Pluto and the Platypus or: The Role of Classificatory Norms[1]

MATTHEW H. SLATER
Department of Philosophy, Bucknell University
http://www.facstaff.bucknell.edu/mhs016/

Draft for resubmission to *Mind*

Many astronomers seem to believe that we have discovered that Pluto is not a planet. But this is not so. Recent discoveries of trans-Neptunian Pluto-sized objects do not militate for Pluto's expulsion from the planets unless we have prior reason for not simply counting these newly-discovered objects among the planets. I argue that this classificatory controversy — which I compare to the controversy about the classification of the platypus — illustrates how our classificatory practices are "norm-laden". I conclude with a discussion of the relevance of norm-ladenness to other controversies in the metaphysics of classification, such as the monism/pluralism debate.

1. Pluto's Fall from Grace

Many astronomers now suppose that we've been laboring under the delusion of a simple and familiar solar system. Nine planets no more: the International Astronomical Union (IAU) recently resolved to define 'planet' in such a way that Pluto is excluded from their ranks; it is now classified as a "minor planet". According to their definition, a planet is a celestial body that:

(a) is in orbit around the Sun

(b) has sufficient mass for its self-gravity to overcome rigid body forces so that it assumes a hydrostatic equilibrium (nearly round) shape, and

(c) has cleared the neighborhood around its orbit.

On this definition, Pluto is not a planet. Recent observations show that it fails condition (c). Pluto's vicinity turns out to be crowded with other Pluto-sized objects.

Wails of protest rise up from some corners. Being a "minor planet" is cold comfort. What about tradition? *What will we tell the children?* Homely mnemonics now demand revision: "My Very Eager Mother Just Served Us *Nothing!?*"[2] *We cannot just change our minds like this!* Such appeals do not — and *should not* — affect the IAU's resolve. The history of science is *full* of mind-changes that reflect new knowledge. Indeed, it took the scientific revolution to establish the belief that the *Earth* is a planet on a par with the other "wandering stars" long known to ancient Greeks. But like a brown

[1] Thanks to audiences at the University of Idaho, Washington State University, and the Pacific APA for helpful comments and questions on this material. I'm particularly grateful to Stéphanie Ruphy and Alan Stern for their written comments on a previous draft and for helping to make sure I have my facts straight (though they should not be blamed for any remaining errors). I am also deeply indebted to four anonymous referees from *Mind*.

[2] One version ("My Very Eager Mother Just Served Us Nine Pizzas") records the usual order of the nine familiar planets: Mercury, Venus, Earth, Mars, Jupiter, Saturn, Uranus, Neptune, Pluto.

dwarf, so far, the discussion has given off more heat than light (even in scientific corners). In particular, there seems to be the feeling that there is a uniquely correct understanding of 'planet' dictated by recent discoveries at the edge of the solar system.

I doubt that this is so. Astronomer Michael Brown's comment — that "either tradition or logical consistency must be abandoned" (Brown) — badly mischaracterizes our situation. Things are not nearly so simple: no contradiction arises from cleaving to tradition and classifying Pluto as a planet. What, then, motivates Pluto's reclassification? Does the IAU's decision amount to the laying down of an arbitrary *convention* or does it reflect the *discovery* that Pluto is not a planet? That this is a false dilemma can be seen by making plain the role of *norms* informing our classificatory practice.

This paper will examine the Pluto fracas — interesting in its own right — in beginning an investigation of these norms, offering an initial sketch of what the relevant norms might be in this case and how we might in general identify, defend, and criticize them. Philosophers of science have been growing more comfortable with the idea of *epistemic* norms involved in theory choice. The present norms, however, fit uneasily in this category. Rather than guiding belief in certain theories, such classificatory norms appear to influence how we carve nature up into different kinds of things. In this sense, they are more like *metaphysical* norms. However, since I take no stance here about the metaphysics of classification, I will simply call them *classificatory norms*. Though to some degree optional, I argue that they ought to be distinguished from "arbitrary conventions". Their activity shows us, I believe, that a certain sort of classificatory pluralism may be more widespread than previously recognized. It has gone unnoticed because it can obtain even when its outward effects (viz. disagreement) are hidden. In addressing these questions, I attend to a kindred spirit in classificatory perplexity: the platypus.

2. Anomaly and Classificatory Revision

2.1. An Oddball

Historical precedent sides with science's propriety in renovating folk-taxonomic categories — in several ways. A stock example is the discovery that Whales are mammals and not fish. Superficially, whales and sharks have much in common. But as we learned more about their properties and evolutionary history, deep divisions were revealed: whales have *more* in common (both physiologically and phylogenetically) with land-mammals than they do with sharks.

Similar discoveries often significantly affect high-level categories (e.g., higher taxa like families or phyla). We discover, for example, that '*Reptilia*' does not name a monophyletic taxon (a group all of whose members descend from a common ancestor). As such, cladism — an influential school of systematics — does not recognize it as a legitimate potential referent of '*Reptilia*'.[3] To keep the

[3] There are other examples: some suggest that *Rodentia* should be contracted to exclude Guinea pigs contraction from the folk-category, with the recent exclusion of Guinea pigs (see LaPorte 2004, 63).

category, the cladist must expand or contract its membership — say, by including birds or excluding crocodiles. Suppose we contracted the category to exclude the crocs. It might then be tempting to say that we discovered that crocodiles are not reptiles.

These two examples illustrate different ways in which we might claim to have discovered that Pluto isn't a planet: first, by discovering something about Pluto that disqualifies it for fit within the category *Planet*; or second, by discovering facts that prompt the revision (or annihilation) of the category itself, excluding Pluto in the process. Does either model accurately describe Pluto's case? Before addressing this question, let us consider why Pluto was *initially* grouped with the planets.

One plausible answer is that Clyde Tombaugh found Pluto whilst looking for *Planet* X — the trans-Neptunian planet Percival Lowell reckoned was causing orbital perturbations in Neptune and Uranus. But this is only part of the story. The search for Planet X is what got Tombaugh looking so carefully. He *might* have found Pluto if he had been looking for a comet or asteroid in that part of the sky. Would it have been identified as a planet? We cannot say with any certainty — expectations about what they should find might well have played a role in the initially inflated mass-estimates of Pluto. Early information-gathering efforts centered on determining Pluto's size and orbital characteristics, which were pretty clearly characteristic of a *planet* (as they were known in the 1930s), rather than, say, a comet. V. M. Slipher wrote in the *Lowell Observatory Observation Circular* (May 1, 1930) that Tombaugh's discovery "appears to be a Trans-Neptunian, noncometary, non-asteroidal body that fits substantially Lowell's predicted longitude, inclination and distance for his Planet X" (Slipher 1930; quoted in Hoyt 1980, 212).

As astronomers learned more about Pluto, however, confidence that Tombaugh had found Lowell's Planet X waned. We learned that Pluto's brightness was due in large part to its high albedo rather than size (originally estimated to be in the neighborhood of Earth; an estimate off by several orders of magnitude).[4] We learned of its eccentric orbit, straying far from the ecliptic of the solar system, and crossing the path of Neptune in a 3:2 orbital resonance, unlike any of the other planets. In short, as the *Cambridge Companion to the Solar System* notes: "Pluto is an anomaly. It is much smaller than the giant planets that occupy the outer parts of the planetary system, and is comparable in size to some of their satellites. Pluto is smaller than Saturn's satellite Titan and all four of Jupiter's largest moons" (33). The influential astronomer Stuart Ross Taylor deems Pluto's anomalousness as clearly sufficient to exclude it from the planets:

> Tiny Pluto is commonly referred to as the ninth planet. The mass of Pluto, even when Charon is included, is very small. It amounts to less than one fifth of the mass of the Moon, 1/2000 of the mass of the Earth or 1/64000 of the mass of Jupiter.

