.) Obviously the fact that the foundational Randian texts on these questions are found in a book titled *Introduction to Objectivist Epistemology* (Rand 1990) encourages this association. But the theory of concepts and the analytic-synthetic dichotomy, while of course they have epistemological implications, are not *distinctively* epistemological at all; they belong at least as much to the fields of metaphysics, logic, philosophy of mind, and philosophy of language. I suspect that the predominant association of these issues with epistemology among Randians has helped to blur the distinction, for them, between the analytic-synthetic distinction and the a priori / a posteriori distinction.

9. Divergences from Rand-Peikoff

Browne notes that my suggestion that his view "is at variance with that of Rand and Peikoff" is "a theme running through several objections" (Browne 2006, 138). This might make it sound as though I think all of Browne's divergences from the Rand-Peikoff theory are objectionable. (I'm not saying that Browne implies or thinks this, only that his remark might be misread that way.) On the contrary, while I think some of Browne's divergences are changes for the worse (as in his characterization of Newton's and Einstein's achievements), I noted that I regarded others as changes for the better—most notably the introduction of Shallow Kinds, and thus the possibility of what nearly everybody but Browne calls conceptual or a priori truths.

In this section Browne focuses precisely on this latter divergence, complaining that Rand and Peikoff have an "excessive aversion" to *more geometrico* reasoning. He notes that they would nevertheless have to accept such reasoning for logic and mathematics, and that they themselves implicitly practice it in ethics and economics; and he maintains that the Randian approach "can largely be harmonized with the Austrian economics of Ludwig von Mises . . . who does economics demonstratively and, in effect, *more geometrico*" (139). This is essentially what I said my 2005a and 2006b, so naturally I agree with it.

10. Conclusion: Rounding Up the (Un)usual Suspects

Browne ends his article with six paragraphs detailing all the pernicious confusions to which "the denial of necessary factual truths" and "the related beliefs in these various dichotomies" tend to give rise (140). I agree with, and applaud, pretty much everything that Browne says in those final six paragraphs. Where we disagree is as to *which* dichotomies are the "related beliefs" associated with the denial of necessary factual truth. We both agree that the analytic-synthetic dichotomy is one of the partners in crime and needs to be put away. But Browne casts his dragnet more widely, insisting that the distinctions between conceptual and causal relations, between a priori and a posteriori knowledge, and between intension (as ordinarily conceived) and extension are likewise guilty and must also be condemned in a great mass trial, along with various accomplices such as Kripke-Putnam Deep$_2$ Kinds and Wittgensteinian family-resemblance kinds.

I've argued that these additional dichotomies are independent of the analytic-synthetic dichotomy, and that Browne himself quite sensibly accepts them, albeit under other names; I've also argued that the allegedly complicit kinds of kinds are in fact necessary to account for what ordinary speakers actually mean by their words, as well as to avoid more Kuhnian incommensurability than Browne avoids, and that Browne has given us no good reason to reject such kinds. Let us not throw the fair and lovely dichotomies away in our haste to rid ourselves of the grotesque ones.

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