[4] Though this was a matter of controversy for decades, it also turned out the "perturbations" Lowell thought he observed were far too subtle to be genuinely informative (being within the range of observational error); and Pluto was far too small to be causing them anyway. The fact that Clyde Tombaugh found Pluto in the vicinity of where Lowell predicted it would be seems to have been due to good luck and Tombaugh's diligence. Pluto thus represents a sort of Gettier case for reference. Tombaugh did not find what he was looking for.

> Pluto has a highly inclined and eccentric orbit. Sometimes it is inside the orbit of Neptune. . . . In the frozen twilight, nitrogen ice lies on the surface of Pluto. As Pluto [gets] closer to the Sun, it warms up a little. The nitrogen evaporates and forms an atmosphere. As Pluto retreats from the Sun, the atmosphere freezes out again. It is apparent that Pluto is not a planet, although no doubt it will long continue to be referred to as the ninth planet for a combination of traditional and sentimental reasons. (1998, 99–100)

Taylor here suggests an explanation of why, if Pluto was so anomalous, astronomers continued to classify it as a planet. The anomalies were not discovered until *after* it was grouped with the planets — a grouping that Lowell's followers (and indeed many Americans) were keen to maintain.[5] Political motivations aside, we can identify a more innocent kind of classificatory conservatism at work that would explain why it took scientists so long to reflect Pluto's anomalousness in its classificatory status. Such inertial effects merely explain the longevity of Pluto's erroneous planetary status — they do not justify it. Taylor might thus see himself as vindicated. It turned out that he overestimated the influence of tradition and sentiment — for here we are discussing Pluto's "demotion" from the planets.

But this is not the only explanation for Pluto's long stint as a planet despite the discovery of more and more of its anomalous features. For merely noting that Pluto is "anomalous" *tout court* does not, by itself, suffice to show that it not a planet. It might be anomalous in a classificatorily-irrelevant way. Outrageously tall humans might be thought of as anomalous in terms of their height, but this is of course irrelevant to their classificatory status. Membership in *Homo sapiens* does not turn on height. Taylor needs to show not just that Pluto is anomalous in various ways — it surely is — but that the particular parameters on which it is anomalous are relevant to being a member of the kind *planet*. The obvious way of showing this would be to point out that the clustering of certain values of a parameter were thought to be relevant to the classification of *other* objects within the kind. But it is also open to us to take the case of an anomalous object as showing either that the parameter in question was not, in fact, relevant to the kind or that exceptions could be made.[6] For whether a particular parameter is classificatorily relevant is not a matter decided exclusively by the world; it is a matter informed by the aims and purposes of the science in which the category is embedded.

2.2. An Odd Duck

[5] Here I take tradition and sentiment to include political (even broadly nationalistic) motivations. Seen in this light, Pluto represents a fascinating case study for the sociology of science.

[6] The possibility of tolerating exceptions is one of the chief attractions of Richard Boyd's homeostatic property cluster conception of natural kinds — particularly for messy domains like biology. For recent discussion, see Boyd (1999), Wilson *et al.* (2007), and [REFERENCE SUPPRESSED].

There is another explanation for why Pluto's general anomalousness was slow to exclude it from the planets: Astronomers had nowhere else to put it. Pluto's case differs substantially from that of the whales, in two specific respects. For one, the purported discovery about whales concerned not the classification of an individual *object*, but of a *category*. For two, and more importantly, whales were not seen merely as *anomalous* among fish, but more akin to a *different* group: the mammals.[7] This suggests an explanation for why Pluto was not disqualified as a planet, even as evidence mounted that it lacked many of the properties of other recognized planets — something that Taylor's explanation doesn't do particularly well at. If it was not a planet, then what was it? Note the eliminative character of Slipher's reasoning above: this object is not a comet, not an asteroid, and not some other familiar types of astronomical objects (moons, stars, galaxies, &c.), so it must be a planet. With no better option in sight (so to speak), Pluto's anomalousness as a planet could be tolerated.

In this way, Pluto's situation resembles that of the famous duckbilled platypus. Early naturalists struggled classifying this furry, duckbilled, aquatic, elusive egg-layer. Part of the initial dispute was empirical, focusing on whether the platypus actually nursed its young, a hallmark of being a mammal. The evidence was confusing: it lacked nipples but apparently possessed mammary glands — of which the French naturalist Etienne Geoffroy Saint-Hilaire petulantly demanded: "If those glands produce milk, let's see the butter!" (Moyal 2001, 58). But even when this was settled — the platypus indeed secretes a very rich, very mammalian milk through ducts on its belly —, resistance to placing it with other mammals understandably remained. The platypus lays eggs rather than giving live births. It has an electroreceptor-laden "duckbill". It has a poison spur and a cloaca (a channel into which both excretory and reproductive systems empty[8]), features common in reptiles but previously unheard of in mammals.

Unlike Pluto, though, the Platypus was not *initially* classified with things to which it was *later* found to be strikingly dissimilar. We might improve the comparison between Pluto and the platypus by thinking about two ways history might have gone differently: first, by imagining that the platypus was initially thought to be an mammal (this isn't much of a stretch, given how furry and shy it is!); or second, imagining that more of Pluto's anomalous features had been known right from the start (as in the case of the platypus). The second thought experiment is particularly instructive. Forget about Lowell, Tombaugh, American Astronomical Pride, the gross overestimations of Pluto's mass, classificatory inertia, and so on. Suppose Pluto was found fifty years later and that the better instruments of the time staved off glaring inaccuracies. Would we have classified it as a planet?

If the case of the platypus can serve as guide, a plausible answer is 'yes'. Though certainly not a *paradigm* mammal, the platypus appeared to fit *better* with the mammals than with any other group. As Grant puts it in his study: "When all its characteristics are considered, the platypus is much more

[7] As Dupré reminds us, the whales/fish example is not quite as clear-cut as many suppose; for 'fish' does not clearly name a biologically-respectable group (or ensemble of groups).

[8] Accordingly, it now belongs to the (mammalian) order *Monotremata*, from the Greek meaning "one hole".

mammalian than it is reptilian [or, presumably, anything else]. It has all of the necessary attributes to convince even the most skeptical taxonomist that it should be placed in the class Mammalia" (Grant and Fanning 1995, 5).[9] While this latter assertion is compelling, in the context of early platypus scholarship it would have been a dramatic overstatement. As LaPorte has argued, "cases like this spur theorists to refine the use of their terms to reduce vagueness" (2004, 116; see also Kuhn 1990). This refinement apparently could have gone in a number of ways, possibly toward the insistence that mammals *must* give live births.[10] Likewise, our imagined disco-era Pluto-discoverers *might* have used its discovery as occasion to make precise the meaning of 'planet' so as to exclude anything below a certain size-threshold or above a certain orbital eccentricity. What would they — what *should* they — have done in these cases? What informs such decisions? Are they unconstrained expressions of convention? I will suggest in the next section that they are not. They are guided by classificatory *norms*.

2.3. Evidence for the Operation of Classificatory Norms

The comparison between Pluto and the platypus illustrates a potential explanation of why their respective anomalousness did not lead to their exclusion from their initial categories:[11] astronomers did not have available a conceptual box in which to place these anomalous things. But this explanation only goes so far. Why not merely christen a *new* category, placing the platypus species in its own genus or Pluto in its own "species"? Why force them into a conceptual boxes of dubious fit?

I conjecture that the best explanation of our treatment of these cases is that there is a general norm — *avoid lonely categories* — guiding and to some extent constraining our classificatory practice. We can articulate this norm's application to the present cases by saying simply that the platypus and echidna alone did not deserve their own class alongside the diverse and populous mammals and that Pluto did not deserve to be the sole occupant of a novel class of astronomical objects.[12]

The *avoid lonely categories* norm would also seem to underlie Pluto's initial lot. Pluto was clearly more planet-like than star-like, comet-like, asteroid-like, and so on. It had to fit into *some* non-lonely category! Even skeptics like Taylor appear to accept this awkward reality:

> We will just have to put up with a solar system that has only eight planets, despite much hope for ten (a tidy number) or more, a wish that goes back to Kant. The ancients were content with five, as well as the Earth. Pluto is a cousin of Triton, but,

[9] The context of theistically-influenced naturalism may have played a compensating role. God had clearly established well-populated nested circles of similarity — why should there be a high-ranking circle with such a small low-level population?

[10] LaPorte persuasively argues that this picture of referential revision poses a serious problem for the popular causal theory of natural kind term reference (2004, 118–120).

[11] Despite Taylor's conviction, it really was *not* because of Pluto's anomalousness that ultimately resulted in the IAU's decision to reclassify it as a minor planet.

[12] What justifies this norm? I am concerned here only with motivating its descriptive plausibility; I address its justification in §4.1.

> like most relatives, is not identical. It is both smaller, a little darker and denser, probably the result of a different history. Pluto has a higher content of rock than Ganymede, Callisto or Titan, so calling Pluto an ice dwarf is a bit of a misnomer. *It's yet another example of the resistance of objects in the solar system to being put into neat pigeonholes*" (1998, 99–100; *my emphasis*).

Pluto apparently did not deserve a non-planet class of its very own. Interestingly, just as this norm is plausibly responsible for Pluto's initial inclusion with the planets, so is it complicit in Pluto's recent exclusion from their ranks.

3. Discovering Pluto's Non-Planethood?

Back to the question of whether we *discovered* that Pluto is not a planet. On what facts did this discovery depend? As it turns out, it was not by finding out more about *Pluto* (e.g., determining that it was in fact an alien artifact), but by identifying *other* Pluto-like objects in a distant zone of the solar system known as the Edgeworth-Kuiper Belt, the postulated home of millions of short-period comets around 30–55 astronomical units (AU) away.[13] Though Gerard Kuiper and Kenneth Edgeworth postulated this region in the 1950s — on the grounds that we shouldn't expect a sudden drop-off in the mass-density of the solar system, it wouldn't be until 1992 that the first "Kuiper Belt Object" (KBO)[14] — dubbed '1992 QB_1' — was found. Over the next decade or so, hundreds more small KBOs were discovered, some as large as three-quarters Pluto's mass. Things came to a head in July of 2005 with Brown, Trujillo, and Rabinowitz's (2005) announcement of object 2003 UB_{313} (now officially named 'Eris'). Two things stood out about Eris: it's bigger than Pluto and it has a moon (aptly named 'Dysnomia'). It started looking likely that Pluto was an inner KBO — one of potentially thousands.

Now, it seems clear that the discovery of a number of KBOs (especially Eris) led to Pluto's official "expulsion" from the class of planets.[15] But did we therefore discover that Pluto is not a planet? No. Return to the analogy with the discoveries surrounding *Rodentia* and *Reptilia*. Though tempting to regard each of these episodes as discoveries that each class was in fact bigger or smaller (potentially pathologically out of step with folk-biological terminology), LaPorte argues that we should regard the ensuing revision or abandonment as a *choice*. Rather than contracting *Rodentia*, we might have expanded it to include *all* the descendants of the ancestor of all paradigmatic rodents (horses, seals, and so on). If expansion and contraction of the category were both live options, it seems questionable to reckon either as a discovery (LaPorte 2004, 66–67). Likewise, perhaps all the discovery of Eris (and its siblings) does is force a choice: either we have fewer planets than ordinary thought or far, far more.

---

[13] 1 AU is the average distance of the Earth to the Sun (approximately 150 million kilometers).

[14] As often happens with the vicissitudes of scientific naming, Edgeworth was unjustly unrecognized in the naming of the region he independently postulated.

It's worth taking a moment to consider the choice/discovery dichotomy. Those with a sympathy for Quine's holistic empiricism, where elements of convention and discovery potentially infects any theoretical change will prefer to see this dichotomy instead as a gradient. Even paradigmatic discoveries — e.g., that Pluto's atmosphere freezes and thaws again during its orbit — incorporate an *element* of choice. For observational data do not uniquely compel this conclusion. This much is familiar.[16] If we are to make sense of scientific discovery, it will likely be in the context of accepted norms of theory choice, interpretation, evidence, and so on.

This seems dimly reflected in recent work on the Pluto affair. In his book *Is Pluto a Planet?*, David Weintaub puts the choice in conditional form. Shortly after describing the discovery of Eris, he writes: "Hence, if Pluto is a planet, then [Eris] also is a planet and October 21, 2003 marks the day when the (most recent) tenth planet in our solar system was found" (2007, 163). Either Pluto is *not* a planet or Eris *is*. After the discovery of many KBOs (though before the announcement of Eris), astronomer David Jewitt reflected on Pluto's place in these terms:

> So, bluntly put, one has two choices. One can either regard Pluto as the smallest, most peculiar planet moving on the most eccentric and most inclined orbit of any of the planets or one can accept that Pluto is the largest known, but otherwise completely typical, Kuiper Belt Object. The choice you make is up to you, but from the point of view of trying to understand the origin and significance of Pluto *it clearly makes sense to take the second option*. . . . The processes that shaped the orbits of the KBOs are the same ones that gave Pluto its prominent dynamical characteristics. Some people see this as a demotion of Pluto from Planet-hood. I think that it can reasonably be portrayed as a promotion. Our perception of Pluto has been transformed from a singularly freakish and unexplained anomaly of the outer solar system to the leader of a rich and interesting family of trans-Neptunian bodies whose study will tell us a great deal about the origin of the solar system. (quoted in Weintaub 2007, 183; my italics)

Of course, just when it "makes sense" to accept a certain theory or take a certain stance is a notoriously tricky issue. In this context, it would seem that Jewitt's tendency is to cinch tightly a constant number of categories as if in an effort to conserve string. If we can reduce the perceived anomalousness of Pluto by relocating it among the KBOs, better to do that than *increase* the number of anomalous objects among the planets.

This rationale would seem to presume *a priori* a certain degree of acceptable — or preferable? — diversity within a category. But the presumption begs the question. Counting planets among the Kuiper Belt would, it is true, increase the diversity of the planets. But it would also, in a straightforward sense, reduce the anomalousness of Pluto. The oddballs at the party seem less odd

---

[15] Brown (2010) clearly sees it this way.

[16] If a scientific *choice* is a conclusion underdetermined by evidence, then *every* novel conclusion is a matter of choice. I have no sympathy for this view, though of course many influential post-Modern critics of science seem to accept something like it.

the more of them there are. On the other side, there seems to be a certain temptation to regard the potential profusion of Pluto-sized KBOs as problematic.[17] This temptation too should be resisted. We might as well pine for only five elements or fewer beetles.

More reasonably, the norm of avoiding lonely categories would appear to be at work here. Much like the adopted child finding its biological parents, the discovery of the Kuiper Belt represents the opening of a new, well-populated family into which Pluto might be coherently fit. Marc Ereshefsky has claimed along these lines that "when we individuate an entity we individuate it against a background of other entities. In particular, whenever we ask if X is a part of an entity of a certain type, we need to check whether X might be a part of any other entities of that type" (Ereshefsky 2001, 39). If that background changes, perhaps the original classification should as well. But nothing about this norm or Ereshefsky's claim entails that all changes of the taxonomic background *compel* a foreground change. Perhaps they open the door to renegotiation — but that's *de rigueur.* Whether these negotiations result in revision depends on the details of the case.

Doubtless, we can again imagine some details which make change very compelling. Imagine discovering a large and varied collection of organisms that closely resembled the platypus and echidna especially in the respects that make them "anomalous mammals". It's not hard to imagine this (unprecedented) discovery leading to the budding of a novel class of organisms (the platypus, echidna, and their newly-discovered cousins) and to the corresponding contraction of *Mammalia*. But once again, nothing compels even this compelling revision. We could always simply group the newly-discovered monotremes in with the mammals, making *Mammalia* more diverse and the platypus and echidna less anomalous. Discovering objects that are very similar to already-classified objects need not lead to their reclassification. This is clearer when the already-classified objects are less peripheral. Imagine the (again unprecedented) discovery of some near relatives to the elephant. And suppose they resemble elephants in ways that previously made elephants unique among mammals. Presumably few would feel much pressure to exclude this reunited family from *Mammalia*.

So where does Pluto fall in this continuum? Even if the discovery of many Pluto-sized KBOs doesn't *compel* reclassification of Pluto, does it nevertheless "make sense" to group Pluto with the Kuiper Belt Objects? The International Astronomical Union clearly thought so, noting in Resolution 5 concerning the "Definition of a Planet in the Solar System" that:

> Contemporary observations are changing our understanding of planetary systems, and it is important that our nomenclature for objects reflect our current understanding. . . . The word "planet" originally described "wanderers" that were known only as moving lights in the sky. Recent discoveries lead us to create a new

[17] More and more KBOs are being discovered. Indeed, Brown's team (2004) has also identified an object (2003 VB12, nicknamed 'Sedna') from the Oort Cloud (long-postulated home to many long-period comets, believed to stretch halfway to the nearest star). Sedna has an orbit that puts it "well beyond" the Kuiper Belt. Many thousands more similar distant objects are postulated to exist in these regions — possibly, many as big as (or bigger than) Pluto.

> definition, which we can make using currently available scientific information. (IAU, 2006)

Pluto falls short of meeting condition (c) in the IAU's revised definition: that planets must have "cleared their orbit" (quoted at the outset). As Brown put it, "Pluto is disqualified because it is in the Kuiper belt but has not cleared out the Kuiper belt nor accumulated most of the mass in the asteroid belt [sic: presumably he means 'Kuiper belt' here], nor does it dominate the Kuiper belt. Pluto is part of a vast population and is rightly classified with that population where it belongs" (Brown).

We should take issue with Brown's reasoning as trading on an ambiguity of 'population' (and cognate terms) between classificatory and spatiotemporal readings. Identifying a group of objects in the latter sense (say, by pointing to them) clearly need not imply that they are all of the same kind. Pluto can be part of the Kuiper Belt in the sense of being located in a certain region of the solar system and yet be a member of a class of objects not all of which are located in that region. Condition (c) of the IAU's definition of 'planet' helps blur this distinction by excluding Pluto as a planet on the basis of its neighbors — but the distinction should be kept sharp.[18] The Kuiper Belt may not be taxonomically homogeneous; we have no reason to think that 'KBO' names a natural kind. Accordingly, discovering that Pluto is not the lone body at the icy outskirts of the solar system does not entail that it should be reclassified with the objects near it.

Nevertheless, Brown's rationale for reclassifying Pluto can be revised in light of the abovementioned ambiguity. Even if the Kuiper Belt is not taxonomically homogeneous, it might harbor a novel natural kind of object into which Pluto could be placed: small, round, icy objects crowded by each other (and other yet smaller objects). But even if this was so, we would still face a choice: should we incorporate the newly-discovered objects into the preexisting category or let them pull an object out of that category? And if our circumstances are properly and substantially characterized in terms of choice, it seems inappropriate to refer to Pluto's reclassification as a *discovery*. Insofar as 'discover' is a success term, describing classificatory revision this way in effect places it of the realm of rational discourse.

This point leads to a tension. What alternative picture of classificatory revision should we adopt? If many cases of classificatory revision cannot be described as discoveries, but rather as expressions of often unarticulated scientific norms, doesn't classificatory revision become a largely non-rational matter? For after all, norms are not *discovered*, they are adopted, often non-rationally, by means of persuasion. We have here an instance of the worry articulated by Kuhn's critics in the wake of *The Structure of Scientific Revolutions* that theory choice became "a matter for mob psychology" (Lakatos and Musgrave 1970, 178). Kuhn's response was that the "maxims, norms, or values" that guide — rather than determine — theory choice are in fact shared (Kuhn 1977, 322). That these values could be equivocal, differently emphasized, or come into conflict did not imply that they should be rejected (330). Still, insistence that the norms guiding theory choice are largely shared skirts the issue

of persuasion. What if we disagree about which norms to adopt in the first place? Would such disagreement foreclose on the possibility of rational, evidence-based discourse about classification? The answer, I will argue in the next section, is mixed.

4. Classificatory Norms

4.1. Avoidance of Lonely Categories

I hypothesized above that a classificatory norm bidding us to avoid lonely categories helps explain certain features of classificatory practice in the case of Pluto and the platypus. Recognizing the operation of such norms should disabuse us of the notion that we can straightforwardly discover that our taxonomies need intensional or extensional revision. How, then, do we avoid the specter of subjectivity, arbitrariness, or mob-rule in classificatory revision? The simple answer is that norms are not above justification and evaluation.

Consider the *Avoid Lonely Categories* norm: how might we go about justifying it as not merely descriptive of past classificatory behavior, but as prescriptive for future efforts? A complete story is likely to be long, but it might begin by highlighting the pragmatic dimension of taxonomies as information-bearers. Grouping non-duplicate items into a category only makes sense if that category has a reasonable number of members which appreciably resembled each other (in some sense). A taxonomy with as many categories as individuals fails at a central task of information conveyance.

This norm and its justification face a number of questions and worries. How many members should a category ideally possess? What could possibly justify a univocal answer to such a question *a priori*?[19] Moreover, don't we in fact accept many lonely categories? As the Dodo declined to a handful and then just one member, *Didu ineptus* did not cease to be an acceptable class of bird. Or consider the genus *Otocyon*, which contains just one species: *Otocyon megalotic* (the bat-eared fox). Such examples suggest that the *Avoid Lonely Categories* norm isn't even descriptive of our classificatory practice.[20]

There are a number of things we might say in response: In the first place, I don't suppose that there *is* a univocal answer to the question of how lonely a category has to be in order run afoul of the norm. This vagueness is characteristic of moral and epistemic norms too. When Kuhn writes that theories should be accurate, consistent, have broad scope, and be simple and fruitful (1977, 321–322), he rightly abstains from attempting to set specific tolerances for the satisfaction of these notions: "Individually the criteria are imprecise: individuals may legitimately differ about their application to concrete cases. In addition, when deployed together, they repeatedly prove to conflict with one another" (322). This latter remark points the way to a response to the worry about our two sorts of lonely categories (the last individual member of *Didus ineptus* and the single species taxon

[18] Indeed, as I shall suggest below, there are independent reasons for wanting to dispense with Condition (c).
[19] Thanks to an anonymous referee for *Mind* for raising a concern along these lines (the statement of it is my own).

within *Otocyon*). As with epistemic norms, classificatory norms often compete with one another. Avoidance of lonely categories represents but one attractor. That it is violated in a particular case does not show that it does not exist; others may have simply overridden it. And we can readily imagine candidates. In the case of *Otocyon*, we have a widely-respected (though debated) norm that biological classification should be historical (perhaps even that genuine groups should be monophyletic).[21] And as Stephen Jay Gould has pointed out, evolution may aptly be characterized as a process of repeated diversification and decimation (1989, 47). We know from fossils, for example, that the late Pliocene and early Pleistocene saw a cousin of *Otocyon megalotic*: *Otocyon recki* (Clark Jr. 2005, 1); that *Otocyon megalotic* persists alone in *Otocyon* reflects only its relative good luck among its evolutionary siblings.[22] A similar story applies to our lonely last dodo: being the last of a formerly populous group differs substantially from being literally "one of a kind" in a diachronic sense.

4.2. Extrinsic Parameters of Classification

My motivation and defense of *Avoid Lonely Categories* is necessarily brief and impressionistic; but I hope to have illustrated both its prescriptive plausibility and how its justification could be approached without immediately resorting to brow-beating or mob-incitements. Casting off (or the language of "discovery" and ringing the activity of classificatory norms into the open and exposing them to scrutiny is a way of making sure we are not merely talking past one another about classificatory revision.

We can see this reflected in some of the minority resistance to Pluto's reclassification. Consider the crucial condition (c) in the IAU's resolution. Many philosophers might seem puzzled that an object's *company* would hold decisive sway as to its kind. As Astronomer Alan Stern (principle investigator of the New Horizons mission currently en route to Pluto) puts it: "We do not classify objects in astronomy by what they are near, we classify them by their properties".[23] Stern of course has in mind *intrinsic* properties — and it seems a very plausible principle about classification: that extrinsic properties ought not bear on an object's kind. We should not classify perfect intrinsic duplicates differently.

What reason might we cite for accepting this norm of classification? Many will regard it as *obvious* and beyond question (or need of justification) — I don't know whether this is Stern's view. There does seem to be something basically credible about the thought that in discovering what kind of thing something is, one discovers something about that thing — *just that thing* — and not about its circumstances. Transgression of this norm seems to violate a basic aim of classification: conveying

---

[20] I owe this example and associated worry to a different anonymous referee.

[21] See, e.g., Velasco (2008) for a defense of this norm.

[22] Interesting complexities likely stem from the interaction of different norms. My own view, which I will not attempt to defend here, is that the *avoid lonely categories* norm is probably subordinate to the *biological species should be classified historically* norm (really a broad class of norms). Such interactions are guided by further *meta-norms*.

[23] quoted in *Nature* 31 August 2006, 965.

information about the objects so classified. An illustration: In times gone by, wealth rather than, say, personal character determined whether a man was a "gentleman". When told, for example, that Mr. Darcy is a gentleman, there is a clear sense in which we learn relatively little about *him*. Of course, a skeptic might reply that this simply begs the question. Mr. Darcy's property (his land) *is* among his properties (indeed, it might even be a causally-relevant property). Granted, it is not a quality of Mr. Darcy's independent of other things, but this is just to say, more or less, that it is not an intrinsic property of his. Moreover, the skeptic continues, does not astronomy already offer precedent for recognizing extrinsic classifications? After all, many moons are bigger and more geologically active and interesting than some planets (Jupiter's moon Ganymede is noticeably bigger than Mercury, for example). They are moons rather than planets owing only to their orbiting planets.[24]

Perhaps this classificatory norm reflects a more general stance about natural kinds: that they are to be individuated on the basis of their intrinsic, not extrinsic properties; that classification is, ideally, a sort of systematic recording of what natural kinds of things (broadly speaking) exist. That may do no more than repeat the norm of intrinsic classification in different terms.[25] The skeptic simply asks again: *why take an ontology of natural kinds to be defined in terms of only intrinsic properties?*

The most compelling reason I can think of involves the idea that our classifications ought to exhibit an appreciable degree of stability. As already mentioned, taxonomies are information bearers. To be told, for example, that a certain creature is a platypus is to be told a great deal about many of its interesting biological properties: that it is aquatic, possesses a poison spur, lays eggs, and so on (assuming it is a "normal" specimen). Inferring that the creature is in Australia, is presently in water, or is in the company of three to four other platypuses (be these properties ever so *typical* of the platypus) tends to strike us as inappropriately risky. Such extrinsic properties would never be a serious candidate for a kind-relevant property (for the *essence* of what it is to be a platypus, if you believe in Real Essences). Why? Well, there seems to be a difference between the intrinsic biological properties and extrinsic properties mentioned above: while the former are relatively stable, the latter are subject to easy manipulation.

This basic rationale requires revision if it's to be plausible. For one, the intuitive concept of stability here lacks even qualitative precision. Is it to be understood in a modal sense, a temporal sense, or what? For two, intrinsic properties can change and extrinsic properties can remain stable (in either sense). Organisms familiarly undergo radical change in their intrinsic properties during development. On the other hand, some extrinsic features remain stable: the phylogeny of a particular platypus (taken by many to constitute what it *is* to be a platypus) is not something that one can

[24] We could also cite biology as a domain in which extrinsic properties — position on the "tree of life" — are commonly taken as classificatorily relevant.

[25] I say 'may', because one might wish to distinguish an ontology of natural kinds from various systems of classification that might be set atop it. But that question exceeds the scope of this essay.

change willy-nilly by plucking it from its pond.[26] So the simple proposal that only intrinsic properties are stably possessed is not correct and thus cannot be used as a rationale for the purported classificatory norm that only intrinsic properties may be used for classification.

However, observe that we rarely choose *unstable* properties (be them intrinsic or extrinsic) as classificatorily relevant. If platypuses are individuated as a kind of animal by their phylogeny then we have an at least partially extrinsic classification, but one which exhibits a great deal of stability in the face of trifling actual and counterfactual perturbations. Of the many intrinsic properties that are unstable across a normal platypus life, the only plausible contenders for intrinsic individuators are those like genetic profile or developmental-stage-relativized phenetic properties which are likewise stable across a great many actual and counterfactual perturbations.

On the basis of these scant considerations I postulate the following (as yet underspecified) norm of classification responsible for the prima facie plausibility of the intrinsic classification norm above: *Whenever possible, objects are to be classified only on the basis of their stable properties*.[27] The norm is underspecified in that stability is undefined. I shall have to leave it this way here, as this is an extremely difficult question.[28] But I strongly doubt that we can (or should) attempt to find a sharp division between stable and unstable properties. Even when technical thermodynamical definitions are available, ordinary estimations of stability are often hedged by interest. Consider stars: they exhibit what Stéphanie Ruphy (2010) has called "taxonomic nomadism": they traverse a well-understood sequence of kinds. Over time, our sun will consume enough of its fuel to become a red giant star. But again, the transition is slow enough to satisfy the stability norm for many purposes.

Let us assume that something along the lines of the stability norm above is correct. Where does that leave us with respect to the purported taxonomic discovery about Pluto? What degree of taxonomic stability do the IAU's chosen parameters exhibit? Clearly, our focus should remain with (c). Several worries arise. With regard to temporal stability, we may note that the solar system has likely become much less populated over time. The planetesimals crowding the inner planets early in their formation have been ejected. Possibly, this clearing was a Cambridge change to these planets: they went from being non-planets — owing to their crowded orbits — to being planets once the crowd cleared.[29] Accordingly, if some interloper — a "rogue planet" — ever comes through and

---

[26] I have argued before that different kinds of enantiomers (mirror-image molecules) are also plausibly individuated on the basis of stable extrinsic properties (e.g., their "spatial embedding"); see my [REFERENCE SUPPRESSED].

[27] Basri and Brown signal tentative agreement with this norm when they note of scientific definitions: "One might hope that a definition not be time or history dependent (once a planet, always a planet), although there are proposed definitions for which that is not the case" (Basri and Brown 2006, 195).

[28] I believe the story will turn out to be similar to that Marc Lange (2000, 2005) has told in connection to his work on natural laws. I pursue the question of stability in connection with an account natural kinds in my [REFERENCE SUPPRESSED].

[29] This bumps up against a further worry about the "clearing criterion". The predicate '*x* has cleared its orbit' looks most amenable to a causal reading (as analogous to, say, '*x* has cleared his plate'). But in this case, it is a controversial empirical matter whether Earth (or any of the inner, rocky planets) deserve credit for "clearing their orbits", as Jupiter was probably a dominant influence in the formation of this part of the solar system (see Taylor 1998, 61–62).

crowds Earth's orbit, condition (c) dictates that Earth would cease to be a planet (again, without befalling any intrinsic change). Fortunately, the gravitational influence of the gas giants makes this rather unlikely (interlopers would likely be ejected). However, if the gas giants had never formed, it is conceivable that the inner solar system would have been less guarded and rather more crowded with planetesimals. As we do not yet know how common gas giants are in planetary systems, this counterfactual deserves prima facie assent. Analogously, we have excellent reason to believe that objects like Pluto were once *much* more common (like species within the *Otocyon* genus). Pluto and the other so-called "Plutinos" remain only because of their orbital resonance with Neptune, the others having been ejected from the solar system by close-encounters. Still, Pluto's orbit is crowded. But even if Pluto is for that reason *not* a planet given condition (c), it *could have been one* had its orbit and the Kuiper Belt formed in a slightly different way. It would thus seem that having a clear orbit is neither temporally or modally very stable. That qualifier ('very') keeps considerations like the foregoing from condemning condition (c) decisively, but we can still see how to sharpen the debate about Pluto's classificatory status.

4.3. Normative and Classificatory Pluralism

I am less concerned in the foregoing to establish the descriptive accuracy or prescriptive worth of the various classificatory norms I mentioned — or use them to push an interpretation of Pluto's status as a planet — than I am to clear conceptual room for the recognition, defense, and revision of classificatory norms. In this section, I would like consider the consequences of this stance for the question of classificatory pluralism. Let us approach this question in the widely-discussed context of biological systematics before broadening our view to classification more generally.

One standard way of motivating pluralism about classification in a certain domain begins by pointing out that, in fact, many distinct, cross-cutting ways for dividing up the objects in that domain coexist. Their coexistence may be comfortable or it may be contentious, but to the extent that the different systems each have some share of "legitimacy", pluralism can seem a reasonable response. Kitcher's classic argument for species pluralism takes roughly this form. He notes the paleontologist's plight in using Mayr's biological species concept (which emphasizes reproductive compatibility):

> There is a perfectly legitimate paleontological question which focuses on the rates and patterns of morphological diversification within evolving lineages, and paleontologists pursue this question by dividing lineages into species according to morphological changes. To insist that they should always formulate their inquiries by using the biological species concept is to make them take a risky trip around Robin Hood's barn. (1984, 317)

Monists may of course remain unmoved. Sure, it would be *nice* for our one and only species concept to be fully operational for all biological sub-disciplines, but surely it's not guaranteed! The

committed monist insists that there is a uniquely correct single classification system, whether or not we've found it.

Some worry, moreover, about a slide from moderate to radical pluralism; going pluralist might entail that "one can pick and chose [sic] among a variety of criteria, such as reproductive isolation, and similarities and differences in this, that, and the other. But we are not told how to make the criterion of membership be an objective one" (Ghiselin 1987, 136; see also Hull 1999, 36).[30] Without such a criterion, Ghiselin suggests, *any* respect of similarity can count, leading to a truly radical pluralism. Kitcher is unbothered, remarking that "to the best of [his] knowledge no pluralist believes that any [species concept] is as good as any other…. Pluralism about species no more contends that *any* set of organisms can be a species than pluralism about musical interpretation contends that any sequence of notes can be a performance of the B Minor Mass" (Kitcher 1987, 187).

Notice the apparent disconnect between Ghiselin's worry and Kitcher's response: Ghiselin is worried that there is no criterion for distinguishing between similarities that carry classificatory import and that this leaves open *the possibility* of any number of insane systems of classification. Kitcher responds that pluralists don't *in fact* recognize just any classification system. As a sociological fact, being a pluralist doesn't mean accepting that "anything goes". But just because there don't happen to be any radical pluralists doesn't mean that pluralism isn't committed to the *possibility* of radical pluralism and the legitimacy of inane or insane systems of classification.

I believe that recognizing the role of classificatory norms helps us understand what is at stake in this debate (and how to defend a sort of moderate pluralism). While Ghiselin is surely correct that objective similarity is cheap, he takes too simplistic a view of the role that recognizing such similarities play in informing our classification systems. Not any objective division defines a line of *legitimate* classification even if they do define *possible* lines of classification. Legitimacy is clearly a normative matter. We do not *merely* pick and choose among the plenum of parameters of objective similarity when classifying the world. Our choices are guided and constrained by our aims, conventions, and norms. On the other hand, nothing guarantees that such guidance and constraint will be perfect. This is what opens the door to moderate pluralism. The moderate pluralist contends not merely that different classification systems are *possible* — this is *obviously* true — but that among those different possibilities, a number of them are legitimate. Classificatory choice can sometimes operate within a limited space of legitimate possibilities.

The question of classificatory pluralism for a given domain, then, turns on the size of this space — the degree to which the world and our norms constrain our classificatory activities. On its face, this appears to be something like a factual question. And to some extent it is: it depends on the specific character of the relevant norms. But once again, it is misleading to talk of discovering what

[30] I understand the infelicitous turn of phrase "making the criterion of membership objective" as expressing the idea of *identifying* what membership criteria are objectively correct.

norms there are. We might be able to discover what norms in fact *guide* classifiers — what norms some people take themselves to be constrained by. However, this differs from discovering that those norms are *true*. While I cannot hope to argue for "non-cognitivism about classificatory norms" here, the view seems extremely natural. Assume that it is true. That is, assume that classificatory norms are not truth-apt. This doesn't mean that they are beyond evaluation. Rather, their evaluation only occurs "internally". We can cite *other* norms — *meta-norms*, if you like — in justifying or condemning some "ground-level" classificatory norms. Or we can critique a system of norms internally (say, on grounds of prescriptive consistency). But there is no final non-normative, factual appeal. Thus, one blunt way of motivating moderate pluralism starts from non-cognitivism about classificatory norms.

At first glance, the resulting moderation looks unstable. Consider a certain domain of classification. However constraining the norms guiding our classificatory activities in this domain are, the non-cognitivist can point out that many other possible and consistent systems of norms would guide us differently (even holding the empirical data fixed). While such systems may not be legitimized by *our* classificatory meta-norms, those meta-norms are no more stitched into the fabric of the universe than are the ground-level norms. Other meta-norms are possible. So we have a plurality of legitimate ground-level systems of norms as judged. But now if the legitimacy of a given norm (whether it governs behavior or the acceptance of other norms) depends on still further norms, a "legitimacy regress" threatens — and in its tow threatens a very radical form of pluralism (though not necessarily the *maximally* radical form imagined by Ghiselin).

I will not address the question here of *how* radical we might expect the resulting pluralism to be — partly because I'm not sure how to do so and partly because I don't think that such a pluralism is ultimately a serious threat. When it comes to legitimacy, "proximity to the ground" matters. I suspect this is what Kitcher had in mind when he noted that there aren't *in fact* any radical pluralists. Such characters are *possible*, as are biologists advocating what would seem to us outré systems of classification (as, e.g., some of Aristotle's schemes now look). But it would be perverse to call them legitimate — even though there may be *some* possible meta-norms that would sanction their legitimacy. In describing them as such, we mean to employ *our* standards of legitimacy.

It seems to me likely that a realistic and interesting moderate pluralism can result from normative freedom at either the ground- or meta-levels. Suppose for simplicity that an overarching meta-norm of classification is that we should maximize informativeness and compactness — perhaps along analogous lines to Lewis's (1973) Best System Analysis of laws. Since those goals pull in opposite directions, it's reasonable to expect that this meta-norm would sanction the legitimacy of various systems of ground-level norms.[31] Even a single system of ground-level norms could leave room for a plurality of classificatory systems.

[31] As we dial back the simplicity of the above exemplar meta-norm (or add more meta-norms), the plausibility of multiple legitimate systems of ground-level norms goes up.

Consider, for example, some of the questions that advocates of Boyd's Homeostatic Property Cluster (HPC) account of kinds face: What properties are legitimately included in an HPC cluster? What constitutes a sufficiently rich "cluster" of properties? How stable must such clusters be in order to count as HPC kinds? How many properties in the cluster can go missing in particular instances of a kind? Here is Rob Wilson's approach to some of these questions:

> First, what counts as having "enough" of the relevant properties, as with what are the relevant properties in the first place, is an *a posteriori* matter determined in particular cases by those practicing the relevant science, rather than by philosophers with a penchant for crisp universality. There need be no one answer to the question of what is "enough" here, but whatever answers are given in particular cases will be responsive to the clusters of properties that one finds in the world. (Wilson 2005, 113)

As in LaPorte's precisification cases, neither the world nor our norms decides how we out to refine our vague categories. They nevertheless guide and constrain how we approach classification subject to our broad aims in classifying things in the first place.

Though standardly bidden to recognize pluralism only when faced with an actual plurality of actively used classification systems, pluralism concerning classificatory legitimacy can lurk behind even pristine classificatory univocality. An interesting consequence of the present stance is that matter-of-fact agreement about how to classify the world cannot always be wielded as a club. Even when it comes to revisions to a classification system that face no opposition or controversy, we cannot always rightly say that we've *discovered* that we ought to revise things in such and such a way.

5. Conclusion

So far I have been doubly critical of the claims that we have discovered that Pluto is not a planet. First, and most importantly, I believe that the straightforward language of discovery fails to do justice to the role that classificatory norms play in the construction and subsequent revision of taxonomies. Alternatively, we might say that many proponents of revision overstate the universality and justificatory status of the relevant norms to the point where it seems expedient to ignore them. In such a case, talk of the discoveries concerning the Kuiper belt and so on transitions into talk of the *discovery* that classificatory revision is appropriate — with arbitrariness as the supposed alternative. Speeches like the following are offered to justify this picture:

> We can draw a reasonable parallel between Pluto and [the large asteroid] Ceres. If we do not consider Ceres a planet but call it the largest asteroid, why do we call Pluto a planet rather than the largest Kuiper Belt object? Can Pluto be both? If it can, should Ceres be both also? Does the solar system really have ten planets, counting Ceres? Making such a decision should not be an arbitrary activity. *Is Pluto a planet?* is a scientific question, not a matter of public opinion or a decision to be made by NASA or a panel of distinguished astronomers. Science moves forward at a pace dictated by

> progress in understanding, not by fiat or a majority vote of a committee. (Weintaub 2007, 184)

I hope to have made it plausible that this represents a false dichotomy — or, to put it in more positive terms, that something is a "scientific question" need not entail that nature dictates a univocal answer. By denying that we have discovered that Pluto is not a planet we do not thereby assert that whether it is a planet is a purely arbitrary matter. What, then, *should* we say about Pluto?

This brings me to my second critical point about the Pluto case. If my claims about the the norm-governedness of classification are on the right track and my (more tentative) postulated norms are reasonable, we can see how the specific decision of the IAU can be criticized as offending from a plausible norm of classification. This criticism must, of course, be tempered by the pluralism I am committed to. But if members of the IAU do in fact share — in explicit acceptance or implicit expression by their actions — the relevant classificatory norms and meta-norms, their decision can be criticized on the basis of its consistency.

We unquestionably now know more about the structure and origin of the solar system than we did even a decade ago. And it is an exciting prospect of learning much more when *New Horizons* gets close to Pluto-Charon and the Kuiper Belt.[32] But there is much we do not understand and may never uncover. Perhaps the mechanics of solar system formation determine that objects like Earth will resist orbital crowding and objects like Pluto will resist orbital clearing. Perhaps the relevant changes in these properties occur so slowly as to fall under the stability norm's aegis. However this may be, whether the IAU's criterion (c) falls within accepted tolerances for stability is a matter of *acceptance*, not discovery — both of whether the stability norm is accepted and how precisely to interpret it. At best, we can determine how we have classified in the past (in this domain or in others) and ask what, if anything, this might imply about the tolerances of stability.

If it is *not* an arbitrary matter how we should treat Pluto, then what should guide us? I hereby posit a final norm of classification: *In circumstances like these — i.e., a classification dispute spurred by a relative poverty of potential parameters of classification clearly satisfying other norms of classification —, extend high-level categories and multiply lower-level ones.* The category 'planet' already features quite a bit of internal discontinuity. As Taylor writes,

> The most striking difference in the solar system is the distinction between the giant planets and the small rocky inner planets. There is really a threefold division, because Uranus and Neptune are ice giants rather than gas giants like Jupiter and Saturn. The planets themselves thus mirror the three main constituents of the solar nebula, gas, ice, and rock. In addition to these differences in composition between the three groups of planets, there is also the striking difference in mass between them. (Taylor 1998, 52)

[32] For more information on this NASA project, see http://pluto.jhuapl.edu/.

History suggests that the more we learn more about a certain domain, the finer the classificatory distinctions we make. When we consider the great heterogeneity among even our small sample of known planets, it hardly seems inappropriate to add a subcollection containing the untold icy-dwarfs whose origin is the Kuiper Belt.

Indeed, in multiplying lower-level properties we might make distinctions in our distinctions, locating cross-cutting categories governed by different classificatory norms. *Planet* and *KBO* need not be disjoint categories. Nor need either be regarded as univocal. We already encountered a tension between two readings of 'KBO': between a *location*-specifier and *quality*-specifier (we could add *origin*-specifier). In cases of ambiguity, the best move is often to lay bare the options and refuse to choose in perpetuity. Allowing low-level categories to overlap reduces the need for divisive, distracting, and pointless debates. We avoid apparently awkward conundrums of intrinsically similar objects being forced into different categories, as when Weintaub notes "[Pluto's moon] Charon . . . is essentially indistinguishable from [the Plutino] Ixion. However, we label one as a Kuiper Belt object while the other is designated a moon of Pluto" (2007, 167). Nor need *Planet* and *Moon* — a category which quite clearly offends against the stability norm[33] — remain disjoint. Some planets are moons (owing to dynamic properties they happen to have), some are not. Perhaps some are KBOs, owing to their origin/location.[34]

Perhaps many different kinds of planets await discovery. Stern nicely sums things up this way in the postscript to his and Mitton's book:

> As this revised edition of our book was in press, we happily learned of the discovery of a remarkable body, provisionally called 2003 UB_{313} [which] orbits beyond Pluto and is a bit larger than Pluto itself. On average, its distance from the Sun is about three times Pluto's. This body confirms predictions . . . that many more planets, most of them ice dwarfs, would be found in the deep outer solar system. We suspect that, in the coming decade, many more ice dwarf planets will be discovered, along with planets of much greater size. A revolution is indeed upon us as we come to realize that our solar system has many, many more planets than the nine we have known about since our youth. Once again, Pluto has proved to be a pivotal body for understanding the content and origin of our solar system. (2006, 176)

---

[33] Whether a body orbits a planet or the sun is probably a modally very fragile matter. Neptune's moon Triton, orbiting in retrograde, was probably a body very like Pluto which was captured by some happenstance (Taylor 1998, 102). In fact, the standard criterion of moonhood — whether something orbits a planet — is somewhat more subtle than might appear, as moons do nor orbit the center of planets, but rather a point between their centers called the "barycenter". In the case of the earth-moon system, this point "lies slightly closer to the center of the Earth than the surface of the earth. . . . With the Pluto-Charon system, the barycenter is actually located in between the two objects. If we watched Pluto and Charon carefully, we would see both objects orbiting around an empty spot in space; that spot is closer to Pluto than to Charon but is well above the surface of Pluto. This criterion, that the center of the orbits of both the bigger and smaller objects in a double-object system lies somewhere in between the two objects, is a good reason to label Pluto-Charon a double-planet system rather than a planet-moon system" (Weintaub 2007, 176).

[34] Weintaub (2007, 220) closes in on something like this conclusion in answer to his title question, though he presents it in a rather less circumspect way.

So too for understanding our classificatory practice more generally.

REFERENCES

Basri, Gibor, and Michael E. Brown (2006) "Planetesimals to Brown Dwarfs: What is a Planet?", *The Annual Review of Earth and Planetary Science* 34:193-216.

Boyd, Richard (1999) "Homeostasis, Species, and Higher Taxa", in Robert A. Wilson (ed.), *Species: New Interdisciplinary Essays*. Cambridge: MIT Press.

Brown, M. E., C. A. Trujillo, and D. L. Rabinowitz (2005) "Discovery of a planetary-sized object in the scattered Kuiper belt", *Astrophysics Journal Letters*.

Brown, Michael E. (2008), *The Eight Planets* 2008 [cited 6 June 2008]. Available from http://www.gps.caltech.edu/~mbrown/eightplanets/.

——— (2010) *How I Killed Pluto and Why It Had It Coming*. New York: Speigel & Grau.

Brown, Michael E., Chadwick Trujillo, and David Rabinowitz (2004) "Discovery of a Candidate Inner Oort Cloud Planetoid", *The Astrophysical Journal* 617:645–649.

Clark Jr., Howard O. (2005) "*Otocyon megalotis*", *Mammallian Species* (766):1–5.

Ereshefsky, Marc (2001) *The Poverty of the Linnaean Hierarchy*. Cambridge: Cambridge University Press.

Ghiselin, Michael (1987) "Species Concepts, Individuality, and Objectivity", *Biology and Philosophy* 2 (2):127–143.

Gould, Stephen Jay (1989) *Wonderful Life*. New York: W. W. Norton & Company.

Grant, Tom, and Dominic Fanning (1995) *The Platypus: A Unique Mammal*. Sydney: UNSW Press.

Hoyt, William Graves (1980) *Planets X and Pluto*. Tucson: University of Arizona Press.

Hull, David (1999) "On the Plurality of Species: Questioning the Party Line", in Robert A. Wilson (ed.), *Species: New Interdisciplinary Essays*. Cambridge: MIT Press, 23–48.

Kitcher, Philip (1984) "Species", *Philosophy of Science* 51:308-333.

——— (1987) "Ghostly Whispers: Mayr, Ghiselin, and the "Philosophers" on the Ontological Status of Species", *Biology and Philosophy* 2 (2):184–192.

.

Kuhn, Thomas (1990) "Dubbing and Redubbing: The Vulnerability of Rigid Designation", in C. Wade Savage (ed.), *Minnesota Studies in the Philosophy of Science*. Minneapolis: University of Minnesota Press, 298–318.

Lakatos, Imre, and Alan Musgrave, eds. (1970) *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London, 1965*. Cambrdige: Cambridge University Press.

Lange, Marc (2000) *Natural Laws in Scientific Practice*. New York: Oxford University Press.

——— (2005) "Laws and Their Stability", *Synthese* 144 (3):415–432.

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

Lewis, David (1973) *Counterfactuals*. Cambridge: Harvard University Press.

Moyal, Ann (2001) *Platypus*. Sydney: Allen & Unwin.

Ruphy, Stéphanie (2010) "Are Stellar Kinds Natural Kinds? A Challenging Newcomer in the Monism/Pluralism and Realism/Antirealism Debate", *Philosophy of Science* 77 (5):1109–1120.

Slipher, V. M. (1930) *Lowell Observatory Obervation Cicular*. May 1, 1930.
Stern, Alan A., and Jacqueline Mitton (2006) *Pluto and Charon: Ice Worlds on the Ragged Edge of the Solar System*. 2nd ed. New York: John Wiley & Sons, Inc.
Taylor, Stuart Ross (1998) *Destiny or Chance*. Cambridge: Cambridge University Press.
Velasco, Joel (2008) "Species Concepts Should Not Conflict With Evolutionary History, But Often Do", *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 39 (4):407–414.
Weintaub, David (2007) *Is Pluto a Planet?* Princeton: Princeton University Press.
Wilson, Robert A. (2005) *Genes and the Agents of Life*. Cambridge: Cambridge University Press.
Wilson, Robert A., Matthew J. Barker, and Ingo Brigandt (2007) "When Traditional Essentialism Fails: Biological Natural Kinds", *Philosophical Topics* 35 (1/2):189–215.